

TRACTS

Consisting of

OBSERVATIONS

About the

SALTNESS of the SEA:

An Account of a

STATICAL HYGROSCOPE RARE BOOK

And its USES:

Together with an APPENDIX 155
about the B68

FORCE of the AIR'S MOISTURE: 1674

A FRAGMENT about the
NATURAL and PRETERNATURAL
STATE of BODIES.

By the Honourable *ROBERT BOYLE*.

To all which is premis'd

A SCEPTICAL DIALOGUE

About the POSITIVE or PRIVATIVE
NATURE of COLD:

With some Experiments of Mr. *BOYL'S* referr'd
to in that Discourse.

By a Member of the *ROYAL SOCIETY*.

London, Printed by *E. Fleisher* for *R. Davis* Bookseller
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1674

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National Oceanic and Atmospheric Administration

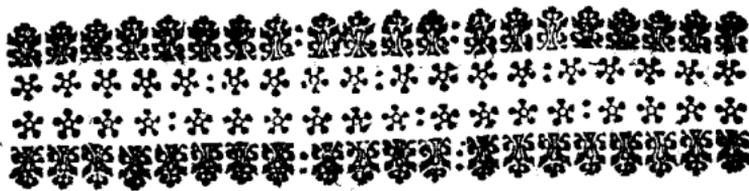
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AN
ADVERTISEMENT
OF THE
PUBLISHER.

THE Reader of the following Dialogue may easily conclude from the beginning of it, (where the occasion of the Conference is set down) that

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An Advertisement

if the Author had been so minded, it might have long since come abroad. But though, as his backwardness to publish it kept it long lying by, first in His hands, and then in Mine; yet the Affinity it has with some of the ensuing Tracts of *Mr. Boyle*, and some other of his Papers, that he design'd (but was hinder'd) to have added to them, engag'd me to take the liber-

liber-

of the Publisher.

liberty of publishing it and them all together : Which I had sooner done than now I do, if, by some Accidents, they had not been kept from appearing for many weeks after they were quite printed off.

(1)



OF THE
P O S I T I V E
OR
P R I V A T I V E N A T U R E
O F
C O L D .

A Sceptical Dialogue

Between

*Carneades, Themistius, Eleutherius,
Philoponus.*

SECT. I.

Elen.



May one be allowed to ask *Carneades*, what Book it is he is reading with so much attention?
Carn. The Question, *Eleutherius*, is very allowable, and as easily answer'd, by saying, that what I was reading, is our Friend Mr. *Boyle's* newly published *History of Cold.*

B

The.

Themistius. Your readiness, *Carneades*, to answer, encourages me also to ask you a Question; which shall not be, as probably you expect it should, How you like this new Piece? for I know you would be too kind to the Author, not to tell me that he has detected some Old Errours, and made discovery of some New Truths; but my Question shall be about what is my Wonder, as well as that of divers others, who think it strange that a Writer that has deliver'd so many Effects and other *Phaenomena* of Cold, should omit to tell us so much as whether he asserts it to be a *Positive Quality*, or a bare *Privation* of Heat; as, since *Cardan* (in his Treatise *De Subtilitate*) some other Learned Men, and especially *Cartesius*, hath maintain'd.

Carneades. You will not wonder, if a Person that you look upon, and I confess not injuriously, as a Friend to Mr. *Boyle*, tell you, that this Author, by the many Histories he has presented us, and by his not seeming to dare to determine the Controversie you have mention'd, shews, that he was more sollicitous to lessen his ignorance, than to pretend to knowledge: And upon the observation I have made of his humour in general, I presume one principal reason of his silence may be, that he has not yet compleated the trials he had design'd about Cold; and thinks, that in Abstruse Subjects, such as this is, 'tis not so convenient to deliver a positive opinion of the Nature of it at the Beginning, as to reserve it for the latter End, after the History of the *Phaenomena*; when the nature of the thing inquir'd into may, as it were, spontaneously Result from the Considerations suggested by the precedent matters of Fact survey'd together.

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Eleutherius. If such a wariness were indeed the motive of your friends silence, I shall easily excuse it; and perhaps think too, that the like would not mis-become Naturalists on many other occasions. And yet I do not dislike *Themistius's* question; for 'tis one thing to venture upon declaring the adæquate Nature of Cold, and another to determine, Whether it be a Positive, or a Privative Quality? The latter attempt importing* a much less venture than the former.

Carneades. I will not pretend to know the very Reasons that induc'd the Author silently to pass by this Controversie; but having been once present, when he had occasion to discourse of it, I then conjectur'd, that among his Experiments of Cold, that are not yet publish'd, there may be some uncommon ones, that may have suggested to him scruples, which oblig'd him to forbear declaring himself, till he had clear'd them, which those that are unacquainted with such Tryals, may probably have never thought of.

Themistius. If what you call a Controversie, were indeed worthy of that name, I should not unwillingly allow of your Friends silence; but the Opinion broach'd by *Cardan*, and adopted by Mr. *Des Cartes* and others, seems to me so devoid, not only of reason, but of all appearance of it, that me thinks one that has deliver'd such considerable Effects of Cold, as Mr. *Boyle* has done, may well ascribe to their cause, at least, a Positive Nature; and, without at all being guilty of boldness, reject an Opinion, that is not only barely an Errour, but an Extravagance; and perhaps a plain Absurdity.

Of the Positive, or

Carneades. Possibly the Gentleman we are speaking of, may be wary and sceptical enough to reckon among difficult things, not onely the declaring the adequate Nature of Cold, and the manner of its Operations; but the demonstrating whether it be a *Positive* Quality or not. And though I will not take upon me to know his thoughts about that subject, which, perhaps; are grounded upon some of his peculiar Experiments and Notions; yet, for discourse sake, I am content to debate with *Themistius*, Whether or no the Opinion he so severely censures, be not only erroneous, as, for ought appears, Mr. *Boyle* himself may be found to have thought it; but also, as *Themistius* would have it, absurd.

Themistius. I readily accept of your offer; for it cannot be an unpleasant entertainment to observe the arts whereby one that I know will not speak impertinently, will endeavour to make Reason elude the clearest Testimonies of Sense. And though I might press you with the concurrent authority of *Aristotle*, and all the Philosophers that have liv'd between his time, and those of that extravagant Fellow *Cardan*; yet I shall rather employ, to convince you, the authority and reasons of a grand Leader among your New Philosophers, who being a great broacher of Paradoxes, and having upon that score written Books expressly against *Aristotle*, was not like to have sided with him, unless the Evidence of Truth had, as it were, necessitated him to do so.

Carneades. I presume, you mean the Learn'd and Subtle *Gassendus*, whom I am glad you have pitch'd upon for your Causes Champion, not only because
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in defending the common opinion, he waves the common practice of troubling his Readers with a multitude of Authorities, which to me, in such a case as this, would signifie very little, and betakes himself to arguments; but because, being so modern and judicious a Writer, we may well suppose him to have summ'd up and improv'd what can be said in behalf of the cause he maintains. Upon which account, I shall be excus'd from answering impertinent Objections against the Opinion I defend, and from the trouble of ranging about among other Authors for more weighty Arguments than those, which the disproving of his will shew to be unsatisfactory.

Themistius. I am glad you nam'd the Author I meant, *Carneades*; for I apprehended you had not met with what he sayes upon this subject: because I could scarce imagine, that an intelligent person, after having read his arguments, will doubt of a Truth he hath so clearly evinc'd by them. But since I perceive you have seen what he has written, I shall, without farther preamble, propose his Reasons to you, though not in the very same order wherein he has couch'd them.

Eleutherius. But before you begin them, give me leave to ask *Carneades* a short question, whose answer will, I suppose, conduce, if not be necessary, to the clearing of the state of the Controversie betwixt you. For 'tis one thing to deny belief to the receiv'd Opinion, that Cold is a Positive Quality, and another thing to assert, that 'tis but a Privation of Heat; since, if *Carneades* does undertake the latter of these two, he must bring positive Arguments to prove Cold to be but a negative thing.

Whereas, if he content himself to play a doubting part, it may suffice him, being in effect but a Defendant, to shew that the proofs brought to conclude Cold to be a Positive Quality, are not Cogent.

C Carneades. I acknowledge your Question, *Elen-therius*, to be pertinent, and not unseasonable. And I presume, you will not be surpriz'd, that a Person accus'd of Scepticism answers it by declaring, that he undertakes not to demonstrate, that Cold must be a Privative or Negative Quality, and thinks it sufficient for his turn, to shew that the Arguments brought to evince it to be a Positive one, are not concluding. And, since you have already diverted *Themistius* from beginning so soon as he intended, 'twill not be amiss, that I continue that suspension a little longer, to prevent, what I know we both hate, Verbal Controversies; which yet may very easily spring from undetermin'd acceptions of Words as ambiguous as I have observ'd Heat (of which I now make Cold but a Privation) to be.

We may therefore consider, that the word *Heat*, being made use of to signifie, as well the operations of that quality upon other Bodies (as when the Heat of the Fire makes Water boyl, or that of the Sun melts Wax; and hardens Clay) as its operations upon the Sense of man, (as when a moderate degree of Heat is said to cause pleasure, and an excessive one to produce pain;) this Term, I say, as *Mr. Boyle* also has somewhere noted, may be employ'd sometimes in a more absolute and indefinite sense, and sometimes in a more confin'd and respective sense; In the latter of which, 'tis estimat-

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ted by its Relation to the Organs of Feeling of those men that judge of it. Upon which account, men are wont to esteem no body Hot, but such an one, the agitation of whose small parts is brisk enough to increase or surpass that of the particles of the Organ that touches it: For if that motion be more Languid in the Object than in the Sentient, the Body is reputed Cold; as may appear by this, that if the same Person put one of his hands when 'tis hot, and the other when 'tis cold, into luke-warm Water, that Liquor will feel cold to the warm hand, and warm to the cold.

Eleutherius. So that according to this Doctrine, methinks, one may, for brevities sake, conveniently enough apply to your two-fold Notion of Heat, those expressions which some School-men employ about certain Qualities, of any of which they say, that it may be either materially or formally consider'd. And by Analogy to their Doctrine, since *Heat* is a Tactile Quality, and as such, imports primarily a relation to the Organ of Touching, that relation, with what depends upon it, may pass for that which is the *Formale* in the Quality called Heat; and its Effects and Operations upon other Bodies may supply us with a Notion of Heat, materially taken.

Carneades. I do not always quarrel, *Eleutherius*, with Terms borrow'd from the Schools, if they be as much more short and expressive than others, as they are more unusual, or even barbarous. But there is another Distinction of *Heat*, partly grounded upon that already propos'd, which, because it may be of use in our future Discourse, will not be unfit to be here intimated. For we may consider,

that though, for the most part, a hot Body is taken in the vulgar sense for that wherein the degree of Heat is sensible to our Organs of Feeling; yet in a looser sense, and which, for Distinctions sake, we may call Philosophical, because concluded by Reason, though not perceiv'd by sense, a Body may be conceiv'd not to be destitute of Heat, even when the degree of that Quality is not great enough to be felt by the Touch; provided it can produce in some degree those other Operations, which, when more intense, are acknowledged to proceed from manifest Heat. For elucidation of which, we may alledge, That in very frosty, and yet clear, Weather, the Sun may be judg'd to warm the Air, when it melts Snow, and thaws Ice; though, perhaps, many men, especially of tender Constitutions, feel in their Fingers and Toes much stiffness and more pain, upon the account of Cold. To this I may add the common Observation, if you grant the truth of it, that Snow melts much sooner upon Land newly turn'd up by the Plow, than, *ceteris paribus*, in the neighbouring ground; which argues a warmth in that newly expos'd earth: though according to the Touch it would questionless appear Cold. But we may be furnish'd with a clearer and more pregnant Instance, by but recalling to mind what was just now mention'd of the warmth of tepid water, which was not to be felt by a hot hand, but produc'd there a contrary sensation of Cold. Which Instance I therefore scruple not to repeat, because it affords an Experiment in favour of that premis'd Distinction, which, I think, may also have this ground in Reason, that a considerable Heat is often requisite to be sensible to our hands,

hands, &c. which are continually irrigated with the Circulating Blood that comes very warm out of the Heart, and enliven'd by Animal Spirits, plentifully supply'd from the Brain.

If *Eleutherius* think fit to accommodate this Distinction in the Vulgar and in the Philosophical sense to his *Heat*, formally and materially taken, I leave him to his liberty. And I shall also leave it to you both, Gentlemen, to accommodate to Cold, *mutatis mutandis*, as they speak, what has been said about the distinctions of Heat; because, I fear, *Themistius* thinks himself to have been too long detain'd already from proposing his Arguments, which he may now begin to do as soon as he pleases.

S E C T. II.

Them. I Will then, with your permission, begin with that Argument of *Gassendus*, which I am able to give you in his own words; because upon the occasion of Mr. *Boyle's* book, I made a Transcript of what he says to evince the *Positive Nature of Cold*; and having the Transcript yet about me, 'tis ealie for me to tell you, that 'tis this: *It sunt frigoris effectus quales habere privatio, quae actionis est incapax, non potest.*

Vid. *Gassend. Physicam. Sect. 1. Lib. 6. Cap. 6.*

This Argument, though he begins not with it, I choose to make the first, because I think it of such weight, that, though it were the only one he could alledge, it would serve his turn and mine, 'since 'tis drawn from the Effects of Cold, which, though he

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mentions them but in few and general words, experience shews to be both so manifold and so considerable, that if *Carneades* employ an hundred times as much time to answer the Argument they afford, as I have done to recite it, he will, I think, do no more than would be necessary, and perhaps not enough to be sufficient. For, *Cold* affects the Organs of Feeling, and sometimes causes great pain in them, condenses Air and Water, and breaks Bottles that are too well stopt, congregates both Homogeneous and Heterogeneous things, increases Hunger, checks fermentation in Liquors, produces Heat by *Antiperistasis*, in deep Cellars, Mines, &c. and yet freezes Men and Beasts to Death, dismantles whole Woods and Forrests of their Leaves, and does (I know not how many) other Feats; among which, it is not the least admirable, though one of the most common, that it turns the fluid and yielding Waters of Rivers and Lakes, and sometimes of part of the Sea it self, not too far from the shoar, into firm and solid Ice, which is often in Northern Climates strong enough, not only to be travell'd upon by Merchants with their Carriages, but to be fought upon by whole Armies with their trains of Artillery. From which, and other Instances, it is manifest, that Effects so numerous and great, cannot proceed from a meer privation, or any negative thing, but require a considerable, and therefore sure a Positive, Quality to produce them.

Carneades. This Objection, *Themistius*, is, I confess, a considerable one, and of more weight than any of the rest, if not than all of them put together: But, as I think it very worthy to be answer'd,

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swer'd, so I think it very possible to be well answer'd; and to give you my reasons for my so thinking, I shall distinctly consider in the Argument the two particulars which it seems to consist of.

And first we are told, that if Cold be but a Privation, it cannot be the object of sense. To clear this difficulty, which, I know, you will think it very hard, if at all possible to do, I must beg your leave to observe something about Sensation in general; not as designing an entire and solemn Discourse of that Subject, but because the particular remark I am about to make, is necessary to the Solution of our present Difficulty. I observe then, that That, which, at least in such cases as we are speaking of, produces in the mind those perceptions, which we call Sensations of outward Objects, is the Local Motion, caus'd by means of their Action upon the Outward Organs in some internal part of the Brain, to which the Nerves belonging to those Organs correspond; and the diversity of Sensations may be refer'd to the differing modifications of those internal motions of the Brain, either according to their greater or lesser Celerity, or other Circumstances, as our Friend Mr. Boyle has somewhere exemplify'd in the variety of Sounds; whereof some are grave, some sharp, some harmonious and pleasant, some jarring and offensive; and yet all this strange variety proceeds from the variations of those strokes or impulses, which the Air, put into motion by sonorous Bodies, gives to the ear.

To this it will be consonant, that as the Air, or rather the mind by the intervention of the Air, is differently affected by a very grave sound, and a very acute one; though the former proceed from the

want of that Celerity of motion in the undulating Air, which is to be found in the latter; which slowness or imminution of motion, does, as such, participate of, or approach to, the nature of Rest: so in the sensory of Feeling, there may, upon the Contact of a Cold Body, be produc'd a very differing perception from that which is caus'd by the contact of a Hot Body; and this, though the thing perceiv'd, and by us call'd Coldness, consists but in a lesser agitation of the parts of the cold Body, than of those of the hot Body, in respect of our hands or other Organs of Feeling.

And this leads me, for the farther clearing of this matter, to represent to you, that since 'tis manifest, that Bodies in motion are wont to communicate of their motion to those more slow Bodies they happen to act upon, and to lose of their own motion by this communicating of it: Since this, I say, is so, if, for Instance, a man take a piece of Ice in his hand, the agitation of the particles of the Sensory will, in good part, be communicated to the Corpuscles of the Ice, which, upon that account, will quickly begin to thaw; and the contiguous parts of the Hand losing of the motion they thus part with to the Ice, there needs nothing else to lessen the agitation they had before. And there needs no more than this slackning or Decrement of Agitation, to occasion in the mind such a new and differing perception, as men have tacitly agreed to refer to Coldness.

Eleutherius. It seems by this Discourse, *Carnades*, that you think, that Sensation is properly and ultimately made in, or by, the Mind, or discerning Faculty; which from the differing motions of the inter-

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internal parts of the Brain is excited and determin'd to differing perceptions; to some of which Men have given the names of Heat, Cold, or other Qualities. So that, according to you, if a considerable Change or Variation be made in the most ordinary, or in the former motion or modification of motion of the parts of a Sensory, and consequently of the parts that answer them in the Brain, new Sensations will be produc'd, whatever the cause of this Alteration be, whether Privative or Positive.

Carneades. You do not mis-apprehend my thoughts, *Eleutherius*, and what you say gives me a rise to illustrate this matter yet a little farther by observing, that the Sensories may be so accustomed to be affected after a certain manner by those external Objects, whose Operation on them is very familiar, or perhaps almost constant, that the Privation, or the bare Imminution of the wonted operation leaves the parts of the Sensory, for want of it, in a different disposition from what they formerly were in; which change in the sensory, if it be not too small, will be attended by a perception of it in the mind. To declare and confirm this by an example, we may consider, that though Darkness be confessedly a Privation of Light, and the Degrees of it, gradual Imminutions of Light; yet the Eye, that is, the Perceptive Faculty by the Intervention of the Eye may well enough be said to perceive both Light and Darkness, that is, both a Positive thing, and the Privation of it. And 'tis obvious, that the motion of a shadow, which is a gradual Privation of Light, is plainly, and without difficulty, discoverable by the Eye; of which
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14 *Of the Positive, or*

the reason may be easily deduc'd from what I have been lately saying. And to shew you that there is on these occasions such a change made in the Organs of Seeing, as is visible even to By-standers, I shall need but to appeal to the Experiment of making in the day time a Boy or Girl look towards an enlighten'd Window, and then towards an obscure part of the Room; for when the latter comes to be done, you will plainly perceive, that for want of such a degree of Light as was wont to come in at the Pupill, and straiten a little that perforation of the Uvea; that round Circular Hole, or, as you know they call it, Apple of the Eye, will grow very manifestly larger than it was before and than it will appear again, if the Eye be expos'd to a less shaded Light.

... This observation may be seconded, by what happens to a man, when coming out of the Sun-shine, where the Sun-beams much contract his Pupill to shut out an excessive Light that would be offensive to the Organ, he comes presently into a dark room, where he must continue some time before he can see others as well as he is seen by them, whose Pupills have had time to be so enlarged, as in that darker place to let in light enough to make Objects visible to their Eyes, which are not so to his, whose Pupills are yet contracted by the Light they were but just before expos'd to. To this I might add divers other *Phænomena*, explicable upon the same grounds; but I shall rather chuse to relate to you an uncommon Accident, which happening to eyes somewhat unusually disposed, do's more remarkably discover, what alteration Darkness, or a privation of Light, may have upon those Organs. I know

a very Learned man, who is no less studious of Mathematicks, and other real parts of Knowledge, than skill'd in those which are taught of the Schools: This *Virtuoso*, who seem'd to me to have something peculiar in his eyes, confess'd and complain'd to me, that if he come, though but out of a moderate light of the open air, into a room that is any thing dark, he does not only feel such an alteration as other men are wont to do on the like occasion; but is so powerfully affected by it, that he thinks, he sees flashes of fire before his Eyes, and feels a troublesome discomposure in those parts, that sometimes lasts an hour or two together, if he so long continue there.

Eleutherius. I know not, *Carneades*, whether after this, you will think it any great confirmation of your Opinion, that *Aristotle* has somewhere this saying, that, *Oculus cognoscit Lucem & Tenebras.*

Carneades. I thank you, *Eleutherius*, for so pertinent an Allegation, though not for my own sake, yet for theirs, that will more easily receive a Truth upon the Testimony of *Aristotle*, than that of *Nature.* And now, I hope, that *Themistius* will consent, that dismissing the Argument hitherto examin'd, we proceed to the next.

S E C T. III.

Them. **S**ince you will have it so, I shall comply at present, and the rather, because not only I foresee there will be occasion to speak of

of it again, but because you Experimental Philosophers, that are wont so much to cry up the Informations you think you receive from Sense, sometimes in spite of contrary dictates of Reason, will, I hope, be prevailed with by the Argument I am about to propose, which is so manifestly grounded upon Sense, that without denying that we do feel what we feel, we cannot deny Cold to be a Positive Quality. For thus *Gassendus* most convincingly argues; *Cum per hyemem immittimus manum in labentis fluminis aquam, quod frigus in ea sentitur non potest dici mera privatio, aliudque prorsus esse apparet sentiri aquam frigidam, & sentiri non calidam. Et fac eandem aquam gelari, sentietur haud dubie frigidior, an dices hoc esse nihil aliud quam minus calidam sentiri? Atqui calida jam antea non erat, quomodo ergo potuit minus calida effici?*

Carnead. I will not say, *Themistius*, his Argument is not specious, but you, perhaps, or at least *Eleutherius*, will not affirm it to be more than specious, if you please to consider with me two or three things that I have to suggest about it.

And first, to shew, *Themistius*, that, whatever he was just now intimating, Experimental Philosophers do not prefer the immediate Impressions made on the Senses to the dictates of Reason, though they think the Testimony of the Senses; however sometimes fallacious, much more informing than the Dictates of *Aristotle*, which are oftentimes (and that groundlessly) repugnant to them; I will represent to you, that the Organs of Sense, consider'd precisely as such, do onely receive Impressions from outward Objects, but not perceive
what

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what is the cause and manner of these Impressions, the Perception properly so called of Causes belonging to a superior Faculty, whose property it is to judge whence the alterations made in the Sensories do proceed, as may easily be proved, if I had time and need to do so, by many Instances, wherein the Senses do, to speak in the usual phrase, mis-inform, and, as far as in them lies, delude us, and therefore must be rectified by Reason. As when the Eye represents a straight Stick, that has part of it under water, as if it were crooked; and two Fingers laid cross over one another, represent us a single Bullet or a Button roll'd between them, as if there were a couple: So that 'tis very possible (for I forbear saying 'tis true, having not yet proved it,) that though the Sensory be very manifestly and vehemently affected upon the contact of cold Water, or other cold Bodies, yet the cause of that impression or affection is, and may be judged and determin'd by Reason to be, other, than that which the Sense may to an inconsiderate person suggest. As when a Child, or one that never heard of the thing before, first sees a Stick, whereof one part is in the Air, and the other under Water, he will presently, but erroneously, conclude that *Phenomenon* to be caused by the Stick's being crooked or broken.

Next we may consider, that Sensations may in divers cases be made, as well from alterations that may happen in the internal parts of the Body, as from those that are manifestly produced in the external Organ, by external Objects and Agents; as may appear by Hunger, Thirst, the Titillation of some parts of the Body, barely upon Venereal thoughts,

thoughts, and (which belongs directly to our present Argument) the great Coldness that we have known Hysterical Women complain of in their Heads and Backs, and the great and troublesome degree of Cold, which we every day observe upon the first invasion of the Fits of Agues, especially Quartans ; which troublesome symptoms, that sometimes last for several hours, are therefore commonly called the Cold Fits.

And now it would be seasonable for me to call upon you to remember (and add to what I have now said) that which at the beginning of our conference I took notice to you of about Sensation in general ; if I did not presume that those things are yet fresh enough in your memory, to allow me to proceed directly to answer the Objection, which I shall do, though not like a *School-man*, yet like a *Naturalist*, by giving an account of the proposed *Phenomenon*, without having recourse to that *Hypothesis* which 'tis urged to evince.

I observe then, that though in the respective sence above-mention'd, Water, wherein the Objection supposes the hand to be plunged, be cold, in regard its parts are less agitated, than the Spirits and Bloud harbour'd in the Hand ; yet in a Philosophical sence, it is not quite destitute of Heat since 'tis yet Water, not Ice, and would not be a Liquor, but by reason of that various agitation of its minute parts, wherein fluidity, a Quality essential to Liquors, consists. Upon the score of this respective Coldness of the Water, the Hand is refrigerated ; for the Spirits and Juices of that Organ meeting in the Water with Particles much less agitated than they are, communicate to them
some

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some part of their own Agitation, and thereby lose it themselves, upon which Decrement of wonted Agitation, such a change is made in the Sensory, and, (though not so manifestly) in some other parts of the Body, as is perceived by the Animadversive Faculty under the Notion of Coldness; Sensation, (whatever obscure Definitions are wont to be given of it) being indeed an Internal Perception of the changes that happen in the Sensories.

And if now, as the Objection supposes, the Water wherein the hand is plunged comes to be more refrigerated than before, the Spirits, Blood, and other parts of the hand, finding the Aqueous Corpuscles more slowly moved than formerly, must, according to the Laws of Motion, (according to which a Body that meets another much more slowly moved than itself, communicates to it more of its motion than if 'twere less slowly moved,) transfer to them a greater measure of their own motion, and consequently themselves come to be deprived of it: And upon this increase of the slowness of motion in the parts of the hand, there follows a new and proportionable perception of the Mind, and so, a more vehement sensation of Cold. But though it be not to be admired, that the bare slowness of motion in the Object should be discernable by Sense, albeit it seems to participate of Rest, which with you passes for a Privation, since the Ear perceives when a Voice grows faint, and when a sharp Sound degenerates into a flat one; and we can perceive by the hand (abstracting from Heat and Cold) the celerity or slowness of Bodies that in their passage strike upon it, as for instance, of

Winds or streams; yet this is not the only thing I think fit to be taken notice of on this Occasion. For, I consider farther, that besides the most consistent and stable parts of the Hand, there are from the Heart and the Brain fresh blood and spirits continually transmitted to the Hand; and the former of these, the Blood, is, according to the Laws of its Circulation, and after it has received a great change in the much refrigerated Hand, carried back through other parts to the Heart; whence it is in the same Circulation distributed to the whole Body. To which may be added, that when the great refrigeration of the Hand happens, external Agents may contribute to the Effects of it, as I shall by and by have occasion to shew.

If then you please to remember, that upon the turning ones eye to the dark part of a room less inlighten'd than the Window, though Darkness be but a Privation, and though the Obscurity of that part be not absolute, but consist only in a less degree of Light; yet the action of the Spirits and other parts of the Body is so changed upon occasion of the Light's acting more faintly than was usual upon the Organ, that the Pupill is immediately and manifestly dilated, and in some cases, as in that which I mention'd to you of a Learned Man, much considerabler Effects ensue; you will not wonder, that, where not only the Spirits, but the Blood, (whence those Spirits are generated) that circulates through the whole Body, and upon whose Disposition all the other parts so much depend, is very much disaffected, there should be felt a great alteration in the Hand, which is the most immediately expos'd to the action of the cold Water.

