PHILOSOPHICAL TRANSACTIONS.

I. *A Second Paper on Hygrometry.*

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IN a Paper which I had the honour to prefent to the Royal Society in the year 1773, I fketched the following propofitions, as fundamental for the conftruction of an *hygrometer.* 1ft, That *fire,* confidered as the caufe of *heat,* was the only agent by which *abfolute drynefs* could be *immediately* produced (§ 5.). 2d, That *water,* in its liquid ftate, was the only fure *immediate* means of producing *extreme moifture* in hygrofcopic bodies (§ 8. and *feq.*). 3d, That there was no reafon, *à priori,* to expect, from any hygrofcopic fubftance, that the meafurable

effects produced in it by *moisture* were proportional to the intensities of that cause; and, consequently, that a true *hygrometrical* scale was to be a particular object of inquiry (§ 2.). 4th, lastly, That perhaps the comparative changes, of the *dimensions* of a substance, and of the *weight* of the same or other substance, by the same variations of *moisture*, might lead to some discovery in that respect (§ 72.). The same propositions will be the subject of this Paper.

Of absolute dryness.

1. An *hygroscopic* body, which is not brought into contact with any other body *drier* than itself, cannot lose any part of its *moisture* but by *evaporation*; and if this is intirely produced by *fire*, there may be such a degree of *heat* as will cause the total *evaporation* of that *moisture*. This is the principle on which the above first proposition was founded; but at the same time I mentioned, that I had not brought it into practice, because of the impossibility of submitting the substance of the hygrometer to such a degree of heat. However, I soon removed that difficulty by considering, that the degree of *heat* necessary to produce *extreme dryness*, might be applied to some substance that could bear it; and that *dryness* be transmitted to the *hygrometer*, by inclosing it with that substance in a proper vessel. The substance I chose was *potash*; and I prepared, for this and some other hygrometrical purposes, an apparatus which was made by Mess. NAIRNE and BLUNT in 1776. But a new objection stopped me again in that pursuit, and led me for some time to a very great and now almost useless labour. The degree of dryness produced by *potash* so used, could be only proportionable to the degree of *heat* that it had received; and

not conceiving yet any known limit to the intenſity of *heat*, I could not expect any limit to *dryneſs*, nor even a fixed degree of it.

2. I remained at that point, with however a *comparable* hygrometer laborioufly conſtructed, till I came to conceive, that *heat* muſt be at its *maximum* in a body, when it is *incandeſcent*; which opinion I have explained in my work, *Idées ſur la Météorologie*. From that firſt idea I ſoon after concluded; that every *hygroſcopic* ſubſtance, which could retain that property after having been brought into *incandeſcence*, would anſwer my firſt purpoſe. The following is the theory reſulting from the whole of the above conſiderations. 1ſt, The *hygroſcopic* ſubſtance which has the moſt capacity for *moiſture*, and receives it the moſt readily, being placed in any quantity in a given ſpace, cannot bring that ſpace to a degree of *dryneſs* greater than its own; and if that degree is undetermined, it cannot afford any *fixed point* for the *hygrometer*. 2d, The *hygroſcopic* ſubſtance which has the ſmalleſt capacity for *moiſture*, and is the ſloweſt in receiving it, if it is really reduced to *extreme dryneſs*, will have the power of producing it in a given ſpace, provided its ſmall capacity be compenſated by a greater quantity, and its ſlowneſs by more time. 3d, Every *hygroſcopic* ſubſtance, which may be brought to *white heat* without loſing its property, is fit to produce *extreme dryneſs* in a cloſe ſpace. 4th, It is indifferent for that purpoſe, that the ſubſtance uſed be of the claſs which has a chemical *affinity* with *water*, it being ſufficient that, after having been reduced to *extreme dryneſs*, it be ſtill capable of receiving it from the ambient *medium*, as may every porous ſubſtance. 5th, But for the practical purpoſe of fixing the point of *extreme dryneſs* on *hygrometers*, ſuch a ſubſtance muſt be choſen as, with a great

capacity for *moiſture*, receives it but ſlowly: as by that firſt property it may be taken in leſs quantity; and by the laſt, it will be leſs ſubject to acquire a ſenſible quantity of *moiſture* in the time neceſſary for the operations.