And

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And for the Reasons newly given, it ought to be as little strange, that in other parts of the Body, the disorder'd and not circulating Blood should have its wonted action on them considerably alter'd; since the more stable parts, and especially those external ones that are most expos'd to the Cold, have their pores straiten'd, and consequently their Texture somewhat alter'd; on the same occasion on which the wonted agitation of the Spirits with the Particles that compose the Blood, is notably lessen'd. And that such Causes may produce great Effects in a Humane Body, you will be more prone to admit, if you consider the disorders that happen in the cold fit of an Ague, and oftentimes upon the shutting up of those excrementitious steams that are wont to be discharged by insensible Transpiration; to whose being stop'd in the Body by the constriction of the Pores, which chiefly happens through Cold, some Learned Physicians, especially the famous *Sennertus*, impute the cause of most Feavers, as indeed Experience it self does but too frequently shew it to be guilty of many.

Philopontus. I confess, *Carneades*, you have said some things that I thought not on before; but yet *Gassendus's* Argument seems to be such, that I fear 'twill be hard to hinder many from saying, That if Cold be but a Privation of Heat, 'tis a Privation of a strange nature: For, it may be introduc'd into Bodies that were not Hot before, nay, in some cases, into such as are naturally Cold, and also by consequence must have been put into a preternatural state to be at any time Hot.

Carneades. This Objection, *Philopontus*, being in effect

effect so much the same with that of *Gassendus*, that it differs from it but in the dress you give it, 'twill scarce require a peculiar and distinct answer; and therefore, as soon as I have reminded you of the Distinction, that we have formerly made of the Vulgar and Philosophical sence of the word *COLD*, I shall need to alter but a little what I said before, by telling you, that since Fluidity consists in the various agitation of the insensible Corpuscles of a Liquor, and that Heat consists in a *tumultuary*, but a more vehement agitation of the insensible parts of a Body, and so, that Hot Water scarce differs otherwise than gradually, from that which is cold to Sense; if Cold be taken in the larger and Philosophical sence, it may well be said, that as long as Water retains the form of Water, and so continues to be a fluid Body, though it may be very cold to the Touch, yet it is not absolutely or perfectly cold, and therefore is capable of a farther degree of coldness, which it receives when brought to Congelation: for till then it was not destitute of those agile Corpuscles, that were requisite to keep it fluid; and till then, *Gassendus* himself must acknowledge, that it was not absolutely or perfectly cold; because He, as you may remember, did in his former (but lately mention'd) Argument ascribe the Glaciation of Water to the invasion of those that he calls Corpuscles of Cold.

Eleutherius. Give me leave to add, *Carneades*, that 'tis not every Glaciation it self that brings Liquors to be perfectly Cold in the Philosophical sence of that expression, and quite expells or subdues all the agile Particles that were in the Water before 'twas turn'd into Ice. For, I think, that to effect

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effect this change, 'tis sufficient, that so many of these restless Particles be destroyed or disabled, that there remains not enough of them to keep the Water in a state of Fluidity, so that the surplusage may yet continue in the frozen Liquor, and whilst they are there, perform several things, as the making it evaporable in the Air, and even odorous, and by *their* recess or destruction the Ice may grow yet more cold. And *as* this Notion suits very well with the differing degrees of hardness, that we find in differing portions of Ice, sometimes upon the account of the matter, (as frozen Water is harder than frozen Oyl,) and sometimes upon that of the different degrees of Cold in the same Water or other matter, (as our Friend somewhere observes;) so it may be highly confirmed by an Experiment I saw him make, but that is not yet published.

The summ of the Experiment was this; That he first put an Hermetically seal'd Thermoscope into a Glass broader at the top than at the bottom, and greas'd the inside with Tallow, that Ice might not strongly stick to it. In this Glass was put Water, more than enough to cover the ball of the Instrument; and that Water being warily frozen, notice was taken, whereabouts the tinted Spirit of Wine rested in the Stemm; after which, the Instrument and the Ice being removed into the open Air, upon an exceeding frosty morning, the Ice was taken off from the ball, and presently after, the tinted Liquor, as the maker of the tryal expected, subsided a pretty way (the length of the Instrument considered) below the former mark; which argued that he rightly guess'd, that such a degree of Cold as is sufficient to turn Water into Ice, may not produce

a Body perfectly Cold; this Ice it self keeping the inclos'd ball, in a fence, warm, by fencing off the Air, which, at that time, (even in our temperate Clime) by the Effect appear'd to be colder than the very Ice. And, me thinks, it may strengthen *Carneades's* Discourse, to represent, that there is no sufficient cause, why many things that are reckon'd among Privations or Negations by the Peripateticks themselves, as well as Cold is by *Carneades*, may not admit of degrees; as may be exemplified by Deafness, Ignorance, and divers other things. And to bring a case, not very unlike that under consideration, we may take notice of a total Eclipse of the Moon, which you know alwayes happens when she is at the full. For Darkness in the Air being acknowledged to be a Privation or Negation of Light, when the Earth interposed between the Moon and the Sun has Eclipsed her, for instance, nine digits, (as Astronomers speak,) Men generally complain of darkness in the air, though there remain a considerable part of the *Discus* or the Hemisphere of the Moon obverted to us yet enlighten'd by the Sun; but when the interpos'd Earth proceeds to cover the remaining three digits, and so makes the Eclipse total, the darkness also is said and esteem'd to be much increas'd: Nor would men otherwise be perswaded, though *Themistius* should tell them, that the Air cannot have grown darker, though it were dark before, and indeed though the Air was more and more darken'd in proportion to the increase of the Eclipse, yet it was never compleatly darken'd till it became total. But I fear I dwell too long upon one Argument.

S E C T. IV.

Elen. **L** Et me therefore, *Carneades*, sum up what I take to be your Doctrine, and tell these Gentlemen, that I think you do not look upon the Sensation of Cold as a thing effected by an intire Privation properly so called and consider'd as such, but that according to you that slowness of motion in the Particles of cold Water, which the Hand finds when 'tis thrust into that Liquor, does occasion the Spirits and the Corpuscles of the Blood to part with to those of the Water a considerable share of their own surplusage of agitation, whereby they lose it themselves, upon which is consequent a Perception of this change made in the Hand, which, if it be very great, is also frequently accompanied with some sensible change in other parts of the Body, occasion'd chiefly by the frequent returns of the circulating and highly refrigerated Blood to the Heart, whence 'tis dispersed to the whole Body. According to which Doctrine, the Sensation of Cold is but a perception of the lessen'd Agitation of the parts of the Hand either stable or fluid, especially of the Blood; which alterations are in great part produced, not by the coldness of the Water, as Cold is a Privation, but from the new modification of the action of the Blood and Spirits upon the Nervous and Membranous parts, the constriction of whose Pores concurs to that Modification. And, if I do not misunderstand your Opinion, *Carneades*, methinks it may be confirmed by this which I have known observed

served by experienc'd Chirurgeons, that by too strict Ligatures unskillfully made, an Arm, for instance, may be gangrenated; in which case, all the proper and immediate effect of the Ligature is but the constriction of the part, though that constriction being unusual and excessive, it proves the occasion of the mortifying of the Hand and Arm by hindring the free and usual access of the Blood and Spirits to that Limb; upon which, by the depraved action of the parts of the Body one upon another, and the concurrence of external Agents, there ensues a Mortification or Gangrene of the part, which, if due Remedies be not timely employed, is communicated to other parts and kills the Man.

Carneades. Whatever become of your Instance, *Eleutherius*, I thank you for your readiness to propose it in favour of my Hypothesis, which you will easily judge not to be much concern'd in the close of the excellent *Gassendus* his Arguments for the Positive Nature of Cold. For though these words of his _____

Themistius. You may save your self the trouble of naming them now, since, whatever they may seem to you, I profess I look upon them as containing a distinct Argument, which I shall therefore propose in its due place hereafter; but in the mean time, and before we leave the Argument you would have us dismiss, give me leave to remind you, *Carneades*, of some part of your former Discourse, and to take thence a rise to tell you, that you, who told us that we ought not to consider the Operations that Qualities have upon our own Sentories only, but also what they do to other Bodies,

will,

will, I hope, allow me to demand, how a Privation, or if you will, how an Imminution of Motion can produce the hundredth part of those Effects which we daily see produc'd by Cold in the Bodies that are about us.

C Carneades. I thought, *Themistius*, I had intimat-
ed to you already, what might have prevented
your Question; but since I see 'tis otherwise, you
shall not find me backward to explain my self a little
more fully. I do not pretend, that either an abso-
lute privation of motion in a Body, or a slowness
of motion in the parts of it, is, as such, the proper
Efficient cause of the Effects, vulgarly but unduely
ascrib'd to Cold alone; for, in my opinion, Cold
is rather the Occasion, than the true Efficient Cause
of such Effects, which, I think, are properly to be
ascribed to those Physical Agents, whose actions or
operations happen to be otherwise modified than
else they would have been upon the occasion of that
imminution or slackness of Agitation which they
meet with in cold Bodies, by occasion of which
they are both deprived themselves of the Agitation
they communicate to such slow Bodies, and there-
by act no longer as, were it not for that loss, they
would, and by a natural consequence of this change,
which is made in themselves, they do also, though
less notably, modify the action of other Bodies up-
on them: From which unusual alterations happen-
ing in a World so fram'd as this of ours is, and go-
vern'd by such Laws respecting Motion and Rest as
are observed among Bodies, there must in all pro-
bability result many new, and some of them confi-
derable, *Phænomena*. For though Quiescent Bodies
seem not to have any action which among corporeal
sub-

substances seems to be perform'd only by Local motion; yet Bodies quiescent themselves may concur to great Effects both by determining the motions of other Bodies this or that way, or by receiving their motion totally or in part, and so depriving the formerly moving Bodies of it: Thus the Arches of a Bridge, though immoveable themselves, by guiding the water of the River that beats against them, may occasion a rapid and boisterous stream, capable to drive the greatest Mills, and perform more considerable effects, though the River, before it met with them, ran calmly enough, as is evident at *London Bridge*, especially when the Water is near a low Ebb. And now I have mention'd Water, I will add, that though Water it self be not a quiescent Body, but being a Liquor has its parts in perpetual motion among themselves; yet since that agitation is exceeding slow in comparison of the swiftness of a Cannon-bullet, in respect whereof the calm surface of the Water participates of the nature of a Quiescent Body, Bullets themselves shot from out of Guns elevated but little above the Level of the Water, (upon which score they make but a very sharp angle with it;) these Bullets, I say, do not unfrequently rebound from the Surface of the Water, and consequently, even these so wonderfully swift Bodies receive a new Determination from it.

- *Eleutherius*. One may add, *Carnades*, to your Instances, that in a Tennis-Court the wall, against which Balls are strongly impell'd by a Racket, contributes much to the mischief that those Balls do often to By-standers in the Gallery, as the Wall, though it self unmov'd, gives a new Determination

to the moving Ball, and by its resistance, makes it rebound or reflect at an Angle equal to that of the Ball's incidence. And this concurrence of the Wall to such Effects is the more evident, because of this other circumstance, (which also befriends your Opinion,) that, if the impell'd Ball, instead of hitting against the Wall, hits against the Net, this by yielding deprives the Ball of its *Impetus*, and hinders the reflection that would else ensue.

Carnead. You have, I confess, somewhat prevented me, *Eleutherius*; but yet not altogether. For though I was going to propose the example of a Ball, yet 'twas in somewhat a differing way; for I was about to propose to *Themistius* the example of a Ball, which if it be forcibly and perpendicularly thrown against the hard Ground, has its Determination so alter'd, that whereas it moved before towards the Centre of the Earth, it immediately, with almost the like swiftness of motion, tends directly upwards. And if on the other side you throw the Ball, not against a hard, but against a muddy piece of ground, it will not rebound, losing its own motion, by communicating it to the parts of the yielding Mudd; as may be in some measure illustrated by the great commotion made in a small Pond of Water, when a Ball (or a round stone) being but gently let fall upon the surface of it, has its motion thereby deaded, and transferr'd to the parts of the Liquor, which perhaps will be visibly agitated at the remotest brink of the Pond.

Eleutherius. These Examples may conduce much to explicate your Doctrine, *Carneades*, but since *Themistius* himself was so equitable a while agoe, as
to

to allow you much time to defend such a Paradox as yours against *Gassendus's* Argument, I shall with your leave (of which I doubt not) to the Examples already mention'd add this one more. Suppose upon a stream that runs through some Town (which is not very rare) there were built a number of differing Mills, some for the grinding of Corn, others for the Fulling of Cloth, others for the moving of Bellows to melt Oars and Metals; others for forging of Sword-blades; others for making of Paper, and others for other uses: And suppose that an Enemy coming to besiege this Town, should successfully imitate *Cyrus's* Stratagem, when by suddenly diverting the course of *Euphrates* he took *Babylon*; would it not be consequent to this derivation of the Water into some lower place, and this ceasing of the Stream to run in its former Channel, that the action of all these Mills, by which so many differing operations were perform'd, must of necessity cease too? though the Besiegers do not produce this change by any positive and direct violence that they offer to the Mills, but onely by hindring them from receiving the wonted Impulses which were requisite to keep them in motion.

Carneades. I dislike not your Instance, *Eleutherius*, which yet will not altogether render useless what I was going to say about a Wind-mill, which will illustrate one part of my Doctrine, for which your Water-mill does not seem to have been intended. And that this Example may the better do so, I will suppose a Wind-mill to be built in some low place near the bank of your stream, which stream we will suppose to be lyable, as some others are, upon the falling of great and sudden rains upon

on the neighbouring hills, to overflow its banks, in case the increase of the Water be not then hindered by the Wind-mills lifting up constantly some parts of it, and conveying it away by Pipes or otherwise: And then let us suppose, what really sometimes happens, that the Wind should so cease, that there should not blow any wind strong enough to move the sails for a great while together; will it not hence manifestly follow, that by reason of this absence of the Wind, which absence has the nature of a Privation or Negation of a Stream-like motion in the Air, not only there will be a ceasing of those Effects and Operations whatever they were, that were wont to be perform'd within the Mill it self, but also there will be a durable intermission of that main work of the Mill whereby it carried off such a quantity of Water; which work ceasing with the Wind, whilst the flowing in of the Water does not cease too, but continues as formerly, the still-increasing Water must bear down or overflow its wonted Banks or other Boundaries, and by its unruly effusions drown the neighbouring parts, and produce the Disorders, that is, the new *Phænomena*, naturally consequent to an Inundation made by such a quantity of Water. And if the Water conveyed away by means of the Mill through Pipes or Channels were employed to water Grounds, or other particular uses, the growth or fertility at least of the Vegetables that Water was requisite to nourish, or the other uses to which it was necessary, must consequently be much, if not totally, hindered.

Philoponus. I know not whether we may not refer to the Subject of your Discourse, what may be

be observ'd in *Paralytick* affections, where a little Viscous or Narcotick Humour obstructing or otherwise disaffecting one part of a Nerve, though its proper and immediate action be only to hinder or weaken the Spirits, that were wont, in competent plenty, to pass freely along the Nerve to the Muscles whereto it leads; yet the action of the other parts of the Body and the Relaxation of the Fibres do oftentimes produce a tremulous motion in the Limbs, and particularly the Hands; and sometimes also the Mouth, Neck and other parts, are drawn awry in an odd and frightful manner.

Carnades. Though I approve of *Philoponus's* fancy, yet I think a more quick and notable Instance to the same purpose may be taken, from what happens to Birds, and Rats, and Cats, and such kind of warm Animals, in Mr. *Boyle's* Engine. For as the Air by the agitation of its parts, or that of some Ethereal substance that pervades it, entertains the fluidity of Water and other Aqueous Liquors; and when that agitation is hinder'd or too much lessen'd, Water ceases to be fluid, and upon that divers Violent Effects ensue, wont to be ascrib'd to Glaciation: so the bodies of warmer Animals, having been born in the Air, and perpetually expos'd to the action of it, (though that be seldome heeded) when being plac'd in the Receiver of the Air-pump, and by the operation of that Instrument, which withdraws the former Air and keeps out the new, the Air that was wont continually to act upon them, is kept from doing so any longer, though this absence, or not touching of the Air, be but a privative or negative thing, yet by reason of the structure of the *Animal*, his Spi-
rits

rits and Humours, assisted by the concurrence of more general Causes, are brought to act so differing from what they were wont to do, that the Blood and Juices swell, the Stomach vomits, the *Animal* grows faint and staggers, the Limbs, and at length the whole Body are convulsed, the Circulation is stopp'd, and at last the whole *Animal* kill'd; and all this done in a very few minutes of an hour, without the visible intervention of any positive Agent.

Eleutherius. What you say, *Carneades*, concerning the quick and violent Death of warm *Animals* in Mr. Boyle's Engine, puts me in mind of an Experiment I saw made in that Instrument upon cold *Animals*, which, methinks, may well illustrate the Comparison we lately employed of a Wind-mill. For as those great artificial Engines lose their Motion, and the Operations depending on it, if that Stream of Air, we call the Wind, be held from keeping them going; so Insects and some other cold *Animals* have their differing motions so dependent upon the contact of the Air, that, as soon as ever they are deprived of it (by the Engine we are speaking of) divers sorts of them will lye moveless as if they were dead; and I have known several of them that were put in together, continue in that state for many hours, as long as it pleas'd our Friend to with-hold the Air; but when once He thought fit to let a Stream of Air enter the Receiver, these seemingly dead *Animals*, as Worms, Bees, Flies, &c. like so many little Wind-mills of Nature's (or rather her great Author's) making, were set a moving in various manners (as creeping, flying, &c.) suitable to their differing *Species*.

Carneades. So that to sum up in a few words the Result of these Instances, and the rest of the past Discourse on the same Subject, it appears by what has been said, that the Effects undeservedly ascrib'd to Cold, need not in our Hypothesis be referr'd to a Privation, but to those positive Agents or active Causes, which by their own nature are determin'd to act otherwise on, or suffer otherwise from, one another, in cases, where there is a great hindrance or ceasing of wonted agitation, than where there is not.

SECT. V.

Themist. **I**T may perhaps now be time to put *Carneades* in mind, that, in what he has been discoursing all this while, he has propos'd Answers but to a couple of *Gassendus's* Arguments, and left the rest untouch'd.

Carneades. I should readily grant, *Themistius*, that I have dwelt too long upon so few Arguments, if I did not hope, that by fully answering *Them*, and giving the Company a particular account of my Notions concerning Cold, I might very much shorten and facilitate the remaining part of my Task, which engages me to return Answers to the other Arguments you speak of, the grounds of solving which, I think, I have already laid in the past Discourse. And therefore you may go on to propose the next Argument of *Gassendus*, as soon as you please.

The

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Themistius. And I shall do it, *Carneades*, in that Learned Man's own words, which I well remember to be these: *Fac manum immitti in aquam nunc calidam, nunc frigidam; quamobrem manus intra istam, non intra illam refrigeratur? an quia calor manus intra frigidam retrahitur, manusque proinde relinquitur calida minus? At, quidnam calor refugit, quod intra frigidam reperiatur? nonne frigus? At si frigus est tantum privatio, quidnam calor ab illa metuit? Privatio sanè nihil est, atque adè nihil agere, unde ejus motus incutiatur, potest.*

Gassend. Lib.
6. Cap. 6.

Carneades. This Objection, *Themistius*, may indeed puzzle many School-Philosophers, but will easily admit an answer in my Hypothesis. For that does not oblige, or so much as tempt, me to ascribe (as a *Peripatetick* would do,) to a meer Quality, (for such is Heat,) both a knowledge of its danger, and a care and skill to preserve it self from its Enemy, the Cold, by a retreat inwards. For, agreeably to what I lately delivered, 'tis obvious for me to explicate the *Phenomenon* thus: When a man puts his Hand into warm Water, the agitation of the Corpuscles of that Liquor surpassing that of the Spirits, Blood, and other parts of his Hand, cannot but excite in him a sense of Heat; but when he puts the same Hand into cold Water, the case ought to be much altered, not by any imaginary retreat of the Spirits, but the communication of motion by other parts to the surrounding Water, by which means there must be in the Hand a great lessening of the former agitation of its parts, the perception or sense of which decrement of motion is that which we call the Feeling of Cold.

Eleutherius. I think indeed, *Carneades*, that though this Argument may be considerable against those that the Learned framer of it might have in his Eye, it is but invalid against you. But can you as well decline the force of that other Objection, which *Gassendus* more insists on, and which seems as directly to oppose you as any other Adversaries of his Hypothesis?

Themistius. I presume, *Eleutherius*, you mean that cogent Argument, which *Gassendus* proposes and prosecutes more fully than the rest, deducing it from the way of artificially freezing Water by a mixture of Snow and Salt, placed about the outside of the Glass that contains the Liquor. For from this practice he rationally concludes, that since this frigorifick mixture is through the Glass able to freeze the Water into Ice, it may as justly be affirm'd to act by Corpuscles of Cold, as Fire can be to act by Calorifick Corpuscles, when kindled Coals, placed on the outside of the Glass, make the contained Water boyl. And this cogent Argument will, I hope, prove the more satisfactory to *Carneades*, since 'tis not drawn from what he would call a disputable Peripatetick Notion, but from the same Quiver, whence he affects to take his Shafts, Experience it self.

Carneades. I freely acknowledge, Gentlemen, this Argument to be very plausible; but that it is clear and cogent, I must not grant, till I be better satisfied that it is so.

And, I shall scarce think it as evident, that Ice and Salt act by a Positive Quality, as that burning Coals do so, though Cold seems as well to be produc'd by the former, as Heat by the latter. For

innumerable Experiments shew, that *Heat*, in the Fire especially, is a Positive Quality, consisting in a tumultuary and vehement agitation of the minute parts of the Body that is said to be hot, and producing also in the Bodies that 'tis communicated to, a local motion, which is manifestly a positive thing: This is so evident, in the heating of Bodies by mere attrition, the smoaking and melting of divers Bodies in the Sun-beams (especially at fit times of the day and year,) the sudden boiling and dissipation of Water, Oil, &c. dropt on a red-hot iron, and many other obvious instances, that 'twere a needless work to go about to prove it, especially since both *Themistius's* Peripateticks, and *Gassendus* himself, who so often disagree about other things, agree in confessing that Heat is a Positive Quality.

Themistius. But remember, *Carneades*, that the grounds on which they do so, are the same, on which *Gassendus* justly builds the Proposition, that Cold also is a Positive Quality.

Carneades. I did not forget that, *Themistius*; for I was about to subjoyn to what I last said, that 'tis evident not onely by the confession of my Adversaries, but by that (which to me is much more considerable) of Nature her self, proclaiming it in the Instances I just now mentioned, that Heat is a Positive Quality; whereas that Cold likewise is so, does not appear to me by the Experiment of Artificial Congelations. For, in this all that is clear in matter of fact is, that Snow or beaten Ice and Salt are put about a Vessel full of Water or other Aqueous Liquor, and that, within a while after, this Water begins to be turn'd into Ice; but, that this Glaciation is perform'd by swarms of atoms of Cold,

that permeating the Glass, invade and harden the Liquor, is not perceiv'd by Sense, but concluded by a Ratiocination, the cogency of which I am allow'd to examine without affronting the certainty of Sense; that not being concerned in the case. If then an intelligible way can be propos'd of fairly explicating the Phenomenon, besides that insisted on by Gassendus, the objection drawn from this Experiment against my Hypothesis will be invalid. And such an Explication Monsieur Des-Cartes in-

Lib. Meteor. Cap. 3. ingeniously gives in his Meteors: *Quia Materia Subtilis (sayes he) partibus hujus aqua circumfusa crassior aut minus subtilis, & consequenter plus virium habens, quam illa qua circa nivis partes harebat, locum illius occupat, dum partes nivis liqnescendo partibus Salis circumplombantur. Facilius enim per salsa aqua quam per dulcis poros movetur, & perpetuo ex corpore uno in aliud transire nititur, ut ad ea loca perveniat in quibus motui suo minus resistitur: quia ipso materia subtilior ex nive in aquam penetrat, ut egredienti succedat, & quum non satis valida sit, ad continuandam agitationem hujus aqua, illam concretere sinit.*

Philoponus. I leave Themistius to consider, whether this Explication be without Exception; but I confess it is not without Analogy, and that even amongst the four first Qualities themselves. For when we Chymists have a mind to dry (for instance) the Calces or Precipitates or other Powders, from which we have filtrated the Liquors we employ to wash or dulcifie them, 'tis usual either to put the Filters, wherein these Powders remain almost in the form of Mudd, or to spread the stuff it self upon brown Paper or pieces of Brick or Chalk,

Chalk, which much hasten the exsiccation of the things laid upon them, not by any drying Particles which they emitt into the soft substances, but by imbibing the superfluous parts of the Liquor, and thereby freeing from them the Substances to be dried. And, I remember, I have seen our Friend Mr. Boyle, by immersing a piece of soft crumb of bread into an actually cold Liquor, that would hastily imbibe its Aqueous Corpuscles, and dry it in a minute or two of an hour so as to make it feel hard.

Eleutherius. These instances bring into my mind another Chymical Experiment, that I have seen made by the same Gentleman, which was; That by putting into weak Spirit of Wine a sufficient quantity of Salt of Tartar, he quickly defleamed the Spirit without Distillation, or so much as Heat. And this will the better illustrate the *Cartesian* Explication, because 'tis manifest by the change that will be made of the most part of the Salt of Tartar into a Liquor that will not mix with the now defleamed Spirit of Wine, that the reason of the Operation is, that the Aqueous Particles of the Phlegmatick Spirit, finding, it seems, more convenience or facility to continue their motion among the Fixt Corpuscles of the Salt, than the Vinous ones of the Spirit, pass into the Alkaly and dissolve it; and thereby desert the Liquor through which they were diffused before. And I know another Saline body, that so unites with Water, as not to be, by the Eye, distinguishable from it, and yet is of such a Texture, that Water is so much less disposed to mingle with it than with Spirit of Wine it self, that it will forsake the Body it kept in agitation,

to pass into this Spirit; and so leave that which it kept in the form of a Liquor before, to appear in the form of a consistent Body; which instance comes somewhat nearer than the former to the Experiment of Glaciation.

Cartneades. Though what you have recited, Gentlemen, be not unwelcome to me, yet, I think, I can propose you an Experiment fitter to dilucidate the *Cartesian* Explication. For, I remember, that your common Friend, having a mind to shew, that a small proportion of agile matter, invisibly diffus'd through a Body that would be otherwise consistent, may bring it to, and keep it in, the state of Fluidity; devised and shewed me the following Experiment. He took Camphire broken into small bits, and casting a convenient quantity of it upon *Aqua fortis*, suffer'd it to float there, till without Heat the Camphire was dissolv'd into a Liquor, and it look'd and felt like an Oyl, which, though shaken with the *Aqua fortis*, would emerge to the top again. If this Oyl were kept well stop'd, that the Spirits of the Menstruum might not evaporate, it would (as he assum'd tryal had taught him) continue long fluid, he having sometimes kept it a year or two or more. And that 'tis the agile Spirits of the *Aqua fortis* that keep the Camphire fluid, he has made probable by divers things that I must not now stay to recite. And that the quantity of these agile Particles is but small, I am induc'd to think by this, among other things, that when I have made a small parcel of but moderate *Aqua fortis* turn a pretty proportion of Camphire into Oyl, & separated that Oyl from it, I could, by casting fresh Camphire on the same Menstruum, reduce that also into the form of Oyl.

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Oyl. Now, that these Fluidifick Spirits (if I may so call them) are not sensibly warm (no more than the *Cartesian Materia Coelestis*) in Water, is manifest to the Touch: And whereas I at first suspected, that the reason, why the pouring of this Oil into Water doth presently reduce it into Camphire again, might be the coldness of the Water; I after thought, upon a farther information, that the reason rather was, that the Nitrous Spirits being dispos'd to pass out of the Oyl into the Water, this Liquor readily imbib'd and diluted them, and consequently disabled so many of them, that those that remain'd could not do their former work any longer: since he had tryed purposely, that the Reduction of the Oyl into Camphire would presently be made, though that Liquor were not pour'd into Cold Water but Hot; so that the agitation, that it received from the particles of the *Menstruum*, though not to our Touch sensibly warm, was much more efficacious, than that which it received from the Heat of the Water.

Eleutherius. I know not, whether besides the Instances that have been now propos'd, one may not alledge such an Argument also in favour of the *Cartesian* opinion about Cold, as would not be insignificant, though it should be made appear, that Cold may sometimes be produced by or upon the Emission of Corpuscles, that in some sence may be call'd Frigorifick. For there may be Corpuscles of such a Nature, as to size, shape, and other attributes, as to be fit to enter the Pores, and pierce even into the inward parts of Water, and some other Bodies, so as to expell the calorifick Corpuscles they chance to meet with, or to clog or hinder their activity, or on some other account confi-
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derably to lessen that agitation of the minute parts, by which the Fluidity of Liquors and the Warmth of other Bodies is maintain'd. But, even in such cases, though the Agent and the Actions that produce Coldness, be Positive things; yet the Nature of Coldness it self may consist in a Privation. As when a man is kill'd by a bullet, his Death is effected by a Positive and even impetuous Action, and yet Death it self is but a Privation of Life. If also in a dark Room a man cast cold Water upon a burning Coal, though the Water act by its Positive Quality of moisture, and, by virtue of that, extinguish the Fire, and by that means destroy the Light, yet the Darkness that is consequent upon this action, is not a Positive thing but a Privation.

S E C T. VI.

Philop. **T**He pause you here made, Gentlemen, makes me think it seasonable to put the Company in mind, that it begins to grow late, and therefore to call upon *Themistius* to produce what he has yet to alledge out of *Gassendus*.

Themistius. The Philosopher you have nam'd, has indeed another Weapon to destroy the Error about Cold, which he confutes. And this Argument like a two-edged Sword that cuts on both sides, does not only confirm what he maintains, but destroy the chief objection that can be made by his Adversaries. The Argument I speak of he proposes in these terms: *Tameise multa videantur ex sola caloris*

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caloris absentia frigescere, nihilominus nisi frigus extrinsecus introducatur, non tam profectò frigescere quam decallescere sunt censenda. Esto enim lapis, lignum, aut aliquid aliud, quod nec calidum nec frigidum sit, id ubi fuerit admotum igni calefiet sanè; ac cum deinceps calor excedet, neque frigidum ullum circumstabit, non erit cur dicas ipsum frigestori potius quam minus calidum fieri, redirève in suum statum.

Carneades. Whether this contain not a dispute *de modo loquendi*, I shall leave the Company to judge by what I shall return in answer to it. I say then, that it seems to me, that there is in the Discourse an Obscurity, if not an Ambiguity, though I am confident not affected by the Candid *Gassendus*. But to answer as directly as I can; If we speak only of a Coldness as to Sense, I see not, why Water or Wood or any such Body that is heated by the Fire, may not upon its removal thence be said to grow Cold and not barely to *decallescere* in our Philosophers sence of that word. For the Heat and Coldness of Water, in reference to Sense, consisting, (as I lately shew'd) in this, that the Particles of it are more or less agitated than the Hand that is immerfed in it, they need nothing else to make the Liquor grow Cold, than such an imminution of the brisk motion of its Corpuscles, that they cease to be as much agitated as those of our Organs of Feeling: And if this already impair'd agitation be still more or more lessen'd, the Liquor will still grow colder and colder without the help of any Positive Cause, 'till at length the agile parts, that kept it fluid, being quite expell'd or disabled, the form of the Liquor comes to be exchange'd for that of Ice.

Philoponus. But what say you to that part of *Gassendus's* Argument, where he proposes an *Adiaphorous Body*, which, when affected with an adventitious Heat, would not grow cold by the bare removal or cessation of that Heat, unless it were refrigerated by an Agent, that were positively and actively Cold?

Eleutherius. I say, *Philoponus*, this Supposition should not be made, and that I know of no such *Adiaphorous Body*. For since, as I have been oblig'd to inculcate, those Bodies must be Cold as to sense, whose parts are less agitated than those of our Hands, and consequently Metals, Stone, Wood, and other Solid Bodies, and also Water, Wine, and all other unmingled Liquors we know, being heated by the Fire, will grow cold again of themselves, because the adventitious motion ceasing by degrees, either upon the recess of the *Igneous Corpuscles*, or the imparting of the extraneous agitation to the Air or other contiguous Bodies; the Stone or Water, &c. will again have so much fainter an agitation, than that of a mans Sensory, as to be by him judged Cold: And because almost all the *Species* of permanent Bodies here below that are known, have in what is call'd their Natural state a less degree of Agitation of their Insensible Parts, than mens Organs of Feeling are wont to have, those Bodies may be said to be Naturally Cold, and therefore ought not to be suppos'd to be indifferent to Cold or Heat.

Philoponus. But whether or no Nature do really afford us an *Adiaphorous Body*; yet surely the mind is able to conceive one, and therefore *Gassendus* may be allowed to suppose such Bodies, and

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Carneades may be oblig'd to answer what he argues upon that Supposition.

Carneades. 'Tis one thing to propose an Adia-phorous Body, as barely an intelligible or a possible thing; and another, to give Instances of it, as *Gassendus* has done in particular Bodies, in which that indifference is not to be found. And 'tis this last kind of Supposition that I disallow'd in *Gassendus's* Argument. But if a Body should be propos'd as Adia-phorous in reference to Heat or Cold, I might say without prejudice to my Cause, that if such a Body should be carried into a hot place, it might there grow warm; and if it should be removed back again, and kept till it lost that new adventitious Heat, it might rather *decalescere* than grow cold as to Sense. But the reason is, because 'tis not every degree of imminution of Heat that is able to denominate a body Cold, but such a degree as reduces the parts of it to a fainter motion than is at that time in those of our Organs of Feeling; and till this be done, or at least very near done, the propos'd Body is still (if I may so speak) in the state of Heat as to Sense: Which last words I add, because that in reference to other Bodies it may then be notably refrigerated. As Lead that has but heat enough to keep it in Fusion, may, by the pouring on of such Water as to a mans hand would feel Hot, be brought to grow hard, which loss of Fluidity is also the Natural Effect of Cold, though perhaps both the Metal and the Liquor be yet as to Sense considerably Hot.

Eleutherius. So that, according to you, none of the kinds of Bodies that are actually known in Nature, are Adia-phorous as to Sense. On which occasion

sion let me note by the by, that the frequent Variations of Sense must render it but an uncertain standard of Heat and Cold: And upon supposition, that there were an Adiaphorous Body in reference to our sense; yet it would not be so in reference to all other Bodies, or, in the phrase of our *Verulam* speaking of Heat, *in ordine ad Universum*. And for what remains, the controversie grounded on *Gassendus's* Argument seems to be rather Verbal than Real, and may be determined or compos'd by setting the distinct acceptions of the words *Cold* and *Heat*.

S E C T. VII.

Philop. **W**herefore I wish, that we may not waste the little time that is left us upon Niceties of no greater concernment; and I think this short time would be better employ'd, if *Carneades* would be pleas'd to tell us a little more particularly, what he supposes to be the thing that with-held *Mr. Boyle* from delivering an opinion about the *Nature of Cold*.

Eleutherius. Yet me thinks 'tis but fair, that *Carneades*, who has all this while been confin'd to the answering anothers Arguments, should now take his turn to propose his own.

Carneades. I find in each of your motions, Gentlemen, something so equitable and so expedient, that I shall in part comply with both. And that I may

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may hasten to do what *Philoponus* desires, I shall do no more than briefly point at two things that may be alledged in favour of the Hypothesis I defend. For if you reflect upon what we have already discoursed, we may take notice of things there, that will scarce be well accounted for by being ascribed to Positive Cold, but may be far better explained agreeably to our *Hypothesis*. And I must add in the next place, that I, who sustained the person of a Respondent, may pretend to have sufficiently discharged my Office, if I have shewn the invalidity of all the Opponents Arguments; and 'tis his part who asserts a positive thing in Nature, to make it good, whereas he that denies it, needs not alledge any other reason why he does so, than the Authority of that justly received Axiom in Philosophizing, *Entia non sunt multiplicanda absque Necessitate*. And, I hope, there will need no other Engine to demolish an ill-formed and proofless Opinion about Cold, than an *Axiom* so Solid and Efficacious, that in the Opinion of almost all the Modern Naturalists it has been able to abolish such potent and immense Bodies as the *Primum Mobile* it self, and a superior Orb or two, the least of which contained that Firmament, in comparison whereof the whole Earth is but a point. And not only so, but the same Axiom has banished the Angels and Intelligences from the Celestial Orbs, that *Aristotle* and his followers had assigned them to turn about; or rather hath released those Noble and Happy Spirits from the drudgery to which the Philosophers of so many ages had needlessly doom'd them.

Eleutherius. I the less distrust the validity of the Axiom you alledge; because I observe it to be the ground, on which is built a great part of the Reformation of Philosophy, that is introduc'd by the Moderns. For one of the main things that first moved considering men to seek for more satisfactory Opinions than those of the *Peripatetick* Schools; was, that these obruded a great many Tenents in Philosophy, that were not only un-proved, but unnecessary to the Explication of the *Phænomena* of Nature; as 'twere not difficult to shew.

But I see *Philoponus* preparing to renew the motion he lately made, in which the shortness of time makes me now think it seasonable to joyn with him; I being no less desirous than he to know, what may be the motives of your Friend's declining to declare himself fully about the Nature and Cause of Cold.

Carneades. I have already intimated to you at the beginning of our Conference, that he is himself the fittest person to be address'd to for satisfying this inquiry. But not to be altogether silent on this occasion, I shall tell you, that, as far as I can guess; he waits till farther Tryals and Speculations have resolv'd him in some points, wherein he is not yet satisfied: For, being of a temper backward enough to acquiesce without sufficient Evidence, when the inquiry is difficult and the subject important; he seems to me to be kept in suspence, both by some Speculative doubts, and the *Phænomena* of divers Experiments, some of which are not deliver'd in his Book. It would be now improper to mention the
scruples

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scruples and hesitations they have occasioned in him; though of those, I have heard him speak of; I shall name some Instances that occur the most readily. As I remember I heard him make inquiry, as to those that would have Cold produced by Corpuscles of Cold; *Whether*, and on what account, those little fragments of Matter are cold? *Whether* those frigorifick Particles, that must in multitudes crowd into Water to turn it into Ice, have Gravity or Levity, or are indifferent to both? And how any of the three Answers that may be made to this Inquiry, will agree to some *Phenomena* that may be produced? *What* Structure the Corpuscles of Cold can be of, that should make them frigorifick to that innumerable variety of Bodies they are said to pervade? And *whether* the frigorifick faculty of these Corpuscles be loofable or not? As also *whether or no* they be Primitive Bodies, and if it be said, they are not, whether there was not Cold in the World before they were produced, and whence that Cold could proceed? And if it were said they *are* Primitive Bodies, he demanded, *how* it came to pass, that, by putting a certain factitious Body actually warm into Water that was also warm, (both which appeared by a good sealed Weather-glass,) there should presently be produced an actual Coldness (discernable by the same Thermoscope?) These, and I know not what other Scruples and Difficulties, suggested to him by his thoughts or his Experiments, were the things that I suppose prevail'd with a man of his temper to forbear for a while the declaring of his Sentiments about Cold, lest the Event of some

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father tryal should shew him cause to retract them.

Philoponus. What you have freshly intimated, *Carneades*, of Mr. Boyle's having other hesitations than those you have named and suggested by Experiments not published in his History, does, I confess, the more excite my Curiosity to have at least a taste of those perplexing *Phænomena*.

Carneades. You may easily guess, *Philoponus*, by what I have told you already, that you are not to expect a full satisfaction from me on this occasion. But yet, that your curiosity may not be frustrated, I shall venture to acquaint you with two *Phænomena*, which were, I suppose, none of the least motives of his backwardness to declare himself. But though some body perhaps thinks, that the grounds of solving these *Phænomena* and most of the newly recited Scruples, may be pickt out of some things that may already have pass'd among us in this Conference; yet because we have not now time to enter upon a discussion of this matter, I am willing you should suspend the debate, till we have occasion to meet another time; and therefore I shall now only acquaint you with a couple of Experiments, that he set down for a *Virtuoso* who was to solve the two main Problems suggested by them. The *first* whereof was, Whence Water should upon Congelation acquire so vast a force as he found it had to lift up great Weights and burst containing Bodies; though it seem'd by several Circumstances, that the motion of the Water is very much diminished when 'tis changed into Ice. And the *second* Problem is thus conceived; If, as a brisk agitation of a Bodies insensible parts produces

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duces Heat, so the Privation of that Motion is, as *Cardan* and the *Cartesians* would have it, the cause of Cold; whence is it, that, if certain Bodies be put together, there will be a manifest and furious agitation of the small parts, and yet upon this conflict the mixture will not grow hot, but sensibly and even considerably Cold? The Narratives themselves of the Experiments are too long to be now read over to you. And therefore I shall leave the Paper, that contains them, among you, to be perused at your leisure, between this and our next meeting, till when I must bid you farewell: Only desiring you in the mean while to remember, that, as I have but acted a part imposed upon me in our past Conference, so notwithstanding any thing that I have said in my assum'd Capacity, I reserve to myself the right of appearing as little preingaged as any of you at our next meeting.

F I N I S.

TWO PROBLEMS
ABOUT
C O L D,

Grounded on
NEW EXPERIMENTS,
And Proposed
In a LETTER to a FRIEND.

By the Honourable *ROBERT BOYLE.*



To my very Learned Friend
Mr. J. B.

S I R,

I Presume that you will not be surpriz'd to be told, that I send you the inclos'd Papers, not only that I might gratifie your Curiosity, but that you may by them be inabled to help me to satisfie my own; and therefore I shall accompany the Historical Transcripts I made of the following Experiments, as I found them registred for my own Remembrance, with some of the doubts suggested to me by some of the *Phænomena* that occur'd. But yet I shall not trouble you with all the difficulties that at first troubled me, but reduce the Exercise, I desire to give your sagacity, to the solution of two Problems. And I will begin with propounding that first, which is grounded upon the last of the two following Papers, because though the Historical part of that be much the longest, yet the grounds of my *Quere* concerning it, will be much more briefly propos'd, the Experiment it self naturally suggesting this Problem; *How upon the mixture of two or three Bodies, such as those mentioned in the Paper, there should manifestly*

Problem. II.

2 Two Problems about Cold.

ensue a great and tumultuary agitation of small parts, and yet even during this conflict, not any sensible Heat, but a considerable degree of Cold be produc'd; and that even in the internal parts of the mixture?

The Inducements to make this Problem need not be far fetch'd, it being obvious enough, that, according to the Corpuscularian Philosophy, which you and I agree in, a brisk and various agitation of the minute parts of a Body is that, which makes it Hot both in reference to our Sensories, and to its operations on other Bodies. But I doubt, the rise of the Problem is much more easie to be understood, than the Cause of the *Phenomenon*, about which I will not ask you, Whether one may not assert, that Local motion is in its own nature a General thing, which may be so diversified by Circumstances, that one kind of Modification of it, as 'tis made in Corpuscles of several sizes and shapes, may be the cause of Heat, and another that of Cold? Or else, Whether we may suppose, that Cold is a positive thing, and operates by real Corpuscles of Cold, which happening to abound, and yet to be lock'd up in the Bodies whose mixture I employ'd, they are, by the great conflict that dissolves the Texture of the Clashing Salts, separately put into motion and that in such numbers, that though really there would be a Heat produc'd by the brisk and confus'd agitation of some of the parts, yet that Heat is not only conceal'd and check'd, but miter'd by the over-powering operation of the Frigorifick Corpuscles. But to ask you about this or any other particular way of solving our *Phenomenon*, were to forget, that my aim is to learn

Two Problems about Cold. 3

learn not your opinion of this or that particular Conjecture or fancy about our Problem, but in general, how it may be best resolv'd, and what you think to be the true Cause of so odd an Effect.

Having thus dispatch'd the little I had to say about the Paper that suggested the second Problem, I will now suppose that you have read the *Phenomena* that contain the rise of the first, to which I shall proceed without farther Preamble, since the Question or Problem, that these naturally call for, is, *Whence this vast force of freezing Water proceeds?* Problem I.

For, the breaking of resisting Bodies being to be made by a violent Local motion, and Cold, according to the Judgment even of the Moderns, either consisting in, or at least being accompanied with, a Privation, or a great Imminution of Motion, it seems very difficult to conceive how Cold should make Water to exert so wonderful a force. I know the Learned *Gassendus* and divers other Philosophers teach us, that Glaciation is perform'd by the entering of swarms of Corpuscles of Cold, as they call them, into the Liquor. But I much doubt, Whether from this Hypothesis a good Solution of our *Phenomenon* will be deriv'd, since these Atoms of Cold seem not barely as such to make that Expansion of the Water, which is requir'd in the Experiment by me recited. For I see that though Water will be more and more refrigerated, according as the Air grows colder and colder, yet till it be brought to an actual Glaciation, all the swarms of the Frigorifick Atoms in it, are so far from expanding it, that they more and more condense it. And even that degree of Cold which destroys Fluidity, though it expands Water,

4 Two Problems about Cold.

Water, does not do it merely by the multitudes of the Frigorifick Corpuscles that invade the Pores of the lately fluid Body, since pure Spirit of Wine and almost all Chymical Oyles, though expos'd to the same degree of Cold that turns Water into Ice, or as I have tryed, unto a far greater than is necessary to do so, will be but the more condens'd by those swarms of Particles. But, which is more considerable, I have carefully observ'd, that, besides common or exprest Oyls, Chymical Oyl of Aniseeds it self, being frozen or concreted by an intense degree of Cold, will not be expanded but notably condens'd, and accordingly grow specifically heavier than before. And this was one thing that kept me from expecting the removal of our Difficulty from the Ingenious Explication given of Freezing by the *Cartesians*, when they teach, that the Eel-like particles whereof they suppose Water to consist, are very remissly agitated, and their want of pliantness makes their Contexture less close; which yet seems not to agree with the lately mention'd Tryals. And though these Eel-like particles should lose all their flexibility, though in that case it may probably be said, that they would take up less room than before, if nothing oppose their Expansion, yet it does not thence appear, how they should acquire so vast a power to expand themselves in spite of Opposition, as we have shewn Water by Freezing does acquire.

I did not hope to resolve our Problem by the help of a Vulgar-Supposition, that well-stopp'd Vessels are broken in frosty weather *ob fugam Vacui*; since I found that Supposition to be erroneous by divers Experiments, some of which are mention'd in the History of Cold.

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Two Problems about Cold. 5

It seem'd less improbable, that some assistance to the solving of our difficulty might be given by two other things. Whereof the first is, That, for ought I have yet observ'd, no Liquor but Water, or that which participates of Water by having Aqueous Particles separable from it, will be made to swell by Cold; nor will Water it self do so upon every degree of Cold, but only upon so great an one as actually turns it into Ice. And the second is, That upon the Glaciation of Water and Aqueous Liquors, we may observe in the Ice many bubbles greater or smaller intercepted between the Solid parts, and suppos'd to be full of Air, (I say suppos'd, because upon tryal I found them to have yielded but a small proportion of common Air;) which supposition, if true, would perhaps invite one to suspect, that the Air contain'd in these bubbles might have an interest in our *Phenomenon*; since I have found by tryals purposely made, that Air congregated into Visible though not great portions, may exercise a considerable Elasticity, which appear'd not whilst 'twas invisibly dispersed through the Water.

And if I did not suppose, both *that* you had taken notice, that there are wont to be numerous particles of springy Air dispers'd through the Pores of Water; and *that* you had consider'd, *whether* the want of pliantness occasion'd by Cold in the Aqueous Corpuscles, whilst they are yet agitated and brandish'd by some permeating matter; and *whether* upon the change of the Pores, that we may conceive to be made in freezing Water, either by the recess of one sort of subtil Corpuscles or the admission of another, or the closer constipation of the grosser parts, there may not be produc'd in Corpuscles, that compose
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6 *Two Problems about Cold.*

Water, (to say nothing of the intermix'd Air, or the Concretions or the Coalitions occasion'd by the Cold,) a springiness capable to make many little Bodies, endow'd with it, exert a great force against the sides of the Vessel, that oppose their joynt endeavour to expand themselves: If, I say, I did not believe, that these and the like suspicions had occur'd to you as well as to me, together with the difficulties wherewith each of them seems to be incumber'd, I would acquaint you with what thoughts and tryals occur'd to me about these and the like conceits. But I not daring to think this could prove other than a needless work, I must remember, that my business in this Paper is to propose Difficulties, not the wayes of solving them; it being from your Kindness and Sagacity, that these are as well expected as desir'd by,

S I R,

Your, &c.

A N A T T E M P T
To Manifest and Measure the
G R E A T E X P A N S I V E F O R C E
O F
Freezing Water.

By the Honourable *ROBERT BOYLE.*



A N A T T E M P T

To Manifest and Measure the

GREAT EXPANSIVE FORCE

OF

Freezing Water.

Considering when I writ the *History of Cold*, that though divers *Phanomena* might induce an attentive Observer to think, that Freezing Water had an Expansive Force, yet I had not met with any that endeavour'd, or even propos'd, to measure it, whether because they reflected not on it at all, or judg'd not the Force considerable; I who look'd with other eyes upon it, thought fit to repair that omission, but was then so ill furnished with requisites for doing it fully, that I remember I complain'd of it in my *History of Cold*. And though, even afterwards, when the time of the year was favourable, I could not procure such Accommodations as my design exacted; yet thinking an imperfect way of Measuring to be better than none, I preferr'd to the making no attempt

2 *An Attempt to Manifest and*

tempt at all the endeavouring to do what the least defective Instruments, I could procure, would permit me, towards the making an estimate by known Measures, of the Expansive Power of Freezing Water. For though I did not expect, I should be able accurately to define it; yet I hop'd I should make such an estimate as to know that Force not to be, as one would think it, Faint and Contemptible, but very Great and Considerable.

I remember on this occasion, that to manifest the Force of Freezing Water, I caused the Barrel of a short Gun to have a skrew fitted to the Nose of it, by which we might exactly stop it, as we did the Touch-hole another way; then filling the Barrel with common Water, and closing it accurately by the help of the skrew, we laid it in a conveniently shap'd Vessel, wherein we incompass'd it with a Frigorifick Mixture (of Snow or Ice and Salt,) and in a short time we found, as we expected, the Barrel to be burst, part of the Ice appearing along the gaping slit that had been made in the Body of the Iron by the Freezing Water, which by this Effect seem'd to emulate the justly admir'd force of kindled Gun-powder. But the Design of this short Paper tending not so much to *prove*, as (in some sort) to *measure* the Expansive Force of Water, I shall subjoyn the Transcripts of two or three Experiments, made chiefly for that purpose.

EXPERIMENT I.

[There was taken a strong Cylinder of Brass, whose Cavity was two inches in *Diameter*; into this was put a Bladder of a convenient size, with a quanti-

Measure the Great Expansive &c. 3

quantity of Water in it, that the neck of the Bladder (which I had taken care to have oyl'd) being strongly tyed, the Water might not get out into the cavity of the Cylinder, nor be capable of expanding it self some other way than upwards. Then into this Cylinder was fitted a Plugg of Wood, turn'd on purpose, which was somewhat less in Diameter than the Cylindrical cavity, that it might rise and fall easily in it. Upon the upper part of this Plugg was laid a conveniently shap'd flat Body, upon which were plac'd divers weights to depress the Plugg, and hinder its being listed up by the Expansion-wont to be made in Water that is made to freeze; then a Frigorifick Mixture being afterwards apply'd to the Cylinder, it appear'd within half an hour or somewhat more, by a Circle that had been purposely trac'd on the side of the Plugg, where 'twas almost contiguous to the Orifice of the Cylinder, that the Water in the Bladder began to expand it self, and about two hours after, having occasion to shew the Experiment to some inquisitive persons, the circle appear'd to have been heav'd up in my estimate about $\frac{3}{8}$, if not half, of an inch, notwithstanding all the weights that endeavour'd to hinder the ascension, though these weights amounted to 115 pound, which were all the determinate weights we could then procure, besides a brick and some other things that were estimated at five pound more; nor did I doubt that a far greater Load would not have hindered its Expansion.]

EXPERIMENT II.

[We took a Brass Cylinder, whose Dimensions were three inches $\frac{8}{16}$ in Diameter, and in depth four inches. Into this we put a fine bladder of a convenient size, almost fill'd with Water, and strongly tyed about the neck; upon this bladder we put the wooden plugg to stop up the Orifice as much as was convenient, and upon the plugg we put a piece of a flat board for the weights to stand upon. These things being prepar'd, we convey'd the Cylinder with all that belong'd to it, save the board, into a large wooden Bowl, where we applied to the Cylinder a good quantity of the Frigorick Mixture, made with beaten Ice and Bay-Salt; and having first mark'd with a circular line the Edge, or Contact, where the orifice or lip of the Cylinder touch'd the Plugg, we layed on the weights upon the board, and when by their weight they had depress'd the Plugg till the cover of it lean'd upon the Cylinder, we dispos'd our selves to attend the issue of the Tryal. The event whereof was this, that when the action of the Frigorick Mixture had produc'd some Ice in the Water included in the Bladder, that Liquor appear'd to have dilated it self strongly enough to begin to raise the Plugg with the superincumbent weights, and by degrees they were by the growing Ice rais'd till the mark, diligently made on the Plugg where the edge of the Cylinder touch'd it, was about a tenth part of an inch above the station it had before the Plugg had been depress'd. Then we took out the Bladder, and found the Cylinder of Water within the Bladder not to
be

Measure the Great Expansive, &c. 5

be wholly turn'd into Ice, but to contain some quantity of unfrozen Water in the parts about the Centre, which Liquor, if we had not so soon deſiſted from the Experiment, (as for certain Reaſons we did) might probably have rais'd the weights ſomewhat higher. But as it was, the Ice in length was but three inches and about $\frac{1}{8}$, and yet ſo ſmall a quantity of Ice ſuffic'd to raiſe, beſides the board they lean'd on, as many weights of Lead as amount- ed to an hundred pound *Averdupois*.]

EXPERIMENT III.

[The day after the above mention'd Experiment was made, to try yet farther the Expansive force of Freezing Water, the ſame was reiterated after the manner above deliver'd, but with this difference, that, having procur'd more weight, when the Plugg was lifted up $\frac{1}{8}$ or ſomewhat better (which Plugg began ſenſibly to riſe within half or three quarters of an hour after the Frigorifick Mixture was apply- ed,) it was loaded with a weight of two hundred pounds, and a fifteen pound piece of Lead, and other Bodies, as Boards, &c. to lay the weights upon, which being alſo weigh'd by themſelves came to fifteen pound more, ſo that the whole amounted to 230 pound; and if the hundred pounds were both of them, as their bulk and ſhape invited us to gueſs, of that ſort of weights which are call'd the Greater Hundred, containing an hundred and twelve pound a piece, twenty four pound muſt be added to the ſumm, which would thereby be made up 254 pound.]