3. *Pot-aſh* and ſome other alkaline ſubſtances afforded the firſt of thoſe properties, but not the laſt; and I had not fixed on any ſubſtance, when, being at Birmingham in the autumn of 1782, Mr. JAMES WATT informed me, that his friend Dr. BLACK had found in *quicklime* a great capacity for *moiſture*, and much ſlowneſs in retaking it: this he knew, by having kept a long time the ſame *lime* in a cloſe veſſel, for drying ſalts and capillary tubes for thermometers. Theſe were the very properties I wanted for my purpoſe, which thereby I executed as ſoon as I came home. I made thoſe firſt operations in ſmall glaſs veſſels, uſing old *lime*, which I brought again to *white heat* every time I uſed it. Theſe firſt trials agreed with my theory in its firſt point; that of producing conſtantly the ſame degree of *dryneſs*: as for the ſecond, namely, whether that degree was *extreme*, it depended on other experiments.

4. Being ſure then of a fixed degree of dryneſs, the number of experiments I undertook made me wiſh for a means of avoiding the frequent repetitions of bringing again my *lime* to *white heat*; and having found one which has ſucceeded, I am going to deſcribe the apparatus. The veſſel, fig. 1. is of tin, 3 feet high and 1 in diameter. A glaſs plate *a*, *a*, *a*, *a*, is fixed at the top, forming a vertical ſection of the cylinder at 1 inch diſtance forwards from the axis. A woven braſs-wire cage *b*, *b*, *b*, *b*, is fixed in the veſſel through its diameter, in order to keep a ſpace for the inſtruments; for the ſame purpoſe it is open at the top, and alſo oppoſite the glaſs, where the dials of the inſtruments are to be ſeen. For my experiments,

ments, which required inſtruments of various ſizes, I made that cage 18 inches high and 2 deep; but it may be much ſmaller for common hygrometers. The whole veſſel, except that ſpace, has been filled, through the openings *c*, *c*, with *quicklime* taken from the kiln, and ſuffered only to loſe the red heat; after which the openings were covered with heaps of the ſame lime, which abſorbed the moiſture of the air entering the veſſel while it was cooling, and then the openings were ſhut with tin plates and putty. The top of the veſſel has four ſquare openings *d*, *d*, *d*, *d*, correſpondent to the wire cage, for the introduction of the inſtruments, which are hung to hooks. I uſe a hooked wire for putting in or taking out the inſtruments, to avoid bringing my fingers near the openings. Theſe are kept ſhut with tin plates and putty: I never open but one at a time, which I leave open as little as poſſible; and to prevent the introduction of the external air in thoſe ſhort operations, I make them as nearly as poſſible at the ſame temperature, which being 60° of FAHRENHEIT may be obtained in every ſeaſon. With theſe precautions, and alſo by moiſt air being lighter than dry air, there is ſcarcely any moiſture introduced in the veſſel except by the inſtruments themſelves.

5. This application of my method afforded me a very ſtrong confirmation of the practical fixity of the point of *dryneſs* produced in that manner by *lime*: for the apparatus was different from the former ones; 1ſt, by the quantity of the *lime*; 2d, by the *lime* having been put very hot into the veſſel, while, when I uſed glaſs veſſels, I had ſuffered it ſometimes to cool down to 60°; 3d, by that *lime* being of the firſt calcination, inſtead of old *lime* brought again to white heat; and all theſe differences produced no ſenſible effect on the point of *dryneſs*. Since that time Meſſ. NAIRNE and BLUNT, Mr. HURTER, and Mr.

HAAS,

Haas, have made apparatuses of the same kind, and I have made myself some others of different sizes and shapes; and they all produce sensibly the same degree of *dryness*.

6. The described apparatus was ready in the month of October, 1787, and I put in it one of my first *hygrometers*; which, in a few days, came to its fixed point of *dryness*, and there it has remained ever since, though I have opened the vessel above four hundred times. That degree of constancy, much beyond my expectation, has enabled me to make a variety of experiments, which else had been next to impossible: it proceeds partly from the great capacity of *quicklime* for *moisture*, which I shall determine hereafter; and partly from its slowness in receiving it; which circumstances, added to the small size of the openings, to their being at the top of the vessel, and to the care of putting in and taking out the instruments nearly at the same temperature, prevents the *lime* from acquiring any sensible degree of *moisture* during these operations.