A NEW
EXPERIMENT
ABOUT THE
PRODUCTION of COLD
BY THE
CONFLICT of BODIES,
Appearing to make an EBULLITION.

By the Honourable *ROBERT BOTLE*.

An Advertisement of the Publisher's.

TIs manifest enough by the beginning of the following Paper, that 'twas not intended to come abroad alone, as indeed it was but a part of some Writings about Cold, design'd to enlarge the History of that Quality. But yet the Author forbore, by altering it, to accommodate it to the Papers wherewith it now comes forth; because in this very form it was by him (being to take a journey) left seal'd up with the Learned Secretary of the Royal Society in Febr. in the year 1663; since when it did, till lately, continue in those safe hands; the Author having no need to make use of it. Which Circumstances are now mention'd to keep the Reader from wondering, that the Author speaks of the Production he made of cold by the Conflict of two Liquors, as a New Experiment and Phænomenon; though now two or three years ago, the learned Sylvius, as he is inform'd, takes notice in one of his Books, of a way of producing Cold by a Mixture of Spirit of Vitriol, and another Saline Spirit. But besides that the Author's way is differing enough from Sylvius his, 'tis apparent by the time when his Experiment was left with Mr. Oldenburg, who is ready to bear witness to what is here said, that he had made it at least some years before the other, to which he was perfectly a stranger, nor hath yet ever tryed it, came abroad. Nor should he easily have look'd for the Production of Cold by the mixture of the Acid Spirit of Vitriol with every Volatile Spirit; because he found that the Oyl of Vitriol (as that Acid Liquor is commonly, but abusively, called) would by its conflict with Urinous Spirits, produce nor Cold but Heat. Whether the Care and Cautiousness, with which he made the Experiment now to be subjoyned, may give the Diffident and Curious more satisfaction than a bare Affirmation would do of his having produced Cold upon a mixture of contrary Bodies, he leaves it to others to judge. And I shall now only add, that he some years since shew'd some Curious Persons, that Cold may be made to result from the Conflict of Bodies, whereof none is a necessary Ingredient in the Experiment, from which, it may be, I have too long detain'd the Reader.



A N E W
E X P E R I M E N T
A B O U T T H E
P R O D U C T I O N O F C O L D
B Y T H E
Conflict of Bodies, appearing to make
an Ebullition.

AND now that we are searching after the Nature of Cold, I am put in mind that I have sometimes wondred at a certain Experiment that is so Anomalous, and seems so little of kin to the usual *Phenomena* of Cold, that though I do not particularly teach the way of making it, because I could not do it without discovering something in Chymistry, that cogent considerations forbid me at present to publish; yet I cannot forbear to relate, on this occasion, the matter of Fact, both because it may afford considerable Hints to sagacious Inquirers, and because it seems so little congruous to most Theories of the *Causes* of Cold, that it may make the Framers of Theories

2 *A New Experiment about the*

more wary, and help also to excuse my backwardness to propose *Hypotheses* about Cold in a resolute and confident way.

The Experiment is this: We took three Saline Bodies, each of them purify'd by the Fire; and whereas there are divers Bodies, that being mingled together acquire a Heat, which neither of them had apart; and whereas it is said by some that there are a few, which being blended together make a mixture somewhat colder than either of themselves, these Salts of ours being put together in due proportion, do upon their mixture produce that, which the Eye judges to be a great Effervescence; but though the hissing noise be loud, and though the numerous Bubbles suddenly generated will make the matter apt to overflow the Glass, if the one be not capacious, and the other be not put in by little and little; yet even whilst this seeming Ebullition lasts, the Glass, which one would expect to find very hot, (as usually happens upon the mixture of the Salt of Tartar, and Spirit of Nitre, and upon the confusion of the like Saline Bodies dispos'd to produce together such Efflorescencies) instead of growing hot, does, if it be held in ones hand, feel much cooler than before, and that in a wonderful degree; insomuch that ev'n in Winter the outside of the Glass would quickly be cover'd with great drops of Dew, which after a while would unite, and trickle down by their own weight. And this we could make to last for a great while, by casting in by degrees more and more of one of the Ingredients on the other. And besides that, this copious Dew on the outside of the Glass, reach'd as high as the mixture within, which argued whence it proceeded; be-

besides that, purposely looking on the bottom of the Glass whose outside was concave, we found no such drops of Dew there, because the Vapours of the External Air could not in any quantity have access to it; which shew'd the Dew, conspicuous elsewhere, not to come from the transudation of the finer parts of the Mixture through the pores of the Glass: Besides these things, I say, I remember, that having sometimes purposely wip'd off the Dew here and there with my Handkerchief, the dry parts of the Glass would in no long time regain fresh drops of Dew. And this odd Experiment we did for the main repeat not only in the presence of an Industrious Chymist, (whose Tryals unexpectedly gave us the Rise of the Experiment,) but also alone, and at differing seasons of the year.

I shall add, that having afterwards, about the middle of *November*, thought fit to vary a little, and repeat the Experiment, because I could then make use of a seal'd Weather-glass, which I had not at hand when I made the former Tryals; I took two deep Glasses, into the one of which I put a good quantity of fair Water, and in the other I made such a Mixture as I was lately mentioning; and having by a string, (to prevent the altering of the temper of the included Air by the warmth of my fingers) let down the Weather-glass into the Water, that the Liquor shut up in the Instrument might be cool'd by the ambient Water; after it had stay'd there a reasonable time, I took it out by the string that was fastened to the upper part of it, and letting it down into the mixture that was then hissing, and filling the Vessel that contain'd it with multitudes of successively emerging and hastily vanishing

4 *A New Experiment about the*

nishing bubbles; I perceiv'd nevertheless, that the coldness of the seemingly effervescent mixture made the imprison'd tincted Liquor to subside so low, that from four inches and three quarters (or thereabout) at which height it stood in the carefully divided stemm, when the Weather-glass was taken out of the Water, it fell in a short time lower than to one inch and a half. And because I foresaw that this might seem scarce credible, especially if I should relate how swiftly the imprison'd Liquor subsided at the beginning; I shall annex, that, for farther satisfaction of others, I remov'd the Thermometer out of the mixture into the common Water again, where it soon reach'd to somewhat above four inches and a half; and not content with that, I put it a second time into some of the frigeffective mixture before it had done foming, in which it fell, as before, somewhat below an inch and a half, and, presently after, almost as low as to an inch. And having once more put it back into the Glass that contain'd the Water, the included Liquor re-ascended to above four inches and a half, and this in an excellent seal'd Weather-glass, whose stemme was not in all above ten inches long, with a Ball proportionably big. And for farther confirmation, I took notice, that, whilst the mixture, by its hissing noise, and its strangely numerous Bubbles, seem'd to be in a state of Ebullition, the outsides of the Glass that contain'd it, were, as far as the mixture reach'd, so plentifully bedew'd with the condens'd Vapours of the ambient Air, that their weight carried them down in little streams which left round about the bottom of the Vessel a pretty quantity of Liquor, that appear'd by its taste not to have
been

been made by the transfusion of any of the sharp and Saline Liquors that were agitated within the Glass. There remain'd only one scruple, which was suggested to me by the remembering of a circumstance, which however, at the making of the fore-mention'd Tryals, I had not minded, and which possibly most Observers would have neglected; but calling to mind, that the Water, I had made use of to immerse the Weather-glass in, was brought out of a room wherein a Fire was wont (though not constantly) to be kept, whereas the Ingredients of the mixture were kept, and put together in a Chamber, which, though contiguous to the former, had no Chimney in it; I thought fit, for greater circumspection sake, to let the Water stand all night in this last-mention'd Chamber, that the Ambient Air might have the same Operation upon it, as upon those Bodies that were to be Ingredients of the mixture: And then repeating the formerly recited Experiment, though I thought it needless to spend time to watch, as before I had done, the greatest difference in Cold betwixt the Water and the bubbling Mixture; yet by making removes of the Weather-glass to and fro, from one Liquor to another, it sufficiently appear'd, that the greater coldness, remarkable in the mixture, did not before proceed in any considerable degree (if in any degree at all) from the Water's not having been kept in the same Room with it.

So that by these different Tryals it seems manifest, That the coldness of the mixture was not a Deception of the Sensory, since it would be discover'd by the operation, it had, not only upon the Vapours of the Air on ~~the~~ outside of the Glass, but
upon

6 *A New Experiment about the*

upon the Thermometer it self, plac'd in the midst of the mixture, which this last nam'd circumstance argues to have been cold throughout, and ev'n in its innermost parts.

And to shew, how much this strange coldness depended upon the peculiar Texture of the mixture, or the structure of its component Corpuscles, and the peculiar kind of motion that was excited in the tumultuating Particles; I shall here subjoyn a Relation which probably will not appear despicable; namely, That in the first place I took some of the acid Liquor, the rest of which I had made use of to make the mixture, whereof I have been speaking; and put a convenient quantity of fair Water, which had been kept a night or two in the same room (wherein was no Chimney) with it, that there might be no cause of suspicion, that the one had been expos'd to a more or less cold Air than the other; and yet these two Liquors did scarce sensibly differ in coldness; though to discover whether they did or no, I remov'd from one to another of them a good seal'd Weather-glass with a very slender stemm.

And in the next place, I took a convenient quantity of the pure Salt, I had so often employ'd, and cast it into a Glass full of Water, which I had kept many hours in the same room with it, and wherein I had a little before plac'd a seal'd Weather-glass, that the included Liquor might be brought to the temper of the Ambient Liquor; but upon this Injection, the tinted Liquor of the Thermoscope subsided so little, as not to make me look upon this Salt as being it self extraordinarily Cold, since other obvious Salts (that I have at other times
cast

Production of Cold, &c. 7

cast into Water to cool it a little) and ev'n Sea-Salt would (according to my Estimate) have refrigerated it as much, if not more. Nor did I observe the Glass, wherein I was wont to keep store of our Salt, (though I had often occasion to handle it) disclose to the touch any remarkable degree of Coldness; so that the coldness of our hissing mixture could not be attributed to that of either of the Ingredients apart, but was a Quality emerging upon their being blended. Now when I thus made these Preparatory Tryals, having afterwards plac'd in the same Window (of the Chamber last mention'd) a couple of Glasses, with common Water in one, and in the other some of that mixture, of whose frigeactive power I had very recently made Tryal; I left them to stand there together all night, and left also standing by them such a seal'd Weather-glass as I have been mentioning; and the next morning, when all the visible commotion or agitation of the minute parts of the contrary Salts of the Mixture was quieted, I put the Weather-glass first into one of those two Liquors, and then into the other, and after remov'd it back into the former again, without perceiving any difference worth minding betwixt the coldness of the mixture and that of common Water: And with much the like success I repeated the Tryal, after the Water and the other Liquor had stood in the same room (unfurnish'd with a Chimney) for near two dayes and nights.

And for farther confirmation, I shall add, that having instead of the Salt, which I hitherto made use of, taken some of the Spirit, that was wont to come over together with that Salt, and did so abound
with

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with it, that a good deal of it lay undissolved at the bottom of the Liquor; having, I say, employ'd this saline Spirit instead of the Salt it self, and having for Tryals sake mix'd with it another Spirit, drawn in my own Laboratory for the purpose, which to me seem'd as like, as could be made, to that which I had all this while made use of; I found, that the mixture of these two Liquors (though it produc'd far fewer Bubbles than I was wont to have) instead of growing Cold, grew Luke-warm, and quickly impell'd the Liquor in the Weather-glass, from a little above three inches, to as much above eight; and yet, besides that this last Spirit was, as far as I could perceive, and that after the same manner, drawn from the same Materials with that I had us'd all this while; the *Smell* and *Taste*, (which are both of them peculiar and odd enough) concurr'd to manifest the two Spirits to be of the same kind.

And, for farther proof, I shall add, that to satisfy my self the more fully, I took a parcel of the same Liquor, I had lately employ'd with success in making the Frigorifick Mixture, and yet ev'n this Liquor, which with the dry Salt would questionless have produc'd a Frigefactive Mixture as well as the rest had done, which I had a little before taken out of the same Viol; this Liquor (I say) put to a new portion of the Saline Spirit above-mention'd, though they did not produce minute Bubbles numerous enough to make a Fome; yet the Mixture, instead of growing very cold, grew manifestly Luke-warm, not only in the Judgment of the Touch, but by its Operation on a good seal'd Weather-glass, carefully and for a competent while employ'd to examine

amine the Temper of it. Whereas on the contrary, having purposely kept some of the Frigorifick Spirit by the Fire side, till its temper was so alter'd, that it nimbly enough rarified and impell'd up the Spirit of Wine contain'd in a seal'd Weather-glass, immers'd in it, and having into this Liquor cast some of the Frigorifick Salt, ev'n whilst the Spirit of Wine was rising, and would probably have risen a pretty while longer; this injected Salt, when it began to be dissolv'd, did not only give a check to the rising Liquor, and quickly put a stop to its ascent; but, (as I expected) soon made it subside again, till it fell about three inches or more (which was very much in a short Weather-glass) beneath the Station where the Spirit of Wine had rested, before the Liquor was set by the Fire side; nay, afterwards, I try'd, That a Frigorifick Salt, being well warm'd by the Fire side, did, with an appropriated Liquor, that was also warm'd, produce a coldness manifestly perceivable by the Weather-glass. So that in these cases a Body but moderately cold, nay actually warm, hastily reduc'd one, actually warm, or at least tepid, to a far greater degree of actual coldness than it self had.

These are some of the Experiments I try'd with the Liquors and Salts, of which, upon allowable Considerations, I must now forbear to set down the way of preparing: But that ev'n at present I may not be altogether wanting to the Curious, I devis'd a way of making a *Succedaneum* to this Experiment, which I shall here willingly annex, as that, which though it be much inferiour to what I may one day be at liberty to acquaint the Reader with; yet it will shew the main thing intended, by manifesting,

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festing, That Cold may by the mingling of Bodies be produc'd, or increas'd to a degree exceeding that of either of the Bodies that compos'd the Mixture; and this, though at the same time a seeming Effervescence be made by the Bodies, that thus refrigerate each other.

I took then very good Salt of Tartar, and putting to it a convenient quantity of Spirit of Vinegar, I did, whilst the mixture was hissing, (but seem'd to the touch to have refrigerated the Glass that contain'd it,) immerse into it the Ball of a good seal'd Thermoscope, furnish'd with Spirit of Wine. And, though the Weather-glass were not much above a foot long, yet the coldness of this Mixture made the Tincted Liquor descend, hastily enough, two inches and almost a half. And to shew farther, That this Mixture was actually colder than cold Water, removing the Weather-glass out of the Mixture into that Liquor, the tincted Spirit began to re-ascend, and that so nimbly, that in about three minutes (that the Ball of the Thermoscope stay'd under water) the Spirit of Wine had re-ascended about an inch and a half, if not more. And to try whether this coldness of the mixture did proceed from, or depend upon, some Texture of the parts, that was not very permanent, and yet did not quite degenerate, immediately after the Ingredients had ceas'd to work upon one another, I remember, that near an hour after the Ebullition of the Spirit and Salt of Tartar was over, the Thermoscope being remov'd out of the common Water, where it had stood immers'd, into the Mixture, descended about half an inch or more. For want of Salt of Tar-
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Production of Cold, &c. I I

tar I could not begin the Experiment anew, and so am not sure it will alwayes succeed uniformly. *

But yet to give my self what farther satisfaction I could, by trying the same Experiment in such a way as might discover, whether or no the *Phanomenon* did not depend upon, or require some peculiar *Texture*

** The Author's wariness was not here amiss, he having afterwards found, that this Experiment did not alwayes succeed.*

in the fix'd Salt that had been employ'd; I took some Alcaly (made by dissolving Pot-ashes in fair water, and reducing them by coagulation to a white Salt,) and pouring Spirit of Vinegar to it, I found, That this mixture did not, whilst it his'd, grow at all colder, but rather somewhat warmer. And, for farther satisfaction, immersing into it the Ball of the newly mention'd Weather-glass, I found, that it ascended in a short time about an Inch, and, being remov'd into the Water, descended about half an inch; and by making removes of it from one of these Liquors into the other two or three times more, I found, That the Spirit of Wine did rise and fall according to what has been newly observ'd, but its motions upwards and downwards were both less than before, and more slow.

F I N I S.

W

OBSERVATIONS

AND

EXPERIMENTS

ABOUT THE

SALTNESS of the SEA.

By the Honourable *ROBERT BOTLE.*

ADVERTISEMENT

To the following Observations (which may also serve for many Historical passages in the Author's other Writings.)

WHereas the Author does frequently make use of the Relations of professed Seamen and other Navigators, and of Observations made some in the East, and some in the West-Indies, it will be fit to advertize the Readers, that he has been very wary in admitting the informations that he employes; being forward enough to reject, as he has often done, such as many others would gladly have received: But notwithstanding his own rejection of the particulars he saw cause to disbelieve, it was easie for him to be well furnished with such relations as he makes use of; scarce any Writer of Philosophical things having had such opportunities of receiving such Authentick Informations from Sea Captains, Pilots, Planters, and other Travellers to remote parts, as were afforded him by the advantage he had to be many years a member of the Council appointed by the King of Great Britain to manage the business of all the English Colonies in the Isles and Continent of America, and of being for two or three years one of that Court of Committees (as they call it) that has the superintending of all the affairs of the justly famous East-Indian Company of England.



OBSERVATIONS
AND
EXPERIMENTS
ABOUT THE
SALTNESSE of the SEA.

THE FIRST SECTION.

Chap. I.

THe Cause of the Saltness of the Sea appears by *Aristotle's* Writings to have busied the Curiosity of Naturalists before his time; since which, his Authority, perhaps much more than his Reasons, did for divers Ages make the Schools and the generality of Naturalists of his Opinion, till towards the end of the last Century, and the beginning of ours, some Learned Men took the boldness to question the common Opinion; since when the Controversie has been kept on foot, and, for ought I know, will be so, as long as 'tis argued on both sides by Dialectical Arguments, which may be probable on both

2 Observations and Experiments

both sides, but are not convincing on either. Wherefore I shall here briefly deliver some particulars about the Saltness of the Sea, obtained by my own trials, where I was able; and where I was not, by the best Relations I could procure, especially from Navigators.

First then, Whereas the Peripateticks do, after their Master *Aristotle*, derive the Saltness of the Sea from the Adustion of the Water by the Sun-beams, it has not been found that I know of, that where no Salt or Saline Body has been dissolved in, or extracted by Water expos'd to the Sun or other Heat, there has been any such Saltness produc'd in it, as to justify the *Aristotelian* Opinion. This may be gather'd, as to the Operation of the Sun, from the many Lakes and Ponds of fresh Water to be met with, even in hot Countreyes, where they lye expos'd to the Action of the Sun: And as for other Heats, having out of Curiosity distill'd off common Water in large Glass Bodies and Heads till all the Liquor was abstracted, without finding at the Bottom the two or three thousandth part, by my guess, of Salt, among a little white earthy substance that usually remained. And though I had found a less inconsiderable quantity of Salt, which, I doubt not, may be met with in some Waters, I should not have been apt to conclude it to have been generated out of the Water by the Action of the Fire, because I have by several Tryals purposely made, and elsewhere mention'd, found, that in many places (and I doubt not but if I had farther tryed, I should have found the same in more) common Water, before ever it be expos'd to the Heat of the Sun or other Fire, has in it an easily discoverable Saltness.

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about the Saltness of the Sea. 3

of the nature of common Salt, or Sea-Salt; which two I am not here solicitous to distinguish, because of the affinity of their Natures, and that in most Places the Salt eaten at Tables, is but Sea-Salt freed from its Earthy and other Heterogeneities, the absence of which makes it more white than Sea-Salt is wont to be with us. *These last Words* I add, because credible Navigators have inform'd me, that in some Countreyes Sea-Salt without any preparation coagulates very white; of which Salt I have had, (from divers parts) and us'd some parcels.

But some of the Champions of *Aristotle's* Opinion are so bold as to alledge Experience for it, vouching the Testimony of *Scaliger* to prove, that the Sea tastes saltier at the top than at the bottom, where the Water is affirmed to be fresh. But as for the authority of *Scaliger*, though I take him to be an acute Writer, yet, I confess that, for reasons elsewhere given, I do not allow it that Veneration which I find given it by very Learned Men, nor am I over prone, even as to matters of Fact, to acquiesce in what he tells us, when he neither signifies that he delivers things upon his own Experience, or declares from what credible Information from others he received them.

'Tis true, that having often observed, that Sea-Salt dissolv'd in Water, is upon the recess of the superfluous Liquor, wont to begin its concretion, not as most other Salts do, at either the Lateral or Lower parts of the Vessel, but at the top of the Water, I will not think it impossible, that sometimes in very hot Climates or Weather, the Sea may taste more salt at the top, than at some distance beneath it. But considering how great a proportion of the

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Salt common Water is wont to be impregnated with before it suffers Saline Concretions to begin, and how far short of that proportion the Salt contained in the Sea Water is wont to be, insomuch that about *Holland*, a *Dutch* Geographer or two have not found it to amount to the proportion of one to forty, and I in *England* found it to be no more than I shall hereafter specify; it seems not likely that *Scaliger's* Observation was well made, and it must be very unlikely that it should generally hold, if the Saltness of the Superficial parts of the Sea be compared with that of the lower parts of it.

See the third Section, towards the latter end.

And yet I do not build my Opinion wholly upon this Argument of some Modern Philosophers, That Salt being a heavier body than Water, must necessarily communicate most Saltness to the lowest parts.

For though this Argument be a probable one, yet Water being a fluid body, the restless agitation of whose Corpuscles makes them and the Corpuscles they carry with them perpetually shift places, whereby the same parts come to be sometimes at the Top, and sometimes at the Bottom. This consideration, together with what was lately noted of the peculiar Disposition of Dissolved Sea Salt, to begin its Coagulation upon the surface of the Water, may make the Argument we are considering suspected not to be so cogent, as at first sight one may think it, Which suspicion I might somewhat countenance by subjoining, that in divers Metals, and other tinct Solutions, I have not usually observ'd the upper part of the Liquor to be manifestly deeper coloured, than the lower; though between

tween Metalline Bodies and their *Menstruums*, the disproportion of specifick gravity does usually much exceed that which I have met with, between Sea-Salt and Common Water.

CHAP. II.

Tis urg'd out of *Linscotten* by a Learned Modern Writer, that wanting fresh Water near *Goa* (the Metropolis of the *Portugals* in the *East-Indies*) they make their Slaves fetch it, by diving, from the bottom of the Sea, which seems a clear evincement of the *Peripatetick* opinion. But in this Observation I cannot acquiesce, for two Reasons: The one, because that though what is alledged as matter of Fact were strictly true, yet so general a conclusion could not be safely drawn from that particular instance, since in other parts of the Sea the contrary has been found by Experience, as I shall shew ere long. And other reasons than those given by the *Peripateticks* may be rendred of what happens at *Goa*, which reasons may extend to the like cases; if elsewhere they shall happen to be met with. For it may very well be, that Springs of fresh Water may arise in some parts of the surface of the Earth, that are cover'd with the Sea, as they do in innumerable Vallies and other places of the Terrestrial Surface that is not so covered. Not to mention those Springs that appear in divers places upon a low Ebb, cover'd with the Sea during the Flood. The Curious *Hungarian* *De Admirandis Hungarix Aquis.*

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count of the wonderful Waters that ennoble his Countrey, relates, that in the River *Vagns* that runs by the fortress *Galgotium*, the Veins of hot Water spring up in the bottom of the River it self.

Neque in Ripa tantum, sayes he, eruuntur calida, sed etiam intra amnem, si fundum ejus pedibus suffodias; calent autem immodice, &c. Nay, I have been assur'd by more than our Learned Eye-witness, that there is a place upon the *Neapolitan* Coast, where they (and I think a Writer or two of those parts) observ'd the Water to spring up hot beneath the Surface of the Sea, insomuch that one of my Relators (thrusting in his hand and arm somewhat deeper than was convenient, found there an offensive degree of Heat.

Besides, (which is my second conjecture) as to the particular case of *Goa*, I had the curiosity to enquire of a great Traveller, and a man of Letters, that liv'd in that City and the neighbouring places, and gave me a pertinent account of them, and especially of that place whence the fresh water is fetch'd by the Divers, which his Curiosity led him to visit, and take special notice of; but I found by him, that the Divers do not now think it needful to fetch their fresh water so low as from the bottom of the Sea, and that by the little depth, whence his and other mens curiosity caus'd it to be taken up, he judg'd it did not so much come from any fresh water Springs rising at the bottom of the Sea, as from a small River (whose name I do not remember) that not far from thence runs into the Sea, with such a juncture of circumstances, that at the mention'd places, the fresh water does yet keep it self
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tolerably distinct, and is not yet so far made brackish, as not to continue potable, though not very good. Which conjecture of his I could make probable, by what I have had from eminent and observing men among our own Navigators, touching the sliding of Waters one over another, in some parts of the Sea, especially near the mouths of Rivers. But the discussion of this matter, and the particulars of the Account given me of the scituation of the place where Water is div'd for near *Goa*, would require more words than they would in this place deserve, unless the point under debate were more important to our present purpose.

I might here pretend to a clear demonstration by experience of the contrary of what *Scaliger* delivers, by vouching the testimony of the Learned *Patricius*, who affirms, that being upon the Sea which takes its denomination from the Island of *Crete* (now *Candia*), he did, in the company of a *Venetian* Magistrate, *Moccenigo*, let down a vessel (furnish'd with a weight to sink it) to the bottom of the Sea, where, by the help of a contrivance, it was unstopp'd, and fill'd with Water there, which being drawn up, was found to be not fresh but Salt. This Experiment, I say, I could oppose as a Demonstration against *Scaliger*; but though it be a very probable Argument, and more considerable than any I have seen brought by the *Peripateticks* for their Opinion, yet I confess it would be more satisfactory to me, if it would not permit me to suspect, that in the drawing up of the Vessel through the Salt water, though there had been Fresh water taken in at the bottom, the taste may have been alter'd by the
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subingression of Salt water, which being bulk for bulk heavier than Fresh, would by its ponderousness endeavour to sink into the ascending Vessel, and thereby more easily expell part of the Fresh water, and mingle with the rest. Wherefore I shall confirm the Saltness of the Sea at the bottom by some Observations, that are not liable to the same Objections as that of *Patricius*.

The first is that of the Person, whom I elsewhere mention, to be able by help of an Engine to stay a considerable time at the bottom of the Sea; for of him I learn'd, among other things that I desir'd to be inform'd of touching that place, that he found the Water to have as Salt a taste there as at the top.

The next Observation I obtain'd by means of a great Traveller into the *East and West Indies*, who having had the curiosity to visit the famous Pearl-fishing at *Manar*, near the great *Cape of Comori*, answer'd me, that he had the same curiosity that I express'd to learn of the Divers, whether they found the Water Salt at the bottom of the Sea whence they fetch their Pearl-fishes? and that he was assur'd by them that it was so: And the same person being asked by me about the Saltness of the Sea in a certain place under the *Torrid Zone*, which the relation of a Traveller inclin'd me to think to abound extraordinarily with Salt, affirm'd to me, that not only the Divers assur'd him, that the Sea was there exceeding Salt at the bottom, but brought up several hard lumps of Salt from thence, whereof the Fishermen and others were wont to make use to season their meat, as he himself also did; which yet I may ascribe not only to
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the plenty of Salt already dissolv'd in the Water, but to the greater indisposition, that some sorts of Salts, whereof this may be one, have, to be dissolv'd in that Liquor.

To these I shall add this third Observation: Meeting with an inquisitive Engineer, that had frequented the Sea, and had several opportunities to make Observations of other kinds in deep Waters, I desir'd him that he would take along with him a certain Copper Vessel of mine, furnish'd with two Valves opening upwards, and let it down for me the next time he went to Sea; on which occasion he told me, that (if I pleas'd) I might save my self the trouble of the intended tryal, for, with a Tin Vessel very little differing from that I described unto him, he had had the curiosity near the Straight of Gibraltar's mouth, (where he had occasion to stay a good while) to fetch up Sea-water from the depth of about forty fathom, and found it to be as salt in taste as the Water near the Surface.

These Observations may suffice to shew, that the Sea is Salt at the bottom, in those places where they were made; but yet I thought it was not fit for me to acquiesce in them, but rather endeavour to satisfy my self, by the best tryal I could procure to be made with my Copper Vessel, (as more strong and fit than a Tinn one,) what Saltness is to be found in the Water at the bottom of our Seas, not only because it may more concern us to know that, but chiefly because, though I deny not, that in the fore-going Observations the taste may sufficiently prove that the Sea is Salt at the bottom as well as the top, yet I thought the taste,

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taste, by reason of the predispositions and other unheeded affections 'tis liable unto, no certain way to judge whether the top and the bottom be as Salt one as the other. Wherefore I thought it would be more satisfactory to examine the Sea-water by *weight* than by *taste*, and in order thereunto, having deliver'd the above-mention'd Instrument to the Engineer I lately spake of, when he was going to Sea, he sent me, together with it, a couple of Bottles of Sea-water, taken up, the one at the top, and the other at the bottom, at fifteen fathoms deep. The colour and smell of these two Waters were somewhat differing; but when I examin'd them Hydrostatically, by weighing a roul of Brimstone first in one, and then in the other, I scarce found any sensible difference at all in their specifick gravities. So that if the degree of the Saltness of Sea-water may be safely determined by its greater or lesser weight, then so far forth as this single Experiment inform'd me, the Saltness is equal at the top and bottom of the Sea: I said, *if the degree*, &c. because of what I shall hereafter take notice of about Salts of less specifick gravity than Sea-Salt.

CHAP. III.

It follows now that I make out, what I formerly intimated, That though it were granted, that near *Goa*, and perhaps in some other places, the Divers may have found the Water fresh at the bottom of the Sea, it would not therefore necessarily follow, that the Sea-water, generally speaking, is Fresh at the bottom; for the Observations lately mentioned sufficiently manifest the contrary: And as to those very few places (if really there have been any) where the Sea-water has been found Fresh at the very bottom, I think one may ascribe the taste of the Water to the bubbling up of Springs of Fresh Water, at, or near enough to, those very places. I know this may appear a Paradox, since it may seem altogether unlikely, that so small a Stream of Water as can be afforded by a Spring, should be able to force its way up in spite of the resistance of so vast a weight as that of the superincumbent Sea-water, especially since this Liquor by reason of its Saltness is heavier *in specie* than Fresh Water.

But this Objection needs not oblige me to forsake my conjecture; for whatever most men believe, and even Learned men have taught, to the contrary, it matters not how great the quantity of Liquor be, which is laterally higher than the Lower Office of the Pipe or Channel that gives passage to the Liquor that is to be impell'd up into it; provided the upper surface of the Liquor in the Channel or Pipe have a sufficient perpendicular height in reference to that
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of the stagnant Water; for no more of all this fluid will hinder its ascent, than the weight of such a

V. Stevinum prop. 10.
l. 4. Statices. And
See the Author's Hydrostatical Paradoxes.

Pillar of the said fluid as is directly superincumbent on it. Stevinus and I have by differing wayes particularly proved, that, according to the Laws of the true Hydrostatics,

the prevalency of two Liquors that press against each other, is not to be determined according to the Quantity of them, but to be adjudg'd to that which exceeds the other in (perpendicular) height; so that considering the Channel wherein a Spring runs into the Sea, as a long and inverted Syphon, if that part of the either neighbouring or more distant shore, whence the Spring or River takes its course, be a neighbouring Hill, or Rock, or any other place considerably higher, than that part of the bottom of the Sea (or of the shore cover'd with the surface of the Sea) at which the Channel, which conveys Fresh water, terminates, that Liquor will issue out in spite of the resistance of the Ocean.

To illustrate at once and prove this Paradox, I thought upon the following Experiment. I took a Vessel of a convenient depth, and a Syphon of a proportionable length, both of them of Glass, that their transparency might permit us to see all that pass'd within them. Into the larger Vessel we put a quantity of Sea-water, and into the longer leg of the Syphon, which had been for that purpose inverted, we pour'd a convenient quantity of Fresh water, which we kept from running out at the shorter leg, by stopping the Orifice of the longer with the

the thumb or finger : Then this Syphon being so plac'd in the greater Vessel, that the Orifice of the shorter leg was a great deal beneath the Surface of the Salt water, and the Superficies of the Fresh Water in the longer leg was a pretty deal higher than that of the surrounding Salt water, we unstopped the orifice of the upper leg, whereby the water in the Syphon tending to reduce it self to an *Equilibrium* (or equality of height) in both legs, the water in the upper leg being much higher and heavier than that in the other, did, by subsiding, drive away the Water in the shorter leg, and make it spring out at the orifice of the shorter leg, in spite of the breadth and specifick gravity of the Salt water. And this impelling upwards of the Fresh water lasted as long as the surface of that water in the longer leg retained its due height above that of the surrounding Sea water ; which circumstance I expressly mention, because there being a difference amounting to between a fortieth and fiftieth part betwixt the specifick gravity of our Sea water and common Fresh water, by reason of the Salt, which makes the former the heavier, the Fresh water in the longer leg of the Syphon ought to be between a fortieth and fiftieth part higher than the surface of the Sea-water ; to maintain the *Equilibrium* betwixt these two Liquors.

To make the fore-mentioned Experiment the more visible, I thought fit to perform it with Fresh Water ting'd with Brasil or Logwood ; but that it might not be objected, that thereby the specifick gravity of the Liquor would be alter'd or increas'd, I afterwards chose to make it with Claret Wine, which being a Liquor lighter than Common

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Water, and of a conspicuous colour, is very convenient for our purpose.

And when I made this tryal, by placing the Orifice of the shorter leg at a convenient distance below the surface of the Sea-water, 'twas not unpleasant to observe, how upon the removal of the Finger that stopp'd the Orifice of the longer leg, the quick descent of the Wine contain'd in that leg, impell'd the colour'd Liquor in the shorter leg, and made it spring up, at its Orifice, into the incumbent Sea-water, in the form of little red clouds, and sometimes of very slender Streams. And as this shorter leg of the Syphon was rais'd more, and more towards the surface of the Water, so there issued out more and more Wine at the Orifice of it; the Liquor in the longer leg proportionably subsiding, but yet continuing manifestly higher than the surface of the Salt Water, than which it was *in specie* much lighter.

¶ But here I must give an Advertisement to prevent a mistake; for if the Syphon be not exceeding slender, after the Wine in the longer leg is fallen down to its due station, a heedful Observer may perceive after a while, that though the Syphon be kept in the same place, there will issue out of the shorter leg a little red stream, which proceeds not from the former impulse of the Wine in the longer leg, but from the ingress of the Sea-water, which being much heavier *in specie* than Wine, sinks into the Cavity of the Syphon, and as it comes in on one side, thrusts up as much Wine on the other side of the same Cavity. But the red Liquor that ascends on this account may be discern'd to do so, by

by its rising more slowly, and after another manner than that which is impell'd up by the sudden fall of the tall Cylinder of Wine in the longer leg.

THE SECOND SECTION.

CHAP. I.

AS to the *Cause* of the Saltness of the Sea, I therein agree with the Learned *Gassendus*, and some other Modern Writers, That the Sea derives its Saltness from the Salt that is dissolved in it: But I take that Saltness to be supplied, not only from Rocks, and other Masses of Salt, which at the beginning were, or in some places may yet be, found either at the bottom of the Sea, or at the sides, where the Water can reach them; but also (to say nothing here of what may perhaps be contributed by subterranean Steams) from the Salt, which the Rains, Rivers, and other Waters dissolve in their passage through divers parts of the Earth, and at length carry along with them into the Sea. For not only 'tis manifest enough, that several Countreyes afford divers salt Springs, and other running Waters, that at length terminate their Course in the Sea, but I have sometimes suspected, that very frequently the Earth it self is impregnated with Corpuscles, or at least, Rudiments of common Salt, though no such thing be vulgarly taken notice of. Which suspicion may be

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confirm'd (to omit what I have elsewhere deliver'd on another occasion) partly by the Observation of some eminent Chymists, who affirm themselves to have found a not inconsiderable quantity of exceeding Saline Liquor upon the evaporation of large quantities of *some* Waters, (for in *some others* I could not find it,) and principally by the quantity of common Salt that is usually found in the refining of Saltpeter; though that be a Salt, which Sir *Francis Bacon*, and other experienc'd Writers teach, that almost every fat Earth kept from the Sun and Rain, and from spending it self in Vegetation, will afford.

But having on another occasion sufficiently shew'd, that the Earth does abound with common Salt in many more places than are wont to be taken notice of; and that 'tis probable, that by maturation, or otherwise, Salt may daily grow in the Earth, it will not be necessary to add in this place any thing to what I have said already to prove, that our Common Terrestrial Salt being dissolved, may suffice to make the Sea-water brackish; and the rather, if we call to mind what has been formerly said about the possibility of Springs rising beneath the surface of the Sea, and of Lumps of Salt that were taken up by *Divers*, undissolved, at the Bottom of the Sea; the Ocean may receive supplies of Salt from Rocks and Springs latent in its own Bosome, and unseen even by Philosophers. And this may be one Reason, I conceive, (for I deny not but that there may be others, as the very unequal heat of the Sun, &c.) why some Seas are so much saltier than others, or at least, why in some pla-

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places the Sea-water may be much Salter than in others.

And as we have seen, That our common Terrestrial Salt may be copiously enough communicated to the Sea, to impregnate it with as much Saltness as we observe it to have; so I do not see, that the difference between that Salt and Sea-salt is so great, but that it may well be suppos'd to be derived from those Changes that the Terrestrial Salt may be liable to, when it comes into the Sea. For that the Marine Salt and the Terrestrial do very well agree in the main things, may be argued from the resemblance both in shape, taste, &c. that may be observed between the grains that will be produced, if we expose each of them in a distinct Glass to such a heat, as may slowly carry off the superfluous Moisture, and suffer them to coagulate into Cubical or almost Cubical Graines: And the lesser differences that may be met with between these two Salts, may well enough be suppos'd producible by the plenty of Nitrous, Urinous, and other Saline, to which, in some places, may be added, Bituminous bodies, that by Land-floods and otherwise are from time to time carried into the Sea, and by several things that happen to it there, especially by the various agitation tis put into by Tides, Winds, Currents, &c. and (which I would by no means omit) by its being in vast quantities expos'd to the Sun and Air.

CHAP. II.

WE may justly be the more careful to determine, whether the Saltness of the Sea-water proceed from Common Salt dissolved in it, because if it appeared to be so, we might the more hopefully attempt to obtain by distillation Sweet water from Sea-water; since, if this Liquor be made by the bare dissolution of Common Salt in the other, 'tis probable, that a separation may be made of them, by such a heat as will easily raise the Aqueous parts of Sea-water, without raising the Saline, whole Distillation requires a vehement Heat, as Chymists well know to their cost. And such a method of separating Fresh water from that which was Salt, would make our Doctrine of use, and be very beneficial to Navigation, and consequently to Mankind. For in long Voyages, 'tis but too common for the makers of them, to be liable to hazards and inconveniencies, for want of Fresh and Sweet water, whereby they are sometimes forced to drink corrupt brackish Water, which gives them divers Diseases, as particularly the Scurvy, and, the usual effect of drinking Salt water, the Dropsie. And Sea-men are wont to receive so many other incommodities by the want of Fresh water, that, to prevent or supply it, they are oftentimes forced to change their course, and sail some hundreds of miles to a Coast, not only out of their way, but unsafe in it self, and perhaps more dangerous, by being infested by Pyrats, or in the hands of Enemies or Savage people; by which meanes they often lose the benefit of their

their *Monsoons*, and much more easily other Winds, and frequently their Voyage. And these are inconveniencies, which might be in good measure prevented, if potable, and at least tolerably wholesome Water, could be obtain'd by Distillation, in the midst of the Sea it self, to serve the Sea-men till they could be supplied with naturally Fresh water. To make some tryals of this, I remember I took some *English* Sea-water, whence I was able to separate betwixt a thirtieth and fortieth part of dry Salt, and having distilled it in a glass head and body, with a moderate fire, till a considerable portion of it was drawn over, we could not discern any Saltness in it by the taste; and besides that I found it specifically lighter than such Water as is daily drunk by Persons of Quality at *London*, I expos'd it to a more Chymical Examen, and did not by that find any thing of Sea Salt in it, though I have at several times, by the same way, manifestly discovered a Saltness in in-land Waters, that are drunk obviously for sweet Waters. If I would have employed a stronger Heat, and Vessels larger and lower, or otherwise better contriv'd for copious Distillation, I might in a shorter time have obtain'd much more distill'd Water; but whether such Liquors will be altogether so wholesome, Experience must determine. Yet that Sea-water distill'd even in no very artificial way, may be so far wholesome, as not in haste to be sensibly noxious, but at a pinch useful, at least for a while, may be gathered from (what occurs to me since the writing of the last Paper) the Testimony of that famous Navigator, Sir *R. Hawkins*, who commanded a Fleet in the *Indies* for *Queen Elizabeth*. For he, in the Ju-

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dicious Account he gave the World of his Voyage, wherein they were distressed, even in the Admirals ship, for want of Fresh Water, has this memorable passage (as I find it verbatim in our diligent Purchase.)

In Lib. 7. page 1378. of Purchase; out of Sir R. Hawkins his Voyage.

Although our fresh water had fail'd us many dayes (before we saw the shore) by reason of our long Navigation without touching any Land, and the excessive drinking of the Sick and Diseas'd (which could not be excus'd;) yet with an invention I had in my Ship, I easily drew out of the Water of the Sea sufficient quantity of Fresh water, to sustain my people, with little expence of felv's; for with four bittles I still'd a hoghead of Water, and therewith dress'd meat for the Sick and Whole. The Water so distill'd we found to be wholesome and nourishing.

And because the potibleness of Sea-water may concern the Healths and Lives of men, I shall here add, to what I eliewhere deliver about my wayes of examining, whether other waters participate of Salt, two or three Observations I made upon those few distill'd Liquors, I had occasion to draw from Sea-water. Having then upon some of the distill'd Liquor dropt a little oyl of Tartar *per deliquium*, I perceiv'd no clouds at all, or precipitation to be made, whereas a small proportion of that Liquor being dropt into the undistill'd Sea-water it self, it would presently trouble and make it opacous, and, though but slowly, strike down a considerable deal of a whitish substance (which, of what nature it is, I need not here declare;) I found also, that a very small proportion of an Urinous Spirit, such as that of *Sai Armoniac*, would produce a whitish and curl-

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ed substance (but not a near so copious one as the other Liquor) in Sea-water, not yet expos'd to Distillation, but not in the Liquor drawn from it: which argued, that there were but few or no saline particles of Sea-salt ascended with the Water: For else these Alcalizate and Urinous Salts would in all likelihood have found them out, and had a visible operation on them. And I farther remember, that when the Distillation was made in Glass Vessels, with an easie Fire, not only the first running, but the Liquor that came over afterwards, was not perceiv'd to be brackish, but good and potable. To which agrees very well, that by a Hydrostatical Tryal I found our distill'd Sea-water to be lighter *in specie* than common Conduit Water, though it exceeded *that* in specifick Levity, less than 'twas surpass'd in the same quality by distill'd Rain-water.

But to return to the Subject whence we have somewhat, but, I hope, not uselessly, digress'd; I know it may be objected, that if the Terrestrial Salts carried by Springs, Rivers, and Land-floods into the Sea, were the cause of its saline Taste, those Waters themselves must be made Salt by it, before they arrive at the Sea. But besides, that this Objection will not reach the Springs and Rivers of Salt water, that in several places, either immediately or mediately, discharge themselves into the Sea; it might conclude against him that should affirm this imported Saltness to be the only cause of that of the Sea: But it will not be of force against me, who take it to be only a partial cause, that by its accession contributes to the degree of Saltness we observe in the Sea, where this imported Salt
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may joyn it self with the Salt it finds there already, and being detained by it, contribute to the briny-ness of the Water.

If it be urg'd, that from hence it will follow, that the Sea from time to time increases in Saltness, I may suspend my answer till it appear by competent observation that it does not; which, I think, men have not yet made tryals that may warrant them to assert. And if the matter of fact were certain, I think 'twere possible to give a farther answer, and shew probable wayes, how so small an accession of Salt may be dispers'd by nature, and kept from increasing too much.

C H A P. III.

BUT now 'tis seasonable to consider, that the taste of Sea-water is not such a simple saline taste, as Spring-water would receive from *Salt Gemm*, or some other pure Terrestrial Salt dissolved in it, but a bitterish taste, that must be derived from some peculiar cause that Authors are not wont to take notice of. I am not assur'd by any Observations of my own, that this recession from a purely Saline taste is likely to be of the very same kind, and to be equally, or very near equally, met with in all Seas; (not to add a doubt whether it be at all sensible in some.) The cause both of the bitterness and saltness too of the Sea-water, is said to be affirmed by Learned Mr. *Lidiat*, to be adust and bituminous

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luminous Exhalations ascending out of the Earth into the Sea. But that there is abundance of a *Actual Salt* in the Sea-water, to give it its Saline taste and ponderousness, the Salt, that the Sun does in many places copiously separate from the Saltless waterish parts, sufficiently manifests. But as to the bitterish taste, I think it no easie matter to give a true account of it, but am prone to ascribe it *partly* to the operation of some *Catholick Agents* upon that vast body of the Ocean, and *partly* to the Alteration that the Salt receives from the mixture of some other things, among which *Bitumen* may be one of the principal.

But though I have in another Paper shewn, that in some places of the Sea there are considerable quantities of *Bitumen*, or Bituminous matter to be met with; yet I dare not derive the bitterness of the Sea only from Bituminous Exhalations, but in good part, at least, in some places, from the liquid and other Bitumen, that is imported by Springs and other Waters into the Sea; of which we have an eminent instance in that which our *English* call *Barbadoes Tar*, according to the relation I had of it from an inquisitive Gentleman, who is one of the chief Planters of the Island, and took pleasure to observe this liquid Bitumen to be carried in considerable quantities from the Rocks into the Sea; and I think it possible enough, that some of the Springs that rise under the surface of the Sea, may carry up with them Bituminous matter, which may help to make the Saltness of the Sea degenerate, (of which more perhaps elsewhere;) as I not long since made mention of Springs, as well of hot

In the Tract of
*Subterranean Men-
struums.*

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as cold water, rising beneath the surface of the Sea. And this minds me to intimate here, that I have suspected, that in some places the Sulphureous Exhalations, and other emissions from the submarine parts of the Earth, may sometimes contribute to change the saline taste of the Sea-water: For I have elsewhere related, how not only Sulphureous Steams, but sometimes Actual Flames have broken through from the lower parts of the Sea to the uppermost; and have sometimes taken pleasure to make by Art a rude imitation of that *Phænomenon*. And partly some Experiments of my own, and partly some other Inducements, have perswaded me, that divers times (for I do not say alwayes) Sea Salt does not obscurely participate of Combustible Sulphur, of which I may speak farther on another occasion. But in regard that the taste of the Sea-water is not in all parts of the Ocean uniform, it may here suffice to take notice in general, that this difference of taste may partly be caus'd by adventitious bodies of several kinds, of which 'tis probable, that in differing places the Sea-water does variously partake. And not to mention here the fragrant smell of Violets, which has by several, and particularly by an Eminent Person, of whom I enquired about it, been observed, in some hot Countries, to proceed from Sea Salt; I have divers other Inducements to think that it is usually no simple Salt, nor free from mixture. For by more ways than one, and particularly by cohobating from it its own Spirit, we have obtained a dry Sublimate, which seem'd to be no Pure, but a Compounded Body.

And

about the Saltness of the Sea. 25.

And now to come to that which I intimated might be one of the causes, why the taste of Sea-water is not the same with that of Common Salt dissolved in Fresh water; I shall add, that I have suspected, that the various motion of the Sea, and its being exposed to the action of the Air and Sun, may contribute to give it a taste other than Saline; which suspicion might be confirmed by the Observation I elsewhere mention of the Sea Salt, which, by barely being expos'd for many months to the Air, and sometimes perhaps put into a gentle agitation by a digestive Heat, I found to have a very manifestly differing taste from the simple Solution of Sea Salt in Common water.

I might here endeavour the farther confirmation of my Discourse, by what I have learned by inquiry from Navigators, about the manifestly differing Colours, and other Qualities of the differing Parts of the Sea, which seem to argue, that 'tis not every where of such a Uniform Substance as men vulgarly imagined, and that vast Tracts of it are imbued with stupendious multitudes of adventitious Corpuscles, which, by several wayes diversifying its parts, keep it from being a simple Solution of Salt. But of this Subject I have not leisure to discourse here, only because 'tis generally thought, that the Sea-water is, by reason of the Saltness it abounds with, uncapable of Putrefaction; I will add, That having kept a pretty quantity of Sea-water, that I had caused to be purposely taken up between the *English* and *French* shores, in a good new rundlet, in a place where the Summer Sun beat freely upon it, it did, in a few weeks, acquire a strongly stinking smell; though, that the Experiment had
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been more satisfactory, I wished that it had been made in a Vessel of Glass or Earth, instead of Wood. But a much better Observation I procur'd from a much esteemed Navigator of my acquaintance, who having sailed often in the *Indian* and *African* Seas, I enquired of him, whether he had ever in those hot Climats, where the Sea is supposed to be very Salt, observed it to stink, for want of Agitation or otherwise: To which he answer'd, That once being, though it was but in *March*, becalmed, in a place he named to me, for 12 or 14 dayes, the Sea, for want of motion, and by reason of the Heat, began to stink, insomuch that, he thinks, if the Calm had continued much longer, the stench would have poysoned him: They were freed from it as soon as the Wind began to agitate the Water, and broke the Superficies, which also drove away store of the Sea Tortoises, and a sort of Fish, whose *English* name I know not, that before lay basking themselves on the top of the Water.

And to this agrees very well the notable Observation, that I since met with, of the elsewhere commended Sir *R. Hawkins*, who, among other considerable things he takes notice of in his Relations,

Purchase's Pilgrims
in Sir *R. Hawkins*
Observations.

has this passage, to our present purpose. Were it not for the moving of the Sea by the force of Winds, Tides, and Currents, it would corrupt all the world. The Experience I saw Anno 1590, lying with a Fleet about the Islands of Azores, almost six months, the greatest part of the time we were becalmed; with which all the Sea became so replenish'd with several sorts of Gellies, and forms of Serpents, Adders and Snakes, as seem'd wonderfull.

Some

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some green, some black, some yellow, some white, some of divers colours, and many of them had life; and some there were a yard and a half, and two yards long, which had I not seen, I could hardly have believed. And hereof are witnesses all the company of the Ships which were then present, so that hardly a man could draw a bucket of water clear of some corruption. In which Voyage, towards the end thereof, many of every Ship fell sick of this Disease, and began to dye apace, but that the speedy passage into our Country was a remedy to the crazed, and a preservative for those that were not touched.

THE THIRD SECTION.

CHAP. I.

AS for the various Degrees of the Saltiness of the Sea, Authors are wont to be silent of it, save that some Navigators tell us, that they observed some Seas to have a more, and others a less Saline taste; which you will easily believe has not afforded me much satisfaction. And on the other side, my want of opportunity to make Tryals my self, will confine me to acquaint you with no more than the few following Observations.

I. To a Learned man that was to sail to places of differing Latitudes in the *Torrid Zone*, I deliver'd a Glass Instrument, elsewhere described, fitted by the greater or lesser Emerision of the upper part, to shew,
accu-

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accurately enough for use, the greater or less Specific Gravity of the Salt Water it was put to swim in. This he put from time to time into the Sea-Water, as he sailed towards the Indies, whence he wrote me word, *That he found, by the Glass, the Sea-water to increase in weight, the nearer he came to the Line, till he arriv'd at a certain degree of Latitude, as he remembers, it was about the thirtieth; after which, the Water seem'd to retain the same Specific Gravity, till he came to the Barbadoes or Jamaica.*

2. Another Observation I obtain'd by Inquiry of an Ingenious Person and a Scholar, at his return out of the *East Indies*, who affirm'd to me, that he, and a Gentleman of my acquaintance, took up Bottles full of Sea-water, both under the *Equinoctials*, and also off the *Cape of good Hope*, which lies in about 34 Degrees of Southern Latitude, and found the Waters of these distant parts of the Ocean to be of the same weight. And though it may well be doubted, whether this Observation, being made with ordinary Bottles, were so exact as could be wish'd, yet the Persons being curious, and making it for their own satisfaction; and my Relator having, in both the recited places, fill'd with the Sea-water he took up and weigh'd, having, I say, fill'd the same Bottles; since this Vessel held two quarts, (which must be above four pounds of Salt-water,) if the disparity of weight had been considerable, it would in likelihood have been found, at least manifestly sensible in such a weight of Liquor:

3. Inquiring of an observing Person, that had been at *Misambique*, which is thought to be one of the hottest places in the World, whether he did not

not there find the Sea to be more than ordinarily Salt; he answered me, that, coming thither in a great Carack, when he came back from the *Towu* to the Ship, he observ'd near two hands breadth of the Vessel to be above the ordinary part, to which it used to sink; insomuch that he took notice of it, to the Captain, as fearing that part of the lading had been by stealth carried to the shore: But the Pilot, who had made thirteen or fourteen Voyages to the Indies, assur'd him, what he had observ'd about the Ship was not unusual in that place, where the taste it self discover'd the Water to be exceeding Salt.

Nor need we scruple to think, that some Sea-Waters may be very much more impregnated with Salt than ours; for Water will naturally dissolve, and retain a far greater proportion of Salt, than that which is commonly met with in the Sea. For whereas a thirty fifth, or thirtieth, or at most a twenty fifth part of Salt will make Water more Saline than is found in many Seas, I am by a Friend of mine that is Master of a Salt-work, inform'd, that the Water of his Springs afford him a twelfth part of good White Salt, and that another Spring not far off, yields no less than an eighth part. To which, (to avoid anticipation) I shall not here add, what I shall hereafter have occasion to say of the fullest impregnation of Water with Common Salt.

[Whilst I was reviewing these Papers, there came seasonably to my hands a Letter written from *Musilapatan*, on the Gulf of *Benjala* in the *East-Indies*, by an ingenious Gentleman, Sir *William Langborn*, that is intrusted with the care of the Eng-
lish

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lish Factories in those parts; out of which Letter the following passage is *verbatim* transcribed.

“ I did, in order to your command, cause some Water to be saved under the Line, at our first access to it, intending, for want of good scales and weights, (being none to be come at aboard the Ship) to have kept it until it could be weighed, but by the forgetfulness of a servant, it was thrown away. Off the Cape, in 37 d. 00 m. Southern latitude, I saved some again, and through the same want of weights, was fain to keep it until I came to the Line again; and then made the best shift I could for weights, and compar'd it with the Water there, filling the same Bottle again to the same height by a mark, and found it exactly the same weight. The weight I have taken; but accounting this a journey of business, left those notes, and most of the like nature, behind me; in my next it shall be inserted.]