7. I did not trust at first the apparent continuance of the same degree *of dryness* in that vessel. At the end of nine months of frequent use, I began to fear, lest the *whalebone*, of which the *standard hygrometer* is made, had been impaired; and I took it out, to try its point of *extreme moisture* (of which I shall speak hereafter;) but it came exactly to that point, and when put again into the lime-vessel it returned where it stood before. I have repeated many times that trial, with the same result; the last time was at the end of three years, when, instead of a loss of expansibility in the *whalebone*, I found a small increase, but probably accidental; it went a little farther than its point of *extreme moisture*, and came back to its constant point of *dryness*.

8. The

8. The principles of hygrometry being now my only object, it would not be proper to enter into particulars on its practical part; but I shall here mention for once, that the steadiest *hygroscopic* substances are subject to anomalies: for instance, after an hygroscope has remained fixed in *water* for many hours, if it is taken out, suffered to dry a little, and then put again into *water*, it may sometimes happen to overpass that point. In the same manner, after an hygroscope has been long fixed in the lime-vessel, it may happen also, that in taking it out only for a quarter of an hour, and putting it in again, it will move a little farther than it was before. Again, if in taking it out of the *lime-vessel*, where it had long remained fixed, it is put into *water*, and then back into the *lime-vessel*, it may happen, that it will fix itself a little short of its former point, and never move thence, except by repeated great variations of heat; but if, when it shews that disposition, it is taken out for a short time, and put in again, it will then attain its usual point. This was the case in the last trial of my *standard*. Lastly, the same anomalies may take place at every other point of the scale of every hygroscope, only more or less according to the substances; some of which, for that reason, cannot be used for practical hygrometry.

9. Those anomalies of the steadiest *hygroscopic* substances, will probably prevent our ever having in the *hygrometer* an instrument nearly so exact as the *thermometer*; and this I was to premise, that when I mention the results of particular *hygroscopic* experiments, it may be understood, that they have only the degree of exactness that belong to their class. Luckily those anomalies are yet of no consequence for the great objects of *hygrology* and *meteorology*; the present state of *hygrometry* being sufficient to excite on those objects, questions of great importance for natural

natural philoſophy. And in the mean time thoſe anomalies are very intereſting in themſelves; as, from their laws, they ſeem to point out ſome modification of *cohesion*, as being the immediate cauſe of *elaſticity* in *ſolids*. If I can find time to put in order a number of obſervations and experiments I have made in that reſpect, I intend to make it the ſubject of a Paper, in which I ſhall examine, from a general reſult of thoſe phænomena, the comparative uſe of *weights* and *ſprings* for keeping ſtretched the *hygroſcopic* ſubſtance in *hygrometers*.

10. After *fixity* in the degree of *dryneſs* produced by *lime* in the manner I have explained, the next point to be examined in reſpect of my theory, was, if the *nature* of the ſubſtance brought to a *white heat*, had any influence on the degree of *dryneſs* thereby produced; and in order to try at once the effect of a very great diſparity, I choſe ſuch a *ſand-ſtone* as is not affected by *acids*, and ſtrikes fire from ſteel, before and after having been *incandeſcent*. The firſt experiment I made was with a view of finding the comparative capacities for *moiſture* between that *ſtone* and *lime*. For that purpoſe I took ſuch pieces of them as might be readily reduced to half an ounce while *incandeſcent*; which being done, I put them into braſs cups, fitted to a ſcale, and I incloſed them under a glaſs veſſel inverted over *water*. I weighed thoſe ſubſtances from time to time; each of them continued to acquire *weight* during five weeks; at which time the *ſand-ſtone* had gained $\frac{1}{256}$ part of its original *weight*, and the *lime* $\frac{110}{256}$: this laſt was at that time all cracked and fallen in ſmall fragments, eaſily reduced to powder; the *ſand-ſtone* ſtruck fire as before. I next prepared a cylindrical tin veſſel, 10 inches in diameter and 14 inches high, with a glaſs top, which I filled with fragments of that *ſtone*, treated as the *lime*; and when it was cooled, I

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put into it an *hygrometer*, whofe fixed point of *drynefs* has been taken in the *lime* apparatus: and in five weeks it was fixed to the fame point. This is a demonftration, that the *nature* of the fubftance does not interfere with the *degree* of *drynefs* produced, and that *incandefcence* is the only caufe of its *fixity*.