C H A P. II.

IT remains now, that, according to my promise, I set down what I observed my self concerning the Saltness of our Sea between *England* and *France*; not in comparison with the Saltness of other Seas, whose Waters I had not to compare with, but as to the proportion of Salt contained in it to the Water. And though one would think it very easie to make tryals of this sort for a person not

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about the Saltness of the Sea. 31

unacquainted with Hydrostatical practices nor unfurnished with Instruments, yet, I confess, that three or four tryals that I made, not all of them the same way, made me find it more difficult than was imagin'd to arrive at any thing of certainty in this inquiry.

This you will easily believe, if I annex the substance of some Experiments, that, I remember, I made about the gravity of Sea Water, which I had order'd to be taken up, some at the depth of about fifteen Fathom somewhat near our shoar, and some in another place of the Channel between *England* and *France*.

The sum of the first Experiment is this: We took a Vial, fitted with a long and strait neck, purposely made for such tryals, and having counterpois'd it, fill'd it to a certain height with common Conduit water: We noted the weight of that Liquor, which being poured out, the Vial was fill'd to the same height with Sea Water, taken up at the surface, and by the difference between the two weights, the Sea water appear'd to be about a forty fifth part heavier than the other.

The second Tryal (which was for more accurateness made Hydrostatically,) I find register'd to this effect: We carefully counterpois'd in the Scales, formerly made use of, a piece of Sulphur in the upper Sea water, formerly mention'd; it weigh'd $3\beta + 10 \frac{1}{2} gr.$ and being also weigh'd in the Sea water fetch'd from the bottom, gave us the same weight $3\beta + 10 \frac{1}{2} gr.$ which shew'd those two Waters to be of the same Specifick Gravity: And then to compare this with the gravity of common Water, we weigh'd the same Sulphur in com-

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mon Conduit Water, and found it $3\beta + 15\frac{1}{2}$ gr: By which it appear'd, that the Sea-water was but about a fifty third part heavier than this Water: which is such a difference from the proportion found out by the former way of tryal, that I could not well imagine what to attribute it to, unless the Sea-water by long standing in a Vessel, which, though cover'd, was expos'd to the hot Sun, may both have been rarified, and have had some separation made of its Saline or other heavier parts, on which score that portion we took up for our tryal, might appear lighter than else it would have done; or unless, the Experiment having been made in *London*, where great and sudden rains and other accidents will sometimes visibly vary the consistence of common Water, the Liquor, I then employ'd without examining it, might be more ponderous at that time than at another. To which latter suspicion I was the more inclin'd, because, having afterwards weigh'd the same piece of Sulphur by help of the same balance in distill'd rain water, I found the weight of the former liquor to exceed that of the latter by a good deal less than a thirty fifth part; which seem'd to make it probable, that if the Water, we chanc'd to employ, had been free from all Saline and other heavy particles, the difference formerly mention'd betwixt this Observation and the fore-going would not have been near so great as it was.

The last way I made use of to examine the proportion betwixt Sea-water and Fresh, was Chymical; whereof my Register affords me this account.

A pound (*Haverdupois* weight) of the upper Sea-water, was weigh'd out, and put into a head and body

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body to be distill'd in a digestive furnace *ad siccitatem*, and the Distillation being leisurely made, the bottom of the glass was almost cover'd with fair grains of Salt, shot into Cubical figures, and more white than was expected; in the rest of the coagulated matter we took not notice of any determinate shape. The Salt being weigh'd amounted to 3β , *Haverdupois*, and 10 *gr.* At which rate the proportion of the Salt to the Water will be that of 30 and $\frac{72}{1000}$ to one, and so will amount to near the thirtieth part; which was so much greater than the former wayes of tryal made us expect, that I know not whether it may not be worth while to try, whether such a slow abstraction as we employ of the superfluous Water, and our doing it in close Vessels, may not have afforded us more Salt than else we should have obtain'd.

To this Relation I find this note subjoyn'd: Suspecting that there may have somewhat else concurr'd to our finding so great a proportion of Salt, I suffer'd that, which had been weighed, to continue a while in the Scale, and soon perceiv'd, that, according to my conjecture, that scale began manifestly to preponderate, and that consequently some of the unexpected weight of Salt may be due to the moisture of the Air, imbib'd after the Salt was taken out of the Glass, and laid by to be weighed: Wherefore, causing it to be very well heated and dried in a Crucible, we found it to weigh $3ii. + \beta$. (that is 210 *gr.*) upon which account, the proportion of Salt contain'd in the Water was a thirty sixth part, and somewhat above half of those parts, and to express it in the nearest whole number, a thirty seventh part.

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From whence this greater proportion of Salt by Distillation, than our other Tryals invited us to expect, proceeded, seems not so easie to be determined; unless it be supposed (as I have sometimes suspected) that the Operation, the Sea-water was exposed to in Distillation, made some kind of change in it, other and greater than before-hand one would have look'd for; and that, though the grains of Salt we gained out of the Sea-water, seem'd to be dry before we weigh'd it, yet the Saline Corpuscles, upon their concreting into Cubes, did so intercept between them many small particles of Water, as not to suffer them to be driven away by a moderate warmth, and consequently such grains of Salt may have upon this account been less pure and more ponderous than else they would have been. And I might here add, that I sometimes make a certain Artificial Salt, which though being dissolv'd in Water, it will shoot into Crystals finely shaped, and dry enough to be reducible into powder, yet coagulates Water enough with it to make the Water almost, if not quite, as heavy again as before. And I have been assured by a very Learned Eye-witness, that there is a sort of Sea Salt, which they bring to some parts of *England* from the Coast of *Spain* or *Portugal*, which being here dissolved, and reduced by Purification and Filtration to a much whiter Salt, will yield by measure somewhat above two Bushels for one. But to satisfy the scruples and suspicions I could suggest, would require more Tryals than I have now time or opportunity to make. What has been already deliver'd, may give at least as scrupulous an account of the Saltiness of our *East-Ind* Sea-waters, as most other Experimenters would have

have thought it needful to give. And to make a determination with any certainty about the degrees of the Seas Saltness *in general*, a great number of Observations, made in different Climates and in distant parts of the Ocean, would be necessary.

CHAP. III.

I Know not whether I may be so indulgent to my suspicions, as to wish, that Observations were heedfully made, Whether in the same Sea, and about the same part of it, the Waters be alwayes equally Salt? For, though that be taken for granted, yet since we have no good Observations long since made to silence the suspicion, one may suspect, that, at least in many places, the Saltness of the Sea may continually, though but very slowly, increase by the accession of those Saline Corpuscles that are imported by Salt-Springs, and those which Rivers and Land-floods do from time to time rob the Earth of. And I suspect it to be not impossible, that this or that part of the Sea may be sometimes extraordinarily, and perhaps suddenly, impregnated with an additional Saltness from Saline steams plentifully ascending into it, from those Subterranean Fires, about which I have made it elsewhere probable, that they may burn beneath the bottom of the Sea, and sometimes send forth copious Exhalations into it. But it may prove the more difficult to discern

*In the Tracts of
Subterranean Fires
and Steams.*

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this adventitious Saltness, unless the taste as well as ballance be employed about it; because the Salt, that produces it, may be of such a Nature as to be much lighter *in specie* than common Sea Salt. And the mention of this leads me to give you here the Advertisement I promised you not long ago.

That though the weight of Sea-water be as good a way as is yet employ'd (and better than some others) to determine what Sea-water does most abound in Salt; and though it be possible, that in our Sea, and perhaps in almost all others, this way be not liable to any considerable uncertainty; yet I think it not impossible, that it may sometimes deceive us, especially in very hot Regions; because I have observed, that there may be Volatile Salts, which, though by reason of their activity they make smart impressions on the tongue, and give the water imbued with them a strong Saline taste, yet they add very little, and much less than one would think, to its Specifick gravity; as I have tryed, by Hydrostatically examining Distill'd liquors, abounding in Volatile and Urinous Salts, some of which I found very little heavier than Common Water, and consequently nothing near so much heavier as they would have been made, if they had been brought to so sharp a taste, by having nothing but common Sea Salt dissolved in them: So that, if in any particular place, by any other way, or from the Steams of the Earth beneath, (some of which, I elsewhere shew, may be very analogous to those afforded by *Sal Armoniack*) the Sea should be copiously impregnated with such kind of light Salts, the Sea-water may be much more salt to the taste, and yet be very little heavier. For confirmation of which I find among my notes, that weigh-

about the Saltness of the Sea. 37

weighing a seal'd buble of Glass, made heavy by an included Metal, first in Spirit of *Sal Armoniack*, that tasted much stronger than Sea-water, it weighed $3\text{ij} + 51 \frac{1}{4} \text{gr.}$ and weighing this same body in fair Water, it weighed but $3\text{ij} + 45 \frac{3}{4} \text{gr.}$ so that notwithstanding its great Saltness, the Spirit was lighter than Common water; though a good part of that comparative Levity may probably be ascribed to the Liquor wherein the Saline Particles swarm, which, by Distillation, was grown more defecated and light than Common, though clean, Water.

But for a farther proof, we took a hard lump of *Sal Armoniack*, and though we could not weigh it in Water, because that would have dissolv'd part of it, yet by a way (I elsewhere teach) I found, that weighing in the same Liquor this lump of *Sal Armoniack*, and a lump of good white Sea Salt, (brought me as a Curiosity out of the *Torrid Zone*) the proportion of the latter to a bulk of the Liquor equal to it, was something (though exceeding little) above that of two and a quarter to one, and the proportion of *Sal Armoniack* to as much Water as was equal likewise to it, did not above a Centesm exceed that of one and $\frac{7}{8}$ to one; which falls so short of the other proportion as may justly seem strange, especially if it be considered, that the factitious *Sal Armoniack*, the Chymists generally use, and we employ, consists in good part of Sea Salt, which abates much of the Comparative Levity it might have, if it were made up only of Urinous and Fuliginous Salts, which were its other ingredients.

It were indiscreet for me to propose any more suspicions and tryals fitted, clear them. I
knew

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knew those I have already mention'd would not pass for Extravagancies; and therefore I should here dismiss the Subject of this Tract of the Saltness of the Sea, but that since I have been discoursing of the degrees of it, it will not be impertinent to add, what is the greatest measure of Saltness that I have brought Water to, without the help of external Heat. On this occasion I employed two differing wayes, the one was by putting into a well-counterpoysed Vial two Ounces of Common-water, and then putting into it well dryed and white common Salt, and shaking them together till the Liquor would, *whilst cold*, dissolve no more: This Liquor, thus glutted with Salt, weighed 1150 grains, from which two Ounces being deducted, the overplus of weight, arising from the dissolved Salt, amounted to 190 gr. so that a parcel of Salt will without heat be dissolved in about five times its weight, or very little more, of common Water. By which proportion we made so strong a brine, that divers pieces of Amber, being purposely let fall into it, emerged, and floated on it. The other and better way, yet more tedious, that we made use of, was, to let Sea-Salt run *per deliquium*, (as the Chymists speak) that is, to set it in some moist place, till it was dissolved by the Aqueous Vapours that swim in the Air. In this Liquor we weighed a piece of Sulphur, which we also weighed in Sea-water, wherein, finding it to weigh much more than in the former Liquor, it appeared that the Sea-water was *in Specie* much lighter than the other; though how much their gravities differ'd, I cannot find among my Notes, nor be informed by my Memory.

And

about the Saltness of the Sea. 39

And because I have not in any Author met with the proportion of Sea Salt to Water of the same bulk, nor perceive that Hydrostaticians themselves have yet attempted any way to investigate it, (probably deterr'd by the easie dissolubleness of Salt in Water) I shall here subjoyn , that by the help of an Expedient I have elsewhere taught, I have examin'd a hard dry lump of Sea-Salt , and found its proportion in weight to common Water of the same bulk, to be *almost* as 2 to 1, (for it exceeded the *ratio* of $1 \frac{2}{10}$ to 1.) And, I remember, I found the Specifick Weight of a hard and figur'd lump of *Sal Gemm* (which sort of Salt, I suppose to be somewhat *more* pure and ponderous than Sea Salt) to be to that of Water (very near) as $2 \frac{1}{2}$ to 1.

F I N I S.

THE FOURTH SECTION

Belonging to the Tract formerly Pub-
lish'd under the Title,

*Relations about the Bottom of
the S E A.*

By the Honourable *ROBERT BOYLE.*

ADVERTISEMENT
TO THE
READER.

THis Section should have been subjoyned to the Relations about the Bottom of the Sea, when that Discourse was printed, together with some other Tracts at Oxford, An. 1671. but was by the Negligence of him, that should have carried it to the Press, severed from the rest of that Tract, and not seasonably deliver'd to the Printer.



THE FOURTH SECTION

Belonging to the Tract intitl'd,

Relations about the Bottom of the
S E A.

THe presence of the Air is not only so necessary to the Life of many sorts of Animals, but it hath likewise so great a stroke in the growth of Vegetables, especially of the larger sorts, that, after what I had experimented about these matters, (of which this is not the proper place to give an account) I thought fit to make enquiry about the Vegetation and growth of Plants of considerable Bulk in those submarine Regions, where if there grow any, they must do it remote from the free contact of an ambient Air. And having not now the leisure to repeat what *Botanists* (of whose Books I am not now provided) deliver about lesser Plants growing under Water, I shall now onely present you with what information I could procure from Navigators, about Trees and Fruit growing at the bottom of the Sea.

To what I have elsewhere had occasion to say to their Opinion, that will not allow Coral to be really a Stony Plant, but a Liveless Concrete, that is always hard and brittle under Water; I shall now add, that, inquiring lately of an Eminent and Inquisitive Person, that had spent some time upon the Coast of *Africa*, where he had been present at the fishing of Coral, and learning from his answer, that he had seen it not far from *Algiers*; I ask'd him, whether he had himself observ'd the Coral to be soft, and not red, when 'twas newly brought from the bottom of the Sea. To which, he replied, that he had found it soft and flexible; and that, as for the colour, it was for the most part very pale, but with an eye of red, the Bark being worse coloured than the substance it cover'd was; but when the Bark was taken off, and the other part expos'd to the Air, the expected redness of the Coral disclos'd it self.

When I demanded, whether he had observed, that any inky sap ascended to nourish the stony Plant? and whether he had seen any thing like Berries upon it? He ingenuously confessed to me, he had not been so curious as purposely to make inquiry into those Particulars; but that he remembred, That having broken some of the large pieces of Coral, he took notice, that the more internal Substance was much paler than the other, and very whitish, and that at the extream parts of some branches or sprigs he observ'd little blackish knobs, which he did not then know what to make of: And when I enquir'd, what depth the Sea was of in that place? he answer'd, that 'twas nine or ten fathom. But as to the Fruit of some
kinds

kinds of Coral, if I do not much misremember, I was, not long since, assured by a Scholar that navigated much in the East, that they divers times meet with in those Seas a certain sort of Coral, but not white, which bears a small Fruit like a round Berry, of a pleasant colour, and esteem'd as rarities.

Discoursing with a person that made *Diving* his Trade, whether he had not met with any Trees or Fruit in the depths of the Sea? He told me, that in a great Ship, whereinto he descended, to recover thence some shipwreck'd Goods, he was surpriz'd to find in several places a certain sort of Fruit, that he knew not what to make of; for he found them of a slimy and soft consistence, about the bigness of Apples, but not so round in shape, and when he brought them up into the Air, as he did many of them, they soon began to shrink up like old rotten Apples, but were much harder, and more shrivel'd. And 'tis remarkable, that this happen'd in a cold Northern Sea.

One that made a considerable stay about *Manar*, a place I have often mention'd, answer'd me, that he learn'd from the *Divers*, that in some places thereabouts there grows at the bottom pretty store of a certain sort of Trees, bearing Leaves almost like those of Laurel, as also a certain Fruit; but of what virtue, or other use, he had not the Curiosity to enquire.

I was also inform'd by an Eye-witness, that near the famous Coast of *Misambique* in *Africa*, there grows at the Bottom of the Sea store of Trees, that bear a certain Fruit, which he describes

to be very like that, which in *America* they are wont to call *Acayu*, the Leaves also resembling those of that Tree.

But the welcomest Information I could procure about Sub-marine Plants, is that which concerns the famous *Maldivian* Nut, or *Coco*, which is so highly esteem'd in the East, that some write, it is a great Present from one King to another, and even much extoll'd in *Europe* by experienc'd Physicians: For the Origine of this dear Drug is almost as much controverted as the Alexiterial Virtues are extoll'd. Having then once the good fortune to meet with a man of Letters, that had resided in those unfrequented Islands, I found* he had been as inquisitive as I could reasonably expect about these admir'd Productions of the Sea, and that he had often learn'd from the *Divers*, that they are real Nuts or Fruits born by a sort of *Coco*-Trees that grow at the Bottom of the Sea, and are thence either torn off by the agitation of the Water, or gather'd by the *Divers*. These Fruits are smaller than most other sorts of *Coco's*, whose maturity they do not seem to arrive at. He thinks, the *Species* may have been very differing from what it is, and may have come from Nuts fallen into the Sea, together with the ruine of some little Islands undermin'd by the Water, and so submerg'd; of which, he told me, he saw at least three or four instances during his stay there. He told me, that whilst the Fruit was under Water, they observ'd no distinct shell and kernell, but the entire Nut was so soft, that it may be easily enough cut with a knife, and was eaten like their other Fruits; but being kept about a Week in

Bottom of the Sea. 5

in the hot Air, it grows solid, and so hard as to require good Steel Tools to work upon it. He added, that though even upon the place the fairest sort be of very great esteem, yet not of any such prodigious price as is given out. And he presented me one about the bigness of a large Egg, and a Fragment of another, which are both very hard; but as for their Virtues, I can yet say nothing upon Tryal, for want of having had fitting Opportunities.

Other Observations made at the Bottom of the Sea may hereafter follow.

F I N I S.

A
P A R A D O X
O F T H E
N A T U R A L
A N D
P R E T E R N A T U R A L S T A T E
O F
B O D I E S,
E s p e c i a l l y o f t h e A I R.

By the Honourable *ROBERT BOYLE.*

A N
A D V E R T I S E M E N T.

AN attentive Reader will easily be persuaded by a couple of passages in the following Papers that it is only a Fragment. But though the Author, for certain Reasons, has for divers years suppress'd the other Discourses that belong to the same Treatise, yet he will content to let this come abroad without them; not only because, relating chiefly to the Air, it may fitly be conformed with those Papers concerning some Qualities of the Air, which it accompanies; but because 'tis hoped it may prevent, or put an end to, several unnecessary Disputes about the Natural and Foreign Constitution of the Air (warmly agitated among Learned men) by shewing them to be founded, some upon precarious suppositions, and more upon Vulgar Mistakes.



O F T H E
 N A T U R A L
 A N D
 P R E T E R N A T U R A L S T A T E
 O F
 B O D I E S,
 E s p e c i a l l y t h e A I R.

I Know, that not only in Living, but even in Inanimate, Bodies, of which alone I here discourse, men have universally admitted the famous *Distinction* between the *Natural* and *Preternatural* or Violent state of Bodies, and do daily, without the least scruple, found upon it *Hypotheses* and Ratiocinations, as if it were most certain, that (what they call *Nature*) had purposely framed Bodies in such a determinate state, and were always watchful that they should not by any external Violence be put out of it.

2 Of the Natural & Preternatural

But notwithstanding so general a consent of men in this point, I confess, I cannot yet be satisfied about it in the sence wherein it is wont to be taken. 'Tis not that I believe, that there is *no sence*, in which, or in the account upon which, a Body may be said to be in its *natural* state; but that I think the *common* Distinction of a *natural* and *violent* state of Bodies has not been clearly explained, and considerably settled, and both is not well grounded, and is oftentimes ill applyed. For, when I consider, that whatever state a Body be put into, or kept in, it obtains or retains that state according to the Catholick Laws of Nature, I cannot think it fit to deny, that, in this sence, the Body propos'd is in a *natural* state; but then, upon the same ground 'twill be hard to deny, but that those Bodies, which are said to be in a *violent* state, may also be in a *natural* one, since the violence, they are presumed to suffer from outward Agents, is likewise exercised no otherwise than according to the established Laws of Universal Nature. 'Tis true, that when men look upon a Body as in a *preternatural* state, they have an *idea* of it differing from that which they had whilst they believ'd it to be in a *natural* state: But perhaps this difference arises chiefly from hence, that they do not consider the condition of the Body, as it results from the Catholick Laws settled among things Corporeal, and relates to the Universe, but estimate it with reference to what they suppose is convenient or inconvenient for the particular Body it self. But however it seems to me, that mens determining a Body to be in a natural or preternatural state has much more in it, either of casual, or of arbitrary, or both, than they are aware of. For oftentimes we think

think a Body to be brought into a violent state, not because really the former was not so, but because there is a notable change made in it by some Agent, which we also take notice of; whereas before the action of that Agent, if the Body were under any violence, 'twas exercis'd by usual, but often immanifest Agents; though perhaps their Compulsion were not *less*, but only *less heeded*. And sometimes also no more is to be understood by a Bodies being forc'd from its Natural state, than that it has lost that, which it had immediately, or a pretty while before some notable change. Which Conjectures I shall now endeavour to confirm, but with great brevity.

I have already shewn, that Matter being devoid of sense and appetite, cannot be truly and properly said to *Affect* one state or condition more than another, and consequently has no true desire to continue in any one state, or to recover it when once lost; and Inanimate bodies are such, and in such a state, not as the material parts they consist of, elected or desired to make them, but as the natural Agents, that brought together and rang'd those parts, actually made them. As a piece of Wax is unconcern'd, whether you give it the shape of a Sphere, or a Cone, or a Pillar, or a Boat; and whether, when it has that form, you change it into any other; the matter still retaining without willingness or unwillingness, because without perception, that figure or state which the last action of the Agents (your fingers or instruments) determined it to, and left it in.

But this will be best understood, as well as confirmed, by particular examples. I need not tell you, that

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that the most usual instance alledg'd to shew, that a state is natural to a Body, and that being put out of it by external causes it will upon the cessation of their violence be restored thereunto, is, That Water being heated by the Fire, as soon as that adventitious Heat vanishes, returns to its native coldness; and so when, by an excess of Cold, it is congeal'd into Ice, it does upon a thaw lose that preternatural hardness, and recover the fluidity that naturally belongs to it: And the same may be likewise said of Butter, which, being melted by external Heat into a Liquor, does upon the cessation of that Heat grow a consistent body again. But perhaps these instances will rather countenance our *Paradox* than disprove it. For as to the coldness whereto Water heated by the Fire returns when 'tis remov'd thence, it may be said, that the acquired Heat consisting but in the various and brisk agitation of the Corpuscles of the Water by an external agent, it need be no wonder, that when that Agent ceases to operate, the effect of its operation should cease too, and the water be left in its former condition, *whether* we suppose it to have been heated by the actual pervasion of the Corpuscles of the Fire, which by degrees fly away into the Air; or that the Heat proceeds from an agitation imparted by the Fire to the Aqueous Corpuscles, which must by degrees lose that new agitation, by communicating it little by little to the contiguous Air and Vessel; so that, if the former agitation of the particles of the Water, were, as is usual, much more languid than that of our Organs of Feeling, in which faintness of motion the coldness of Water consisted, there will be no need of any positive internal form,

form, or any care of Nature to account for the Waters growing cold again. This will be confirm'd by the consideration of what happens to Ice, which is said to be Water brought into a preternatural state by an excess of Cold. For, I doubt, 'twill not be easily demonstrated, that in reference to the nature of things, and not to our arbitrary *ideas* of them, Ice is Water preternaturally harden'd by Cold, and not Water Ice preternaturally thaw'd by Heat. For if you urge, that Ice left to it self will, when the Frigorifick agents are removed, return to Water; I shall readily answer, that, not to mention the Snow and Ice that lyes all the Summer long unthawed upon the tops of the Alps and other high mountains, I have learn'd, by inquiry purposely made, from a Doctor of Physick, who for divers years practis'd in *Muscovy*, that in some parts of *Siberia* (a large Province belonging to the *Russian* Emperour) the surface of the ground continues more Months of the year frozen, by what is call'd the natural Temperature of the Climate, than thaw'd by the Heat of the Sun; and that a little beneath the surface of the ground, the Water, that chances to be lodged in the cavities of the Soil, continues frozen all the year; so that, when in the heat of Summer the Fields are covered with Corn, if then you dig a foot or two, perhaps less, you shall easily find Ice and a frozen Soil: So that a man born and bred in the inland part of that Country, and inform'd only by his own Observation, may probably look upon Water as Ice violently melted by that Celestial Fire, the Sun, whose heat is there so vehement in their short Summer, as to ripen their Harvest in less time than in our Temperate Climates will easily be credited.

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On the other side we in *England* look upon melted Butter, as brought into a violent state by the Operation of the Fire, and therefore think, that when being remov'd from the Fire it becomes a consistent Body again, it has but recover'd its Native Constitution. Whereas there are divers parts of the *East Indies*, and, I doubt not, of other hot Countries, whose Inhabitants, if they should see consistent Butter (as sometimes by the care and industry of the *Europeans* they may do) they would think it to be brought to a preternatural state, by some artificial way of Refrigeration. For in those parts of the *Indies* I speak of, (though not in all others) the constant temper of the Air being capable to entertain as much of agitation as suffices for fluidity in the parts of what in our Climate would be Butter, 'twould be in vain to expect, that, by being left to it self in the Air, it should become a consistent Body. And I have learn'd by diligent inquiry of Sea-men and Travellers, both *English* and others, that were Eye-witnesses of what they told me, that, in divers parts of those hot Regions, Butter, unless by the *Europeans* or their disciples purposely made in the Cold, is all the year fluid, and sold, or dispens'd, not as consistent Bodies, by weight, but as Liquors, by measure. To strengthen this Observation, I shall add, what was affirm'd to me by a Learned man, that practis'd Physick in the warmer parts of *America*, namely, that he met in some places with several Druggs, which, though they there seem to be Balsoms, as Turpentine, &c. are with us, and retain'd that consistence in those Climates, yet when they come into our colder Regions, harden into Gums, and continue such both

Win-

Winter and Summer. On the other side, inquiring also of a Traveller, vers'd in Physical things, about the Effects of great Heat in the in-land part of *Africa*, where he had lately been; he told me, among other things, that Raisin of *Jalap*, which, when he carried it out of *England*, was of a consistence not only dry but brittle, did, when, and a while before, he came to *Morocco*, melt into a substance like Turpentine; so that some of it that he had made up into Pills, would no more at all retain that shape, but remain as it were melted all the while he stayed in that City, and the neighbouring Countrey, though when he came back to the borders of *Spain*, it return'd to its former consistence. Which I the less wonder'd at, because, having had the curiosity to consider some parcels of Gum *Lacca*, (of which Sealing Wax is made) newly brought ashore from the *East Indies*, though it be a hard and solid Gum, yet I found by several instances, that, passing through the *Torrid Zone*, divers pieces of it, notwithstanding the shelter afforded it by the great Ship it came in, had been, by the Heat of the Climate, melted, and made to stick together, though afterwards they regain'd their former Consistence; though not altogether their former Colour. And on this occasion I shall add, that I learn'd by inquiry from a particular acquaintance of mine, who brought me divers rarities out of *America*, that having at the place where 'twas made, among other things, furnished himself with a quantity of the best *Aloes*, he observed, that whilst he sail'd through very hot Climates, it was so soft, that, like liquid Pitch, it would often have fallen out of the wide-mouth'd Vessel he kept it in, if he had not from
time

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time to time been careful to prevent it. But when he came within a hundred Leagues of the Coast of *England*, it grew hard, and so continued, though this were in a very warm season of the year, being about the Dog-dayes.

For further confirmation of what has been hitherto discoursed, be pleased to consider with me that most obvious Body, the *Air*, or the Atmosphere we live and breath in. For though several Opinions and Argumentations are founded upon what their Authors call the Natural and Preternatural or Violent state of the *Air*, yet he that considers, shall find it no easie thing to determine, what state of the *Air* ought to be reputed its truly Natural state, unless in the sence I formerly told you I employ that expression in. I will not insist on the Heat and Coldness of the *Air*; for, that being manifestly very differing in the heart of Winter, and in the heat of Summer, and in differing Regions of the *Air*, as at the top and bottom of high mountains, at the same time, and constantly in differing Regions of the Earth, as in *Barbary* and *Greenland*, 'twill not be so easie to determine what state is natural to the *Air*. But that only which I shall now consider, is its state or tone in reference to *Rarity* and *Density*. For, since the *Air* is believed to be condensed by Cold and expanded by Heat, I demand, at what time of the year, and in what Countrey, the *Air* shall be reputed to be in its *Natural state*? For, if you name any one time, as the Winter, or the Summer, I will ask, why that must be the standard of the tone of the *Air* rather than another Season, or at least exclusively to all others? And the like difficulty may be made about the Climate

mate or the Place. And these scruples are the more allowable to be propos'd, because Learned men have deliver'd, that in some Countreyes the *Mercury* in the *Toricellian* Experiment, is kept higher than in others, (as in *Sweden* than in *Italy*,) and our *Baroscopes* inform us, that oftentimes, in the same place and day, the *Quick-silver* in the same Instrument does considerably vary its height; which shews, that the Air or Atmosphere must necessarily vary its weight, and therefore probably its degree of Rarity or Density.

But I have yet to propose a further Consideration in this Affair: For, what if it shall appear, that neither in Winter nor in Summer, in *Sweden* or in *Italy*, or in whatever Country, Region, or Season you please, the Air we breath in is in any other than a Preternatural state; nay, that even when we have vehemently agitated and expanded it by an intense heat of the Fire, it is not yet violently rarified, but yet violently constipated, unless in our sense before declared, you understand with me the Preternatural state of Rarefaction in the Air, in reference to the tone it had before the last notable change was produc'd in it. This will, I question not, seem a surprizing, if not a wild, Paradox: But yet to make it probable, I shall only desire you to reflect upon two or three of my *Physico-Mechanical* Experiments; and there you will see, first, that the Air being a Body abounding with springy particles, not devoid of Gravity, the inferiour must be compress'd by the weight of all the incumbents. And next, that this Compression is so great, that though by the heat of the Fire neither others nor we could bring a portion of included Air to be expanded

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panded to above fourscore times its former space; yet without heat, by barely taking off the pressure of the superiour Air, by the help of our Pneumatical Engine, the Air was rarified more than twice as much: And since those Experiments were published, I more than once rarified it to above five hundred times its usual Dimensions; so that, if according to what is generally agreed on and taught, a Body be then in a Preternatural state, when by an external force it is kept in a condition, from which it incessantly tends to get free; and if it be then most near its Natural state, when it has the most prosperously endeavoured to free it self from external force, and comply with its never-ceasing tendency; if this be so, I say, then the Air we live in is constantly in a Preternatural state of Compression by External force. And when it is most of all rarified by the Fire, or by our Engine, its Springs having then far more conveniency than before to display themselves, which they continually tend to do, it answerably approaches to its Natural state, which is to be yet less compressed or not at all. And I have carefully try'd for many months together, that when the Air has been rarified much more than even a vehement heat will bring it to be, yet if it were fenc'd from the pressure of the external Air; it would not shrink to its former dimensions, as if it had been put into a violent state, from whence Nature would reduce it to them, but continued in that great and seemingly preternatural degree of extension, as long as I had occasion to observe it. One might here shew, that this odd constitution of the Air is so expedient, if not necessary for the Motion, Respiration, and other uses of Animals, and in particular

ticular of men, that the Providence and Goodness of the Wise Author of the Universe is thereby signally declared; if it were not improper in such a Paper as this to imploy *final* Causes. Wherefore to avoid the imputation of impertinence, I will conclude, by taking notice that from what has been delivered we may learn two things considerable enough, if not in themselves, yet to some passages of the Treatise, whereof this Paper makes a part. And first, we may deduce from what has been said of the Air, that according to what is noted above, That may sometimes generally be granted and believ'd to be the Natural state of a Body, not which it really affects to be in, or (to speak more properly) has a tendency to attain, but that which it's brought into and kept in by the action or resistance of neighbouring Bodies, or by such a concurrence of Agents and Causes as will not suffer it to pass into another state. And the second thing we may hence learn is, that whatever men say of Natures never missing her aim, and that nothing violent is durable; yet, bating an inconsiderable Portion of Aerial particles at the upper surface, for ought we know the whole mass of the Air we live in, and which invirons the whole Terraqueous Globe, has been from the worlds beginning, and will be to its end, kept in a state of violent Compression.

F I N I S.

A STATICAL
Hygroscope

Proposed to be further tryed,

Together with

A BRIEF ACCOUNT

OF THE

Utilities of HYGROSCOPES.

By the Honourable *ROBERT BOYLE*,
Fellow of the *Royal Society*.



L O N D O N,
Printed by *E. F.* for *R. Davis*, Bookfeller
in *Oxford*, 1673.



A
STATICAL HYGROSCOPE

Proposed to be farther tryed,
In a Letter to *H. Oldenburgh* Esq;
Secretary to the
ROYAL SOCIETY.

S I R,

Though I writ to you from *Stanton* an account of those Hygroscopes, whereof I now present you one; yet, since I remember that it was in the year 1665 that I sent you that Paper, I fear you may by this time have forgotten much of what it contain'd, and thereby made it fit for me in this Letter, both to remind you of some former passages, and to add some Observations that lately occur'd to me; and this the rather, because I do not present you with this trifle meerly to gratifie your Curiosity, but that you
and

2 *A Statical Hygroscope proposed*

and some of your ingenious friends may, by your remarks, help me to discover to what inconveniences our Instrument is liable, how far they may be avoided or lessen'd, or what the uses or advantages of it may be, notwithstanding its inevitable inconveniences or imperfections.

Having had occasion amongst other subjects relating to the Air, to consider its Moisture, and its Dryness, I easily discern'd that they had no small influence upon divers Bodies; and among the rest, upon those of men, as the ambient Air we breath in, either passes from one of those Qualities to the other, or even from one degree to the other in the same quality.

Wherefore I began to cast about somewhat solicitously for a way that might better than any I had yet tryed, or elsewhere met with, discover the changes of the Air as to moisture and dryness, and the degrees of either quality. For which purpose it seem'd to me, that, if a Statical Hygroscope could be had, it would be very convenient, in regard of its fitness, both to determine the degrees of the moisture or dryness of the Air; and to transmit the Observations made of them to others. Whereupon considering further, that among Bodies otherwise well qualified for such a purpose, that was likeliest to give the sensiblest informations of the changes of the Air, which, in respect of its bulk, had the most of its surface expos'd thereunto, I quickly pitch'd upon a fine Sponge, as that which is easily portable, not easie to be divided or dissipated, which, by its readiness to soak in Water, seem'd likely to imbibe the Aqueous particles that it may meet with dispers'd

spers'd in the Air, and which, by its great porousness throughout, has much more of *Superficies* in reference to its bulk, than any Body not otherwise less fit for the intended use that came into my thoughts.

If you recall to mind, when and whence I first gave you notice that I employed our little instrument, you will easily believe, that the Inducements I had to pitch upon it, were, that I should need but such light and parable things, as I could easily both procure in the Country (where I then was) and carry about with me in the frequent removes. I was obliged to make; and therefore that I did not represent this trifle as the best Hygroscope that could be devised, or even as the best that perhaps I my self could have propounded, if I would have fram'd an elaborate Engine with Wheels, Springs, or equivalent Weights, Pullies, *Indices*, and other contrivances, some of which I divers years ago made use of. For I little doubt, but that Mechanical heads may frame Hygroscopes much curiouseer and perfecter than that I now send you, or any other I have used or seen, if they may be accommodated with sufficient room, and dextrous Artificers that will work exactly according to directions; whereas my design being not so much to make a *Machinal* or Engine-like, as a *Statical*, *Hygroscope*, and such an one as may be simple, cheap, contained and set up in a little room, easie to be made and transported, I thought it might be of some use, especially to those that are not furnished with Curiosities and Mechanical Accommodations, if among the several forms of Hygroscopes that I had in my mind, I chose one, that being *statical* and easie, might be as

47 *A Statical Hygroscope proposed*

commodious by its simplicity, as some others by their elaborateness; especially if we consider, that, as slight an instrument as it seems, it may be applied to various uses, some of which are not slight, as will ere long be made probable.

If I should be here told by one, that grants the preferableness of Statical Hygrosopes in the general, that there are *divers Bodies*, other than that pitch'd upon by me, whose weight may vary when the Temperature of the Air is considerably altered as to dryness and moisture, and that perhaps among these, some one may be found that may imbibe the Aqueous particles of the Air better than our Sponge; I shall not resolutely deny it, and therefore shall leave you to make tryals with what other Bodies you shall think fit, contenting my self to have suggested in general the conveniency of making Hygrosopes, where the differing changes of the Air may be estimated by weight; but this I shall tell you in favour of our Sponge, that when I was considering, what Bodies were the fittest to be employed for the making of Statical Hygrosopes, I made tryal of more than one that seem'd not the least promising. I know, that Common or Sea-Salt will much relent in moist Air, and Salt of Tartar will do it much more; but then those Salts, especially the latter, will not so easily as they should, part with the Aqueous Corpuscles they have once imbibed, and are in other regards, (which 'twere not worth while to insist on,) less convenient than a Sponge. I made tryal also with Lute-strings, which were purposely chosen very slender, that they might have the greater surface in respect of their

their bulk; these I found at first to do very well, as to the imbibing of the moisture of the Air, but afterwards they did not continue to answer my expectation. I caus'd likewise to be turn'd out of a light wood a Cup, which, that it might less burden a tender balance, had, instead of a foot, a little button, to which a hair might be tied, to suspend it by; and this Cup being purposely turn'd very thin, that it might have much surface exposed to the Air, proved for a pretty while so good a Hygroscope, as invited me to make divers Observations with it, some of the which I have still by me. It agreed also with several tryals, that I had made on other occasions, of the porousness of such Bodies, that white Sheeps Leather, such as Chirurgeons us'd to spread plaisters upon, would be very convenient for my purpose. And indeed I found by many Observations, whose success you may command a sight of, that if this Leather were a substance as little obnoxious to Corruption as a Sponge, it would, by its copious imbibitions and emissions of the Aerial moisture, be a fitter matter than any other I had employ'd for a Hygroscope.

But taking all things together, I found no Body so convenient for my purpose as a Sponge, which you will perhaps the more easily believe, if I add, that to help me to make some estimate of the porosity of it, [We weigh'd out a dram of fine Sponge, and having suffer'd it to soak up what Water it could, it was held in the Air, not only whilst the weight of the Water would easily make it run out, but till it dropt so very slowly,

6 *A Statical Hygroscope proposed*

slowly, that a hundred was reckon'd after one drop before another fell; then putting it into the balance it had been weighed in before, we found, that as its dimensions were increased to the Eye, so its weight was increas'd upon the scale, amounting now to somewhat above two Ounces and two Drams; so that one Dram of Sponge, though it seem'd not altogether so fine as the portion we had chosen out for our Hygroscopes, did imbibe and retain seventeen times its weight of Water.]

Now when one is resolv'd to employ a Sponge, there will not need to be much added about the turning it into a *Hygroscope*. For, having weigh'd it when the Air is of a moderate Temperature, it requires but to be put into one of the scales of a good balance suspended on a Gibbet (as they call it) or some other fix'd and stable supporter. For the Sponge being carefully counterpoised at first with a metalline weight (because that alters not sensibly with the changes of the Air) it will by its decrement or increase of weight shew, how much the neighbouring Air is grown dryer or moister in the place where the instrument is kept. The weight of the Sponge may be greater or less according to the bigness and goodness of the balance, and the accurateness you desire in the discoveries it is to make you. For my part, though I have for Curiosity's sake with very tender scales employed for a good while but half a dram of Sponge, and I found it to answer my expectation well enough; and though, when I us'd a
bulk

to be further tryed, &c.

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bulk divers times as great, in a stronger, but proportionably less accurate, balance, I found not the Experiment successless; yet after tryals with differing quantities of Sponge, I prefer'd, both to a greater and lesser weight, that of a dram, as not being heavy enough to overburden the finer sort of Goldsmiths scales, and yet great enough to discover changes considerably minute, since they would turn discernably with a sixteenth or twentieth part, and manifestly with half a quarter of a grain.

With such *Hygrosopes* as these (wherein the balance ought to be still kept suspended and charged) I made several tryals, as my removes and accommodations would permit, sometimes in the Spring, and sometimes in the Autumn, and sometimes also in the Summer and Winter. But nevertheless it would be very welcome to me, if you and some of your Friends would be pleased to make tryals your selves, and compare them with mine, and especially take notice, if you can, whether in any reasonable tract of Time there will be any loss (worth noting) of the substance of the Sponge it self; I having not hitherto discover'd any. In the mean time, to invite you to give your selves this trouble, after I have told you, that having once, among divers removes, had the opportunity to keep a dram of Sponge suspended during a whole Spring, and a great part of the preceding Winter and subsequent Summer, I did not think my pains lost, though divers of the observations they afforded me have unhappily been so, among many

8 *A Statical Hygroscope proposed, &c.*

ny other memorials about Experiments of differing kinds; notwithstanding which unseasonable loss I shall venture to suggest some things to you, that occur'd to me about the *Utilities* of the Instruments I am treating of.

A BRIEF



A

BRIEF ACCOUNT

O F T H E

Utilities of H Y G R O S C O P E S.

THe use of a Hygroscope is either general or particular : The former is almost coincident with the Qualifications to be wish'd for and aim'd at in the Instrument it self; The latter points out the particular applications that may be made of it when 'tis duely qualified. Of each of these I shall briefly subjoyn what readily occurs to me.

The general use of a Hygroscope is, *To estimate the changes of the Air, as to moisture and dryness, by wayes of measuring them, easie to be known, provided, and communicated.*

I might here pretend, That as these are the principal things that have been desir'd in Hygroscopes, so 'tis obvious from the description and account we have

have given of our Instrument, that these advantages belong to it in no very despicable degree: And that to make such Hygroscopes as will perform all these things in perfection, whatever it may seem to a Mental contriver, will, I fear, prove no easie task to those that really attempt it. To these things I might add, that if such allowances be made, as what I have represented may invite you to grant, the Qualifications lately mentioned, as desirable in a Hygroscope, may in a tolerable measure be found in ours, when we shall come to mention the particular uses of it. And as for that of conveying to others the Observations made with it, you may please to consider, that the things I employ to measure the degrees of dryness and moisture in the Air, being grains, parts of grains, and greater weights, the accessions of moisture which the Sponge receives, or the losses that it suffers, can be easily and at the same time both found and determin'd. And as the weights employed to determine these differences are easily procurable; so the Observations made with them, may (together with patterns, if it should be needful, of the weights themselves) with the same facility be communicated by Letters even to remote parts. In which conveniency, whether, and how far, our Instrument has the advantage of that made with an *Oaten beard*, and some others that I have employed, I leave you to consider.

I might farther alledge on the behalf of our Instrument, that whereas, besides the Qualifications above mentioned, there is another, namely Durableness, which though not so necessary to constitute a Hygroscope, yet is necessary, as will ere long appear, to some of the considerabest uses of it: And

Utilities of Hygroscopes. 11

And *whereas* such a Durableness is wished, as may not only keep the Instrument from having its substance rotted or corrupted by the Air, but may also preserve it in a capacity to continue pretty uniformly its Informations of the Air's moisture, even when that increases very much, or lasts very long; *Whereas*, I say, these things are much desir'd in a Hygroscope, our Sponge seems herein preferable to the Oaten beard, Lute-strings, &c. For in those and the like Bodies the self-contracting or relaxing power (as 'tis suppos'd) or the disposition to imbibe and part with the moisture of the Air uniformly or after a due manner, is wont to be in no very long time alter'd or impaired; and particularly, when they have imbib'd much aerial moisture, they are very faintly affected by the supervening degrees of it, and so the operation is too disproportionate to what the like Cause would have produc'd, when the Instrument was well dispos'd; whereas in our Sponge neither the degree of springiness, nor any such like quality is consider'd, and it is capable of imbibing so much more of the Aqueous particles, than even moist Airs and Seasons are wont to supply it with, that there is little fear that it will be glutted, or have its pores choaked up with them, so that the decrements and accessions of weight will be more proportionate to the degree of moisture in the Air, and more reducible to known and determinate measures.

But though these and the like specious things may be represented in favour of our Statical Hygroscope; yet, to deal ingenuously with you, I much fear, that 'twill be very difficult to bring either Statical ones, or perhaps any other, to be so compleat as to satisfy

fie a nice and severe Critick. And you would perhaps easily assent to my Opinion, if it were not too tedious to entertain you with all the speculative doubts and scruples, as well Mechanical as Physical, which my accustom'd diffidence has now and then suggested to me. But because such a sceptical Discourse would be too tedious, and also somewhat improper, to be propos'd by one that would recommend Hygroscopes, I shall only now take notice of one great Imperfection, which all that I have been acquainted with are liable to; namely, that men have not yet found, nor perhaps so much as dream'd of seeking, a Standard of the Dryness and Moisture of the Air, by relation to which, Hygrometers may at first be adjusted, and so be compar'd with one another, as we see many of those seal'd Thermoscopes, that have been made and justn'd by Mr. *Shotgrave* the dextrous Operator of the *Royal Society*. I deny not, that, by virtue of a standard to estimate moisture by, I have endeavour'd to remedy this inconvenience; but, as my hopes were but small, so neither was my success great, but I am not sure, that happier Wits, or I my self at some other and luckier time, may not more prosperously attempt it. In the mean while perchance you will not think it altogether nothing, if the Trifle I present you perform at least some of the things desir'd in a Hygrometer less imperfectly, than any you have yet met with. And that you may not be discourag'd by what I have lately acknowledg'd of the defects of such Instruments; I think it now seasonable to proceed to the mention of the particular *Uses*, for which, notwithstanding any inevitable

Utilities of Hygroscopes. 13

evitable defects, a Hygroscope, and even such a one as I now present you, may be made easily to serve.

U S E I.

To know the differing Variations of Weather in the same Month, Day and Hour.

IT may be useful for divers purposes, to know both that the Air is wont to be less moist at one part of the Artificial Day, (and so of the Night,) than at any other, & at what particular time of the Day or Night it most usually is so. And on this occasion I remember, that usually when the Weather was at a stand, it was observed, that the Sponge had manifestly gain'd in the Night, though it were kept in a Bed-chamber, and grew lighter again between the morning and noon. This Observation which was made towards the end of Winter would not hold, in case frosty nights or some other powerful Cause intervened. 'Twere not amiss also to observe, Whether there be not a Correspondence betwixt the Hygroscope and Baroscope; and, if there be, in what kind of Weather or Constitution of Air it is most or least to be discerned. And this enquiry seems the more dubious, because the same changes of the Atmosphere may, upon differing accounts, have either the like, or quite contrary, operations upon these two Instruments. For in Summer when the Atmosphere is usually heavier, the Hygroscope is
M usually

14 *A Brief Account of the*

ually lighter; some strong Winds, as with us the North-west, may make both the Atmosphere and Baroscope lighter, whereas Southerly Winds, especially if accompanied with rain, often make the Atmosphere lighter and the Sponge heavier. And on the other side I observe, that Easterly Winds, especially when they begin to blow in Winter, though, by reason of their dryness, they are wont to make the Hygroscope lighter, yet they are wont, at least here at the West-end of *London*, to make the Baroscope shew the Air to be heavier. It were likewise fit to be observed particularly by those that live on the Sea-coast, Whether the daily ebbing or flowing of the Sea, do not sensibly alter the weight of the Hygroscope. It were very well worth while also to take notice, at what time of the day or night, *ceteris paribus*, the Air is the most damp and most dry, and not only in several parts of the same day, but in several dayes of the same month; especially on those days, wherein the full and new Moons happen. And this seems a more hopeful way of discovering, whether the full Moon diffuses a moisture in the Air, than those Vulgar Traditions of the plumpness of Oysters and Shell-fish, and brains in the heads of some Animals, and of Marrow in their bones, and divers other *Phaenomena*, which, as I have shewn in another paper, 'tis not easie to be sure of. It may also be noted, whether Monthly Spring-tides, especially when they fall out near the middle of *March* or *September*, have any sensible operation upon our Instrument.

U S E II.

To know how much one Year and Season is dryer or moister than another.

THIS cannot be so well perform'd by the Hygroscope made of an Oaten beard, if they; that have made use of them more than I, do complain with reason; that after some months (for I cannot tell you precisely how many) they begin to dry up and shrink; so that their sense of the varying degrees of the moisture of the Air is not so quick as before, and the informations they give of the degrees of it, especially towards the outmost bounds of their power to shew the Air's alterations; recede more and more from Uniformity. But the lastingness and other convenient qualifications of our Sponge making its capacity of doing service more durable, may the better help us to compare the greatest moisture and dryness, both of the same season, and of the seasons of one Year with the correspondent ones of another. And if the weight of the Sponge at a convenient time, when the temperature of the Air is neither considerably moist, nor considerably dry; be taken for a Standard, a person that should think it worth his pains, may, by computing how many dayes at such an hour, and how much at that hour, it was heavier or lighter than the standard; and also by comparing the result of such an account in one year with the result of the like account in another year, be assisted to make a more particular and near estimate of the differing temperatire of

the Air, as to moisture and dryness, in one year than in another, and in any correspondent season or Month, assigned in each of the two years proposed. And how much the Collation or Continuance of such Observations, both in the same place and also in differing Countries and Climates, may be of use to Physicians in reference to those Diseases, where the moisture and dryness of the Air has much interest; and the Husbandman to fore-see what seasons will prove friendly or unkind to such and such Soils and Vegetables; it must be the work of time to teach us, though in the mean while we have no reason to despair, that the Uses to be made of such Observations may prove considerable. And the rather, because if by help of the result of many Observations men be inabled to foresee (though at no great distance off) the temperature of a year, or even of a season, it may advantage not only Physicians and Plow-men, but other Professions of men, who receive much profit or prejudice by the dryness or excessive moisture of the seasons. And not to mention those who cultivate Hops, Saffron, and other Plants that are tender and bear a great price; such a foresight, as we are speaking of, may be of great use to Shepherds; who, in divers parts of *England*, are oftentimes much damnified, if not quite undone, by the rot of Sheep, which usually happens through excess of moisture in certain months of the year. And in order to the providing of foundations whereupon to build Predictions, it may not be amiss to register the number, bigness, and duration of the considerabler spots, that may at this or that time of the year happen to appear or be dissipated on or near the Sun, or to take notice of

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of any extraordinary absence of them, and to observe whether their apparition or dissipation produce any changes in the Hygroscope: Which Curiosity I should not venture to propose, but that (as I elsewhere note) I find, that eminent Astronomers have casually observed great drynesses to attend the extraordinary absence or fewness of the Solar Spots. And those persons that are Astrologically given, may, if they please, extend their Curiosity in the use of this Instrument to observe, whether Eclipses of the Sun and Moon, and the great Conjunctions of the Superiour Planets, have any notable operation upon it.

U S E III.

To discover & compare the changes of the Temperature of the Air made by Winds, strong or weak; frosty, snowy, and other Weather.

THIS may conveniently enough be done as to winds, either by our whole Instruments or (perhaps better and more safely) by the Sponge alone, which may be taken off and hung by a string, for as long time as is thought fit, in the wind, and then restor'd to its former place. For I found by removing it into the wind, that it soon receiv'd a very considerable alteration in point of weight, as also it did when remov'd out of a room into a garden where the Sun shin'd; for though the season were not warm, it being then the Moneth of *January*; yet in three quarters of an hour the sponge lost the 24th part of its weight. We may also in some cases

cases usefully substitute to a Sponge a somewhat broad piece of good Sheeps-leather display'd to the wind. For this having, by reason of its thinness (or very small depth,) in proportion to its breadth, a very large *Superficies* immediatly expos'd to the wind, we found it to be notably alter'd thereby, in so much that half an ounce of well prepar'd Sheeps-leather, (that we had long employ'd as an Hygroscope) being kept an hour in a place, where the Sun-beams might not beat upon it, did, in a strong wind, vary in that short time an eighteenth part of its original weight. But though I think it very possible to make such observations of the Temperature of particular winds, as will frequently enough prove so true as to be usefull, at least to those that live in the places where they are made; yet I am of opinion, that, to be able to settle Rules any thing general, to determine with any certainty the *Qualities* of winds according to the corners whence they blow, as from the East or West, North-east, South-west, &c. there will be a great deal of wariness requir'd; and he that has not some competent skill in *Physicks* and *Cosmography*, will easily be subject to mistakes in forming his Rules. To countenance which advertisement, I shall now make use but of these two Considerations, whereof the first is; That winds that blow from the same *Quarter* are not in some *Countrys* of the same *Quality* that they are in most others, the wind participating much of the nature of the *Region* over which it blowes in its passage to us. At the famous *Port of Archangel* they observe, that whereas a *Northerly* wind almost every where else without the *Tropicks* produces frost in *Winter*, there it is wont to be attended with a thaw, so as to make the *Eeves* to drop. Of which
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the reason seems to be, that this wind comes over the Sea which lyes North from that place; and on the contrary, a Southerly wind blowing over a thousand or twelve hundred miles of frozen land does rather increase the frost than bring a thaw. This was by the Inhabitants averr'd to the Russian Emperors Physician, who was more than once at *Archangel*, and from whom I had the Account. The Northern windes that are elsewhere wont to be drying, are said in *Egypt* to be moist. I remember Mr *Sands*, in his excellent Travells, giving an account of what he observ'd about the largest of the fam'd Egyptian Pyramids, has this considerable Passage; *Yet this hath been too great a morsel for time to devour, having stood, as may be probably conjectur'd, about three thousand and two hundred years, and now rather old than ruinous: yet the Northside most worn by reason of the humidity of the Northern Wind, which here is the moistest.* *Sands in Purchas's Pilgrimage.*

And 'tis yet more considerable to our purpose what I find related by Monsieur *de Serres* in his usefull book of Husbandry, since by that it appears, that even in not very distant Provinces of the same Kingdome the winds that

Lib. 6. Cap. 8. Sect. 3.

Theat. d' Agricult. Lib. 1. chap. 7.

blow from the same Quarter may have very differing Qualities and effects. For, speaking of the Changes of the Air in reference to Husbandry in several parts of *France*, he informes us, that 'tis observ'd, that in the Quarters about *Tholozæ* the South-wind dryes the ground, and the North gives rains. Whereas on the contrary from *Narbonne* to *Lyons*, all over *Provence* and *Dauphiné*, this last nam'd wind causes dryness, and the other brings

A Brief Account of the

moisture. And this may suffice for my first Consideration. My Second is this, That the vehemence or the faintness of the windes, though blowing over the same country, may much diversify its operation on the Hygroscope, and the same wind, which, when it blows but faintly, or even moderately, is wont to appear moist by the Hygroscope, may, when vehement or impetuous, make the Instrument grow lighter, discussing and driving away more vapors by the agitation of parts it makes in the Sponge, than is countervail'd by those aqueous Vapors that are brought along with it. But on such things as these I have not leisure to insist, and therefore I shall proceed to take notice in very few words of some other operations of differing weathers on our Instrument, and tell you, that Frosty weather often made the Hygroscope grow lighter even at night: Snowy weather which lasted not long, added something to the weight of the Sponge. And it has been observed that mists and foggy weather us'd to add weight to it, even notwithstanding Frost.