11. Laftly, in refpect of *hygrometry*, a degree of *drynefs* thus determined might have been fufficient; but for *hygrology*, and even for natural philofophy in general, it was defirable to difcover if that *fixed degree* of *drynefs* was alfo *abfolute*: and the following are the confiderations which directed me in that enquiry: if *evaporation* is produced by *heat* only, and if *incandefcence* is the *maximum* of *heat*; an *hygrofcopic* body, which is brought to *incandefcence*, cannot contain any *evaporable* water; and if that body has fuch a mafs, as to be capable of abforbing all the water *evaporated* in a certain fpace, without acquiring any meafurable *moifture*, that fpace may be called *abfolutely dry*. Now, if an *hygrofcopic* fubftance which is inclofed in that fpace, contains any fenfible quantity of *evaporable* water, when *heat* increafes, that fubftance muft lofe a part of its *moifture* in the *medium*, and take it back by the diminution of heat. Confequently there was a means of difcovering, if *hygrofcopic* fubftances, reduced to the above degree of *drynefs*, ftill retain a fenfible quantity of *evaporable* water; it was that of obferving their *weight*, by changes of heat: and from thefe previous confiderations I made the following experiments.

12. I hung fucceffively to a very fenfible *beam*, fhewing the changes of *weight* by an *index*, different forts of vegetable and animal fubftances, the *beam* being inclofed in a glazed tin-veffel, containing a fufficient quantity of *quicklime*; and during the operations, I produced from time to time great changes in the *temperature* of the veffel. As long as thefe fubftances

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retained a ſenſible quantity of *evaporable* water, the increaſe of *heat* made them loſe ſome *weight*, which they regained partly when the *heat* returned to the ſame point. But that effect diminiſhed by degrees; and, at laſt, a change of 30° of *Fahrenheit* did not produce any ſenſible change of *weight* in thoſe ſubſtances, though they were ſuch as had a great capacity for *moiſture*. An *hygrometer* placed near the *beam*, was then at the point taken in the *lime-veſſel*. That ſingle experiment confirms all the previous conſiderations from which I had expected *abſolute dryneſs* from *incandeſcence*.

Of extreme moiſture.

13. The ſecond propoſition I had ſketched in my firſt paper, is this "that *water*, in its liquid ſtate, is the *only* ſure immediate "means of producing *extreme moiſture* in *hygroſcopic* bodies."

14. *Moiſture*, the nature of which we are firſt to determine, may be conſidered in three different caſes.—1ſt, In ſubſtances which have an *affinity* with *water*; by which their moleculæ and thoſe of *water* may unite, and form a new compound.—2dly, In ſubſtances which have no *affinity* with *water*, but to which *water* has a tendency to adhere; by which cauſe it enters their *capillary pores*.—3dly, In the *medium*, or ſpace free from viſible bodies. I have not undertaken to diſcover what, in the firſt of theſe caſes, might properly be called *moiſture*, and its *degrees*; as I foreſaw great difficulties in that undertaking, which beſides was unneceſſary to my principal purſuit: therefore I come immediately to the ſecond caſe.

15. When I wrote my work *Idées ſur la Météorologie*, having not yet made ſome experiments I had in view to verify the opinion I entertained, that vegetable and animal ſubſtances, as well

well as the porous mineral ones, received *water* merely by the faculty of *capillary pores*, I uſed ſometimes the expreſſion *hygroſcopic affinity*, in treating of the *hygoſcropic equilibrium*; but before that work was come out of the preſs, having converſed on that object with Dr. BLAGDEN, and found him inclined to that opinion, I had time and opportunity to expreſs it, in § 276, as follows: "There are reaſons to doubt, whether ſome "of the ſubſtances, which ſhare amongſt them the *water* diſ-"ſeminated in a ſpace, do not *ſuck* it, by a faculty ſimilar to "that of *capillary tubes*, without any *chymical affinity* with "*water*." That opinion will be now confirmed by the following experiments.

16. 1ſt. EXP. *Sugar* has an *affinity* with *water*, and no ſenſible one with *alcohol*: however, a lump of *ſugar* will *imbibe* this laſt *liquid* as readily as the firſt. Conſequently, *water* is not *imbibed* by ſugar in conſequence of their *affinity*; ſince *alcohol* is alſo *imbibed*: they both aſcend in *ſugar*, by the faculty of its *capillary pores*, as they do in *ſand-ſtone* or in *ſpunge*. But when *water* has thus entered *ſugar*, it *diſſolves* it; which then is a *chymical* effect; whereas *alcohol* evaporates, and leaves the *ſugar* ſenſibly as it was before.

17. 2d. EXP. If *water* penetrated *hygroſcopic* ſubſtances of the vegetable and animal kinds, by an *affinity* with them, it would not be natural to expect, that other *liquids*, which do not ſhew an *affinity* with the ſame ſubſtances as *water*, ſhould penetrate the above-mentioned ſubſtances. Being led by that conſideration, I made two *hygroſcopes* of different elaſtic *animal* ſubſtances; and after having marked the point where they ſtood in *water*, I immerſed them ſucceſſively in *alcohol* and in *ether*; by which *liquids* they were expanded nearly as much as by *water*, and they contracted as much in coming out of them.

In thofe experiments a fingular phænomenon happened in both hygrofcopes. The firft effect of the immerfion of thofe *animal* fubftances in *alcohol* (and I fuppofe it would have been the fame with *vegetable* ones) was *contraction*, foon followed by *expanfion*; and when they came out into the air, the firft effect was *expanfion*, foon followed by *contraction*. The caufe of that reciprocal phænomenon is undoubtedly the *affinity* of *alcohol* with *water*. In the immerfion, fome of the *moifture* came out of the fubftance, to unite with the furrounding *alcohol*; by which lofs of *water*, a *contraction* took place in the fubftance, 'till it came to imbibe the *alcohol* itfelf. In the immerfion, fome of the *moifture* of the air, uniting immediately with the *alcohol* retained by the fubftance, expanded it to the fame degree as if it had been in *water*; after which, the *alcohol* evaporating, the fubftance contracted. Thofe phænomena did not happen with *ether*; this not uniting readily with *water*; but it expanded thofe fubftances as much as *alcohol*, and nearly as much as *water*. From thofe phænomena we may conclude, that the penetration of animal fubftances by *water*, and confequently by *moifture*, is produced, as that of *fugar*, *fand-ftone*, and every other porous fubftance, by the faculty of *capillary pores*, without any *affinity* between them and *water*,

18. 3d. EXP. In that theory, of a mere *imbibition* of *water* by *hygrofcopic* fubftances of the *elaftic* kind, a circumftance, which feems to point out *affinity*, was to be explained; it is that of the *hygrofcopic equilibrium*. In view of that object, I made the following experiments, not new in themfelves, but directed to my purpofe. I took fome glafs tubes, of different fmall bores, which I firft bent in the fhape of fyphons; after which I cut them in the middle of the bent part. This was to enable me, to bring into exact communication the lower end of two tubes, though

though held in a vertical poſition, as an inverted ſyphon (Fig. 2.) The following are the experiments.—1ſt. Exp. When a column of the liquid had aſcended in one of theſe tubes; if I applied to it an empty tube of the ſame bore, the *liquid column* divided itſelf equally between them.—2d. Exp. When the empty tube was of a ſmaller bore, that *column* roſe more in it, than it ſunk in the other; and the contrary happened when the empty tube was of a larger bore.—3d. Exp. When ſome more *liquid* was ſupplied to the united tubes, it roſe in both, in proportion to the reſpective heights of the former *columns*.—4th. Exp. When a ſuperabundant quantity of *liquid* was ſupplied, it roſe to a *maximum* in each tube, and the heights of the columns increaſed in ſome proportion with their former heights.

19. Theſe known facts have a clear analogy with the *hygroſcopic equilibrium* in *elaſtic* ſubſtances.—1ſt. Case. When the quantity of *liquid* common to *capillary tubes* is not ſufficient for them to receive their reſpective *maximum*, they ſhare it between them, and the *equilibrium* takes place, when there is, in each of them, the ſame ratio between its ſpecific *capillary power* and the weight of the raiſed column. In the ſame manner; when the quantity of *water* diſſeminated in a ſpace, is not ſufficient, for ſeveral hygroſcopic ſubſtances to receive the *maximum* of *water* which they can contain in their *pores*, they ſhare it amongſt them; and the *equilibrium* is produced, when there is in each of them the ſame *ratio*, between its ſpecific *capillary power*, and the reſiſtance of their *pores* to be more dilated.—2d. Case. When there is a ſuperabundant quantity of *liquid* common to ſome *capillary tubes*, each of them receives its *maximum*; which is determined by an *equilibrium*, between its total *capillary power*, and the *weight* of the raiſed *column*. In the ſame manner, when there is a ſuperabundant quantity of *water* common

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to ſeveral *hygroſcopic* ſubſtances, each of them receives its *maximum*; which is determined by the *equilibrium*, between their total *capillary power*, and the reſiſtance of their *pores* to be more dilated. That final *equilibrium*, which, from its very nature, cannot be overpaſſed in any *elaſtic* ſubſtance properly uſed, determines the ſpecific *capacity* of thoſe ſubſtances for *moiſture*.