To which may be added an Observation made by my Amanuensis, who having a convenienter chamber than mine, (wherein a fire was daily made,) was diligent and curious to set down the changes of the Hygroscope that was left in his lodging; for this observation makes it probable, that a transient cloud in fair weather may be (for I say not, that it always is) manifestly observable by our Instrument. For by his Diary it appears, that the 9th. of September being for the most part a very fair Sunshiny day, though about ten a clock in the morning the Sun shone brightly, the Sponge began to preponderate, which unexpected *Phenomenon* made him

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him look out at the window, where he discover'd a cloud that darken'd the Sun, but after a while that being past the balance return'd to an *Equilibrium*. On this occasion I shall intimate, that I have more than once or twice observ'd, especially in Summer, that when the Air grew heavier, the Hygroscope either continued at a stand, or perhaps, also grew lighter; as if, when such cases happen, the *Effluvia* that get into the Air, either from the Terrestrial or some other mundane globe, were not fit like vapors to enter and lodge in the pores of the Sponge, and so were Corpuscles of another nature, with which when we find by the Baroscope that the Air is plentifully stockt, it may be worth while to observe, Whether any, and if any, what kind of Meteor, as wind, or Rain it self, or Hail, or in the Winter Snow or frost, will commonly be signified and produc'd.

U S E IV.

To compare the Temperature of differing Houses and differing Rooms in the same House.

AS this is of great use both in respect of mens Health, especially if they be of a tender or sickly constitution, and in respect of conveniency for the keeping flesh, sweet-meats and several sorts of wares and goods, and even household-stuff, that are subject to be indammaged by moist air; so it is readily and manifestly derivable from our Instrument. For, by removing it into several Houses or into several parts of the same house, and letting it stand

stand in each a competent time to be affected with the temperature of the Air of that particular place, we have divers times observed a notable difference, as you may guess by the two or three Notes I met with among some old papers.

Obs. 13. [Three or four days agoe a piece of fine Sponge being taken out of a Cabbinet and clipt till it came to weigh just half a drachm in a nice pair of scales and a warm room, was afterwards remov'd into a neighbouring room destitute of a chimney, (and yet within 3 or 4 yards of a chimney seldom without fire :) This statical Hygroscope, consisting of the scales and the frame they hung on, was yesterday night remov'd into the former room, and the Sponge was found to have gained 3 grains and an eighth or better, and consequently more than a tenth part in reference to its first weight; but being suffer'd to stand in this warm room, in less than 12 hours it lost a grain and about $\frac{1}{8}$ of its former weight, though the time it stood in this room were for the most part night and rainy weather.]

[We took a piece of very fine Sponge, which formerly had weigh'd just a drachm, but having been many months kept in a very warm room where fires were kept every day, it was grown much lighter; for, removing it into an upper chamber in a neighbouring house and weighing it in tender Scales, in the Evening 'twas found to want of a Drachm 4 grains and $\frac{3}{4}$ of a grain; and though there was a fire in the room and the Scales stood not far from it, yet, in a short time, (the day being foggy and rainy,) the Sponge visibly depress'd its Scale $\frac{3}{8}$, and the next morning was found to want but one grain and a half of a Drachm, so that it had gain'd about three grains and

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a quarter, and the following evening, being the second of *January*, it weigh'd one drachm a grain and almost half a grain. So that in about one natural day the Sponge had acquir'd six grains from the moisture of the Air, that is, a tenth part of its first weight (I mean a drachm) and a greater proportion in reference to the weight it had the day before. The third of *January*, the weather being yet moist, the weight exceeded two grains, but about 3 or 4 of the Clock in the afternoon it began to lose of that great weight, which diminished more by the next morning, the weather having chang'd that night and become somewhat frosty.]

In another paper I also find this Note. [The drachm of a sponge, that had for divers weeks been kept in a dry room, was (*January* the tenth) carried out into a room where fire is not wont to be kept, the weather being extraordinarily foggy: This morning being brought into the former room, though now the weather be clear (yet not frosty) it appears to have gain'd in weight about eleven grains; yet it soon lost 2 grains by standing in this room all the while in the balance.]

U S E V.

To observe in a Chamber the effects of the presence or absence of a fire in a Chimney or Stove.

THis is easily done, and the more easily if the room be small. For in such chambers I have often observed a moderate fire to alter the weight of the instrument, placed at a distance from it, after it had been well kindled but a very little while; but in wet weather, if the fire were not seasonably renewed with fresh fuel, the decay of it would, in no long time, begin to be discernable by the Instrument.

U S E VI.

To keep a Chamber at the same degree, or at an assign'd degree, of Dryness.

Supposing the alteration of weight in our sponge to depend only upon the degree of the moisture of the Air, the last named use will be but an obvious Corollary of the former. For, if a convenient part of the Room be chosen for the Hygroscope, and it be kept constantly there, 'tis easy, by casting ones eye on it from time to time, to perceive when 'twill be requisite to increase or moderate the fire, so as to keep the sponge at that weight it was of, when the temperature of the Air of the chamber as to dryness and moisture was such as was desired. I will
not

not trouble you with some scruples, which I confess the consideration of this use of our Instrument suggested to me, because I have not now the leisure to discuss them. I had thoughts to try, whether and how far a good Quantity of salt of Tartar or even dried Sea-salt, being kept in a closet or some closer room, might by imbibing lessen the moisture of the Air in it, but I did not perfect any observation of this kind. But I will add to what I have already referred to this sixth Head, that I have sometimes noted with pleasure, how manifest and great a change in the weight of our Sponge would be made, when the room was washed and a good while after, notwithstanding that a good fire was kept in it to hasten the drying of it.

Besides the hitherto mention'd uses of our Hygroscope, I know not whether there may not be divers others, and whether we may not, by a little altering and helping it, make it capable of shewing us some difference betwixt steams of differing natures, as those of Water, spirit of Wine, Chymical Oils, and perhaps new kinds of substances (such as we have not yet taken notice of) in the Air, in which, I confess, I suspect there may sometimes be dispersed store of Corpuscles, that I do not yet well know what to think of. For I have more than once observed (not without some wonder) the Hygroscope not to be affected with the alteration of weather, answerably to what the manifest constitutions or variations of it seem plainly to require: Whether unobserv'd Corpuscles perform'd this by making the other steams in point of figure, or size, incongruous to the minute pores of the sponge, and so unfit to enter them; or by dissipating or otherwise pro-

procuring the avolation of more of the watery particles than they could countervail, I now examine not. And I am not sure, but by associating this instrument with the Thermoscope, Baroscope and

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some others that may be proposed, it might be so improved, as to help us to foresee divers considerable things, that either are themselves changes of the Air, or are wont to be consequences of them: As sickly and healthfull constitutions of the Air both as to Man and Cattle; and healthful, barren or plentiful seasons in particular places or Countrys; and perhaps also strong Hurricanes, Earthquakes, Inundations, and their ill effects, especially those accidents that depend much upon the surcharge of the Air, with other Exhalations and moist Vapors, which operate before sensibly upon our Instrument, and therefore may be discernable by it a good while before they arrive at that height that makes them formidable Meteors. And if it were but the foretelling approaching rain, this very thing may on divers occasions prove very serviceable, and recommend our instrument, which often receives much earlier impressions from the steams that swim up and down in the Air, than our senses do, so that I have been able to foresee a shower of rain, especially in dry weather, a not inconsiderable while before it fell.

And here I should dismiss our subject, which I have already dwelt on longer than I design'd, but that remembering a caution I gave you when I was speaking of winds, I think it but fit to add two or three lines, to keep you from being by that Advertisement discouraged from endeavouring to make in the

See the III use.

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general such Hygroscopical observations, as may be reduc'd to *Hypotheses*. For, as I elsewhere discours'd concerning *Barometrical* Theories, if I may so call them; so I shall here represent concerning *Hygroscopical* ones, that if a Theory or *Hypothesis* that is it self rational, be found agreeable to what happens the most usually in observation; it ought not lightly to be rejected or so much as laid aside, though sometimes we find particular Instances, that seem to call it in question. For 'tis very possible, that the Theory or *Hypothesis* may be as good as a wise man would require about so mutable a subject as the weather. And the Cause assign'd by the *Hypothesis* may really act suitably to what that requires, though a contrary effect insue by reason of that Causes being accidentally master'd and overrul'd by some more powerfull Cause or Agent that happens for that time to invade the Air. As we know that Tides do for the main correspond with the motions of the Moon, (whose *phases* are therefore argued from them,) and do generally ebb and flow at such times and in such measures as the Theory, that has been grounded on that correspondency, requires; but yet Seamen find, that in this or that particular harbor or mouth of a River, fierce contrary winds, great Land-floods and other casually intervening Causes, do sometimes both very much disturb the regular course of the Tides, and increase or lessen them.

F I N I S.

A NEW
EXPERIMENT
And other INSTANCES
OF THE
EFFICACY
OF THE
AIR'S MOISTURE.

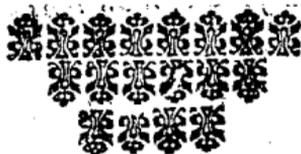
Subjoin'd by way of

A P P E N D I X

To His

STATICAL HYGROSCOPE

By the Honourable ROBERT BOYLE.



L O N D O N,
Printed by E. F. for R. Davis, Bookfeller
in Oxford, 1673.



ADVERTISEMENT

THe Author had thoughts of illustrating the foregoing Paper, with a Collection of Hygroscopical Observations, but though he several times begun Diaries of Occurrences of this Nature, as his Removes and other Avocations would permit; yet besides that these Impediments made him more than once break off his work, after he had continued it for a Month or two or longer, such unwelcome Accidents happen'd since the foregoing Tract was sent away, that he could not sensibly recover any competent number of Observations, and fears he shall never recover some of them, which he doubts not to have been, with many better upon various subjects, stoln away from him. Upon which occasion he thought fit to try, whether the following Paper might not be look'd upon as some amends for the missing of those Observations in whose room it is substituted.



(1)



A NEW
EXPERIMENT
And other
INSTANCES
OF THE
EFFICACY of the AIR'S Moisture.

Since it may probably serve to recommend Hygroscopes to you, if that Quality of the Air, which these Instruments are usefull to give us an account of, be made appear to be more powerful, and have considerable effects, than is commonly believed; it will not be from my purpose to present you here some Instances that have led me to think, that the Effects of the *Moisture* of the *Air* may be considerable not only upon mens Healths, but upon subjects far less tender, and less curiously contriv'd, than Humane bodies. But I hope, you will easily believe, that by the *Moisture* of the *Air* I mean not a meer and

2 *A New Experiment and other*

abstracted quality, but moist Air it self, or rather those humid Corpuscles, (chiefly of an Aqueous nature,) that abound, and rove to and fro, in our common Air.

That the Moisture of the Air may have no small influence, and usually a bad one, upon mens healths, is that, which, though Experience did not so often teach us, I should venture to argue from what I have observ'd of the operation of moist Air upon the dry and firmly context parts of Animals, and even in those cases, where, for want of time or other Impediments, this Moisture cannot produce any sensible degree of putrefaction.

That the skins of Animals may be easily invaded by the moist particles of the Air, is the more probable, because of the numerousness of their Pores, which may be concluded from their hairiness, or their sweat, or both. And I formerly observed to you, that I found *Sheeps-Leather* to imbibe the moisture of the Air, and increase in weight upon it, as plentifully as almost any Body I expos'd to it.

But to shew you, that much closer Membranes, and which Nature made to be impervious to such a Liquor as Urine it self, may be affected by the Vapours of the Air, I shall add, that having purposely taken pieces of Bladders fine and well blown, and, as far as appear'd, of a very close contexture, and counterpois'd them in a good balance, I found, according to expectation, that they would considerably increase their weight in moist, and lose it again in dry, weather; so that I might have employed the most membranous part of a bladder (for I thought not fit to make use of the neck or the adjoining part) to make a *Statical Hygroscope*.

And,

Instances of the Efficacy, &c. 3

And, as for other membranes and fibres, I shall have by and by occasion to take notice, that even when they are strongly and artificially wreathed together into gut-strings, they may imbibe enough of the moisture of the Air to be broken by it. And, I remember, I formerly told you, that I had observ'd *Lute strings* to grow heavier in moist Air.

And whereas *Bones* are by all confess'd to be the firmest and solidest parts of Animals, and as it were the pillars by which the fabrick is sustain'd; yet it seems, that even they may be pierc'd into, and sensibly affected, by the moisture of the Air. For I remember, that having caused the *Skeleton* of a humane body to be so made by a famous and very skilful Artist, that, by the help only of slender wires artificially order'd, the motions which the Muscles make of the bones of a living body might be well imitated in the *Skeleton*, I observed, that though in dry and fair weather the flexures of the Limbs might be readily made, yet in very moist weather the joynts were not easily bent, as if the parts were grown stiff and rigid; which seem'd to proceed hence, that moist particles of the Air, having plentifully insinuated themselves at the Pores into the Bones, had every way distended them, and thereby made the parts bear hard against one another, (which they did not at all before) at the *Junctures* or *Articulations*.

But it will be the more readily believed, that the Moisture of the Air may operate considerably upon the tender and curiously contriv'd Bodies of Men and other Animals, if, proceeding to the Observations I chiefly design, I make it appear, that the moistning Particles, that rove up and down in the

4 *A New Experiment and other*

Air, are able to exercise a notable (and, if I may so call it, a Mechanical) force even upon Inanimate and Inorganical bodies : which may well suggest a suspicion, that *Hygrosopes* being the proper Instruments to discover a Quality in the Air, whose efficacy reaches farther than is commonly taken notice of, they may in time be found useful to divers other purposes, besides those that relate to the health of men.

That *Wood*, especially when it has been season'd, is a Solid of a strong and firm contexture, if it were not obvious by the daily use made of it in building Ships, Houles, &c. might be easily concluded from the weight or force requir'd to alter its contexture by making any considerable, or perhaps sensible, Compression of it. And yet, that *Wood* may suffer a kind of divulsion of a multitude of its parts, and be manifestly distended by aqueous Corpuscles getting into its Pores, I remember, I proved by this Experiment. I got a piece of sound and season'd *Wood* of about an inch (or an inch and half) in Diameter, to be by a skilful Artiff made Cylindrical, and also a ring of some solid matter, as Brass or Ivory, to be exactly turn'd to fit this Cylinder, so that it might without much ease, or much difficulty, be put on and taken off again : Then we put the turn'd piece of *Wood* into fair Water, and left it to soak there for many hours ; at the end of which it was visibly swell'd, and though I cannot now tell you, (for want of a Paper concerning that Experiment,) *how much* it was increas'd in Diameter, yet I well remember the increment was considerable, and that the ring, that was adjusted to it before, was manifestly too little

Instances of the Efficacy, &c. 5

little to be put again upon it, or with its Orifice to cover the whole *basis* of the distended Cylinder, which afterwards being dryed in the Air shrunk into a capacity of entering the ring again. And in this Experiment I took notice, that the great Intumescence of the Wood was not produc'd all at once, or soon after it was put into the Water, but it swell'd by degrees, and lay soaking there many hours before it arriv'd at its utmost distension, the aqueous Corpuscles requiring, it seems, so much time to insinuate themselves sufficiently into the Wood; which argues, that the internal parts were likewise affected, though, when even they came to swell, they had a good thickness of Wood about them to hinder their Dilatation.

I expect you should now tell me, that this distension of so firm a Body was made by Water itself, and not by the humid Vapours of the Air. On which occasion I might represent to you, that by the sweating (as men commonly call the adhesion of waterish drops to the surface) of polished marble and some other cold and smooth Bodies, that sometimes happens even in the Heat of Summer, if they be cold, and the ambient Air moist enough; it appears, that both in hot weather the Air may be plentifully stock'd with aqueous Vapours, and that these Vapours need to do no more than convene together to constitute visible and tangible Water. And on this occasion, if I were sure I had not told you of it already, I should subjoyn an Experiment which would detect the Vulgar error of those that think the adhering drops, lately mention'd, to come from some internal moisture deriv'd by its pression or percolation from the marble or the

6 *A New Experiment and other*

Body they are fasten'd to ; and at the same time I shall shew (what is not wont to be imagin'd) that in the Heat of Summer the Air is furnished with invisible and yet aqueous steams. The Experiment I long since try'd in Winter with Snow and Salt, included in a glass Vessel, and then put to dissolve in a balance. But because neither Ice nor Snow is at all easie to be come by among us in *England* in Summer, and because, at that season, the Air in fair weather is presum'd to be dry as well as hot, I chose, within some dayes of *Midsummer*, and in clear Sun-shiny weather, to make the following tryal.

We took a pint glass-bottle, and having put into it a convenient quantity of Water (for room must be left for the Salt) we plac'd them and four ounces of beaten *Sal Armoniack* in one scale of a good balance, and a counterpoise in the other, and then, putting the Salt into the Water, I observ'd, that though for a while the *Equilibrium* remain'd, yet when the frigorifick mixture had sufficiently cool'd the outside of the Bottle, the roving Vapours of the Air, that chanc'd to pass along the surface of the Vessel, were, by the contact of that cold Body, arrested, and turn'd into a kind of a dew, which from time to time made the scale, that held the glass, preponderate more and more, and at length the drops growing greater and greater, ran down in small rivulets the sides of the Glass, and in less than an hour, (by my estimate,) the condens'd steams amounted to near a dram, which weight was afterwards much increas'd within about two hours more : Whereby it sufficiently appears, both that this dew came from without, (since if it had been a transuda-

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Instances of the Efficacy, &c. 7

tion, it would not have added weight to the scale that received it,) and that there is even in clear Summer weather a vast number of moist particles dispers'd through the Air, since, in about an hours time, such a multitude of them as the Liquor produc'd may be suppos'd to consist of, and may by Heat be actually resolv'd into, could in course come to touch so small a surface, as that of that part of so small a bottle which contain'd the frigorifick mixture. For the rest of the Vessels surface was not cold enough to condense the Vapours into Liquor. But to return to what we were saying of Wood swell'd by water; because, notwithstanding these Considerations, I am willing to allow, that the Experiment of the Cylinder does not fully come home to our purpose, and that I produc'd it not so much to *prove directly* the force of moist Air, as to *countenance* what I am about to say, by shewing what a sufficient number of aqueous Corpuscles may do in the solid wood they penetrate, I shall now add some instances of the force these particles may exercise upon Solids, when they invade them but in the form of Vapours.

That in this form the multitude, figures, and motions of these insinuating particles may enable them to display no small force in their operations on some Bodies, we have one Instance that often happens, though but seldome reflected on, in the breaking of the strings of Musical Instruments, first brought to a good Tension, upon the supervening of rainy weather. For the cause seems to be, that the Vapours that then wander through the Air, insinuating themselves into these strings, (which the Musician often forgets to let down or relax after ha-

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ving skrew'd them up,) distend and swell them, and thereby endeavour to shorten them, and that so forcibly, that they not seldom break with a smart noise and great violence; which, because it happens without any visible efficient, men commonly think and say, that such strings break of themselves. But, to take no further notice of this popular surmise, if we consider how much weight some of those bigger strings, especially of Base Viols, that have been observed to break in rainy weather, will require to stretch any of them to a rupture, you will easily be induc'd to think that this operation of the moist Air exacts, and therefore argues, more than a languid force.

But here probably you will tell me, that the Instances you expected were concerning Wood, which is a far solidier Body than gut-strings. To this I say, that the newly recited Instance belongs directly to the title of this Paper, and, being above refer'd to, ought not to be pretermitted. And, as to your expecting Instances concerning Wood, I might content my self to refer you to what is observ'd about the uneasie opening and shutting some doors well adjusted to the door-case in very rainy weather. But though this Observation favours my design, yet I had rather give you Instances in wood purposely and carefully season'd. And therefore I shall now inform you of these two things; *one* that I found by tryal (as I have elsewhere noted) that Wood counterpoised in a good balance would grow sensibly heavier in wet weather, and lighter again in dry; and the *other*, that, to satisfie my self yet further, I consulted an ancient Musician, to whom I had once been a Disciple, and a famous Organ-maker,

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maker, to know whether they had not observ'd that the wood it self, &c. of Musical Instruments would receive such alterations from the moisture of the Air, as might be discern'd by the Ear? Upon which inquiries, the Master of Musick answer'd me, That though Metalline strings will not change with the weather like Gut-strings; yet Virginals (for instance) though furnished with wire-strings, will for the most part of them, (for some he has observ'd to be so well season'd that they are not alter'd by the weather,) be out of Tune in wet weather, the strings generally then affording their notes sharper than they should or are wont to do. And the Organ-maker confess'd to me, that, upon great changes of weather, divers Organs would (after they had been long ago tuned) grow out of tune, and that not only the* wooden pipes would be thereby swell'd, but the Metalline pipes untuned.

But if Bodies be of such a Constitution as not only to admit but assist the operation of the moist Air, the penetrancy and efficacy of this may be found much more considerable than in the fore-going Instances. For there are some kinds of those Marchasites that yield Vitriol, which, whilst they lye under ground, or are cover'd with the Sea-water, on whose shores they are in some places to be found, retain a stone-like hardness, and are often taken for meer stones; and yet some credible persons that are conversant about Vitriol have casually observ'd, that these, being expos'd to the Air, would in tract of time be so penetrated by the moist particles of it, though perhaps not meerly as moist, that (probably by the help of the Vitriolate Corpuicles they met with among the stony matter) these hard and solid

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Marchasites are brought to swell so much as to burst. That this will happen to such kind of stones (though they be of a close and heavy nature) by the help of rain, Experience has perswaded me, and that it may also happen even to very hard and stone-like Marchasites, (for many are not such,) when they are merely expos'd to the Air, I am apt to think upon some tryals of my own. For from shining Marchasites, though but kept in my Chamber window, I have had Vitriolate Efflorescencies that seem'd to be produc'd by the action of the piercing moisture of the Air upon the Mineral. And I remember, that very hard and heavy lumps that were of a Marchasitical substance, (though not at all glistening,) which seem'd to be stony, were so dispos'd to be wrought on by the Air, that though they were kept partly in my own chamber, and partly in other cover'd places, yet in no very long time they were so penetrated by the moist Corpuscles of the Air, that they were not only burst, but broken into many pieces; insomuch that many of them did of themselves fall off from one another, and several of the divided portions would easily be cumbled betwixt ones fingers. And of some of these I have observed with pleasure, that a Vitriolate substance was produc'd more copiously in their innermost parts than on or near their outside. So that, when I consider'd how great an external force would have been requisite to make such a Comminution of Minerals so solid and hard, 'twas obvious for me to look upon the Air's moisture, as capable, when it meets with fitly dispos'd Bodies, to exercise a far greater force than is wont to be conceived.

To these *Phanomena* I might add some others to
the

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the same purpose; but because the Marchasites, and other Bodies requir'd to the producing of them, are not easie to be come by, and the success often exacts a good length of time, I shall conclude this Paper by subjoyning a far shorter Experiment, that I devis'd not only to shew in general, that the moisture of the Air may have a considerable Efficacy, but to assist a *Virtuoso* to make some estimate in known measures of the Mechanical force of the Aerial moisture. And though I now find to my trouble, that I want some of the Notes that concern the Circumstances and the progress of the tryal, yet enough having escap'd to furnish me with the following account of it, what I shall set down may, I hope, at least put you in the way of repairing my misfortune.

Thinking it then probable, that Ropes themselves would considerably imbibe and dismit the moisture of the Air, and that so as to shrink in rainy weather, though clogg'd with a weight fasten'd at the lower end, I was not discourag'd from attempting the following Tryal, by considering that the weight would stretch the Rope, and consequently hinder the presum'd effect of the Air's moisture to be perceived. For I suppos'd, that after a time this unusual stretch of the Rope would cease, and when the weight as such could not lengthen it any more, it would then be capable of being contracted or relax'd, according as the weather should be moist or dry, and so afford me a kind of Hygroscope. Upon these grounds I first caus'd a Rope that was about 20 or 22 yards in length, but of no great thickness, to have one of its ends fasten'd to an immoveable Body at a convenient height from the ground, and then caus'd a Pully to be so fasten'd to another stable Body at the distance

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stance of 18 or 20 yards from the first, that the Rope, resting upon the Pully, lay almost horizontally. But to the end of that part of the Rope, which from the Pully reach'd within two or three foot of the ground, was fasten'd by a Ring a Leaden weight of at least fifty pound. To which was also fasten'd a light *Index* plac'd horizontally, whose end moved along an erected board, which by transverse lines was divided into inches and parts of inches, reaching both a good way upwards and downwards, that the *Index* might within those bounds have room to play up and down according to the alterations of the weather.

It being then Summer, this Tryal was made in a Garden, though partly under a Penthouse, that the Rope might be more expos'd to the Air than it would have been within doors; and two or three dayes, if I misremember not the time, were spent, before the weight had brought the rope to the utmost stretch it was able to give it, after which it began manifestly to shrink and lengthen according to the weather. And I find in one of my Notes, that once I look'd, when I was ready to go to bed, upon the suspended weight, and mark'd how low it reach'd upon the divided board; and that a great part of the night having been rainy, looking again about half an hour after eight in the morning, I found the Cord so shrunk, that the weight was rais'd above five inches, and yet the day growing dry and windy, and sometimes warm, the weight had at night stretched the Rope more than the moisture had contracted it the day before.

Afterwards having procur'd a far greater weight, but therefore unapt to be near so much rais'd, I substituted

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stituted it in the place of that formerly mention'd, and having suffer'd it to stretch the rope as far as it could, I made and register'd some Observations, two whereof having been preserved, I shall transcribe them just as I find them.

June the 4th. At half an hour after nine of the clock at night, I look'd upon the hundred pound weight that hung at the bottom of the rope, the weather being then fair, and a mark being put at that part of the erected board, where the bottom of the weight touched; I perceiv'd the sky a while after to grow cloudy and overcast, but without rain; wherefore going to view the weight again, I found it to be risen a quarter of an inch or more, and, looking on my Watch, perceiv'd there had passed an hour and quarter since the mark was made.

June the 6th. Being not well yesterday, the weight was observ'd by two of my servants, and it then rested at the eleventh inch of the erected board. This morning about eight of clock I visited it my self, and found it to be risen about half a quarter of an inch above the eighth inch, the morning being cloudy, though the ground very dry and dusty. The weather being more overcast, within somewhat less than an hour afterwards I visited the weight again, (some scatter'd drops of rain then beginning to fall,) and found it to be risen about half an inch above the newly mention'd eighth mark. How much more the rope would have been contracted in such lasting moist weather, as usually happens in Winter, I cannot say, having been reduc'd to break off the Experiment, upon a removal, I was, long before that season, oblig'd to make.

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I am sorry I cannot add my other observations, but these I hope may suffice to let you see, that the force of the Air's moisture is not small, since it could raise such a weight as an hundred pound, especially considering the slenderness of the rope it affected. For having measur'd the Diameter near the weight, I found it (as one of my Notes informs me) to be but about the third part of an Inch.

*Was $\frac{3}{10}$ and
& decimal parts
of $\frac{1}{10}$.*

F I N I S.