20. *Moiſture* then, conſidered in *porous* bodies not *ſoluble* by *water*, may be defined, "A quantity of *water*, which is "*inviſibly* contained in their *pores*; without any other connec- "tion with their ſubſtance, than that which it has with the "*glaſs* of the *capillary tubes* into which it has aſcended."

21. We may ſee now whence proceeds the *hygroſcopic equilibrium* between *elaſtic* ſubſtances incloſed in a ſpace, either filled with *air* or deprived of it. In this explanation it is unneceſſary to determine, how *water* is *inviſibly* diſſeminated in ſpaces free from viſible bodies, therefore I ſhall not enter here into this ſubject; that diſſemination is a fact admitted in every hypotheſis, conſequently the *medium* is only to be conſidered as the ſtock and ſtandard of *moiſture*. By the cauſe, whatever, of *evaporation*, *hygroſcopic* ſubſtances loſe or gain *water* in the *medium*, according to its degree of *moiſture*, till they are in *equilibrium* with it; which implies the *equilibrium* amongſt themſelves according to the laws reſulting from their own nature.

22. Now *moiſture*, in a general ſenſe, will appear to be, "a "quantity of *inviſible water*, either *evaporable*, or *evaporated*." And from that definition, the *maximum* of *moiſture* will exiſt, when, "every circumſtance remaining the ſame, no more "*water* can be admitted in a ſpace, without becoming *viſible*; "on *ſolid* bodies, by their ſurface being *wet*; and in the *medium*,

"by a ſpontaneous *precipitation* of *water*." Laſtly, as immerſing ſolid *hygroſcopic* bodies in *water*, or expoſing them in a *medium* where there is an actual *precipitation* of *water* (as in a *fog*) is an effectual means of furniſhing their *pores* with the whole quantity of *water* they can *imbibe*; it is evidently a *ſure* means of producing *extreme moiſture* in them: and this point cannot be overpaſſed, neither in *water*, nor in *fog*, ſince it depends upon the *reſiſtance* of the *pores* to further dilatation by the mere introduction of *water*; but it muſt be attained in an *hygrometer*, to fix its point of *extreme moiſture*.

23. When formerly I had fixed upon that method for procuring to my firſt *hygrometer* a true point of *extreme moiſture*, it occurred to me, that the *temperature* of *water* might influence ſenſibly the expanſion of its *ivory* tube; and in order to diſcover if it was ſo, I made ſome experiments, related in §§ 104 &c. of my former paper, the reſult of which was, that the *temperature* of *water* had a ſenſible effect on the expanſion of *ivory*. But ſoon after I diſtruſted ſome modifications of hat complicated *hygrometer*; and eſpecially this particular eſult.

24. I then changed that firſt method; which conſiſted in meaſuring the changes of capacity of hollow cylinders, into that of meaſuring the changes in length of *hygroſcopic* ſubſtances; and for ſome preliminary experiments on many of them, I made particular *frames*, in which, by a combination of *glaſs* and *braſs*, the effects of *heat* on thoſe materials compenſated each other; by which means the *indices* of thoſe inſtruments were only affected by the modifications of the *hygroſcopic* ſubſtances tried in them. I have mentioned theſe *frames* in a paper on *yrometry*, printed in the *Phil. Tranſ.* for 1778. With theſe *rames* I firſt tried *ivory*, in *water* of different *temperatures*; and

and I found a very little difference in its *expanſion*, comparatively with what had appeared from my firſt experiment. Then, continuing the ſame trial on various ſubſtances, I found the effect of different *temperatures* of *water* very ſmall in general; and even in ſome ſubſtances, as *deal* taken lengthwiſe, and *hemp*, I could not aſcertain any.

25. Theſe experiments led me to think, that the ſmall variations produced by *heat* in hygroſcopic ſubſtances dipt in *water*, were not *hygroſcopic* modifications, but the mere effects of *heat*, by the ceſſation of all *hygroſcopic* modifications; theſe having then attained their *maximum:* which is a diſcrimination of effects, that I had vainly attempted to produce by other means. When afterwards I had found the method of producing *extreme dryneſs*, I made a lime apparatus, for the purpoſe of repeating in it the ſame experiments with my *compound frames*; and I found that theory confirmed, by the effects of *heat* in that apparatus being nearly the ſame, on the ſame ſubſtances, as when they were in *water*.

26. From the whole of the foregoing experiments there cannot remain any doubt, that *water*, in its liquid ſtate, is a *ſure* means of fixing the point of *extreme moiſture* on *hygrometers*. Particularly, in reſpect of *elaſtic* ſubſtances, as *ivory*, *quill*, *whalebone*, all ſorts of *wood*, and a number of others which I have tried, the laſt experiments in *water* of different *temperatures*, afford an immediate proof, that their faculty of *ſucking* water has a fixed limit, proceeding from a final *reſiſtance* of their *pores*, to be more dilated by the introduction of *water*. Conſequently, their *utmoſt expanſion* is a true ſign, that *moiſture* is *extreme* in them; which point cannot be exceeded. But my propoſition extended farther: I had ſaid, that *water* was the *only* certain means of obtaining immediately the point of

extreme *moiſture* on *hygrometers*; and this is a moſt important queſtion, both of *hygrometry* and *hygrology*, which remains to be examined.

On the maximum of evaporation, *and its correſpondence with the* maximum of moiſture *in a* medium.

27. Since *moiſture* conſiſts in *inviſible* water, an *exceſs* of *water* is the only immediate means of aſcertaining that the *maximum* exiſts; as, if a reſervoir is above our reach, the only means of knowing if it be *full* of water, is when it overflows. From that principle, a *fog* gives the point of *extreme moiſture* on *hygrometers*, like *water* itſelf; becauſe it covers very ſoon the *hygroſcopic* ſubſtance with a coating of *water*: ſometimes even it expands it a little more than an immediate application of *water*; but this belongs to an object that I have waved before, as relating to ſome modifications of *elaſtic ſolids* (§ 8.). No other means then but an *exceſs* of *water* over the ſurface of the *hygroſcopic* ſubſtance of the *hygrometer*, can aſcertain that it is arrived at its point of *extreme moiſture*; and the firſt immediate demonſtration I ſhall give of it, will be afforded by *dew*; a very uncertain, though apparently certain ſign of *extreme moiſture* in the *air*. We ſay, that there is *dew*, when ſome ſolids expoſed in the open air in a clear evening are *wet*; but if that was the effect of a *precipitation* of *water* happening in the *air*, all the ſolids thus expoſed would be *wet*; which is far from being the caſe; conſequently, that phænomenon muſt proceed from ſome particular cauſes, by which, though no *water* is yet diſpoſed to abandon the *medium*, it gathers on ſome particular ſolids. It is very long ſince the phænomena of *dew* have perplexed natural philoſophers; and they were the firſt

which I ſtudied in the beginning of my reſearches in meteorology; but all that I concluded from my experiments and obſervations was, that we could not underſtand thoſe phænomena without firſt having a ſure *hygrometer*. This is the reaſon why, ſoon after I had made my firſt *hygrometer*, I expoſed it in the open air in the country, ſuſpended very little above the graſs, from the morning of a fine day to the time of *dew* in the evening; the *graſs* grew *wet*, and the *hygrometer* remained at a great diſtance from the point which had been fixed in *water*. I have related that experiment in § 91, of my firſt Paper.

28. When I had made hygroſcopes of various ſorts of *ſlips*; for inſtance, of different *woods* and of *whalebone*, cut acroſs the fibres; of *ivory* and *horn*, reduced firſt into thin tubes, and then cut in ſcrew; and of *quills*, by cutting alſo in ſcrew their barrels; I repeated, with thoſe inſtruments, my obſervations on *dew*; and to give a ſhort, but determinate idea of the phænomena I obſerved, I ſhall reduce them to ſome general caſes, as indicated by one only of thoſe *hygroſcopes*, that of *quill*, which, like all the others, is divided into 100 parts, from *extreme dryneſs* to *extreme moiſture*. Theſe hygroſcopes were ſuſpended in the open air, three feet above a graſs-plat in the country. 1ſt Case. When a clear and calm evening ſucceeds to a clear and warm day, the *graſs* frequently grows *wet*, though the above *hygroſcope* ſtands many hours, and ſometimes the whole night, between 50 and 55. 2d Case. If the *dew* increaſes, ſo that taller herbaceous *plants* and *ſhrubs* grow *wet* in ſucceſſion, the *hygroſcope* moves more and more towards *moiſture*; and when it is come to about 80, plates of *glaſs* and *oil-paint* alſo grow *wet*; but at that period, neither *metallic* plates, expoſed like the glaſs ones, nor ſome *ſhrubs* and *trees*, are *wet*; and this alſo may laſt whole nights. 3d Caſe. If the

dew

dew proceeds to its *maximum*, the *hygroſcope* moves from 80 to 100 (and ſometimes a little farther, § 27.). Then we have alſo a certain proof that *extreme moiſture* exiſts in the air; for every ſolid body expoſed to it is *wet*. But it is only at that moment that we can depend on *extreme moiſture* exiſting; for, if in the other deſcribed ſtages of the phænomenon, the appearance of *water* on the ſurface of ſome ſolids had proceeded from a ſpontaneous precipitation in the air, all the other ſolids ought to have been *wet*; but they only become *wet* in a certain ſucceſſion, and in the mean time the ſlip of *quill*, and all the other above-mentioned *hygroſcopes*, move more and more towards their point 100, in ſign of *moiſture* increaſing in the air. Conſequently (as I had concluded from my firſt obſervations), inſtead of having in *dew* an *hygroſcopic* ſtandard for the *hygrometer*, we have in its phænomena many circumſtances which will only be explained with the aſſiſtance of that inſtrument.

29. Some previous obſervations had alſo warned me againſt the general idea, that *moiſture* was to be *extreme* in the *air*, when there was a ſufficient quantity of *water* in the ſpace, even though that *air* might be ſuppoſed to be filled with *evaporated* water to its *maximum*; and the doubts I entertained in that reſpect were the cauſe of the difficulties I expreſſed in the beginning of my firſt Paper, which I only got over when I thought of *water* itſelf, to obtain the point of *extreme moiſture* on my *hygrometer*. This was alſo the reaſon why, as ſoon as my firſt *hygrometer* was finiſhed, I placed it in a cellar, the walls and ground of which were *wet*, and where it continued two months, without ever attaining its point of *extreme moiſture*. I have related that experiment in § 54. *et ſeq.* of my firſt Paper.

30. When alſo I had the *hygroſcopes* mentioned above in the obſervations on *dew*, I undertook a very long courſe of various ſorts of experiments on that important point of *hy-*

grology, of which however I ſhall only give here the general and conſtant reſults, as furniſhed by thoſe hygroſcopes. "The "*maximum* of *evaporation* in a maſs of incloſed *air* is far from "being identical with the *maximum* of *moiſture*; this being "dependent alſo, even to a very great degree, on the *tempera-* "*ture* of the *ſpace*, ſuppoſed to be the ſame, or nearly ſo, as that "of the *water* which *evaporates* in it. *Moiſture* may arrive to "its *extreme* in an incloſed *air*, if that common *temperature* "is near *freezing point*; but it becomes leſs and leſs, even to a "very *dry* ſtate, as that *temperature* riſes, though the *product* "*of evaporation*, thereby increaſing, continues to be at its dif- "ferent *maxima*, correſpondent to the different *temperatures*." This is a very important propoſition in *hygrology*; which, from my experiments, would not be ſubject to any objection, if there were no other *hygroſcopes* than thoſe I have mentioned above, of which I have thirteen different ſpecies; but there is another claſs of ſuch inſtruments, from which ſome doubt might firſt ariſe; and I come now to that point.

On two diſtinct claſſes of hygroſcopes.

31. As I ſhall now frequently ſpeak of *ſlips* and *threads*, which conſtitute thoſe two *claſſes* of *hygroſcopes*, I muſt firſt explain what I mean by thoſe words. The *ſlips* compoſe the claſs of *hygroſcopes* uſed in the above experiments; they conſiſt of very thin and narrow *laminæ* cut *acroſs* the fibres of vegetable or animal ſubſtances, either in their natural or artificial breadths (as *boards*), or by reducing natural or artificial thin tubes of them into *helices*. By *threads* I mean the ſame kinds of ſubſtances taken *lengthwiſe*, either from their being naturally in thin threads, or by reducing them to that ſtate, in

tearing from them thin *faſciculi* of *fibres*; which operation is eaſy in ſome, as *hemp*, *whalebone*, and *gut*, but very difficult in others, as *quill* and ſome ſorts of *wood*.

32. The firſt *hygroſcopes* of the claſs of *threads*, which I obſerved comparatively to the claſs of *ſlips*, were of *hemp*, *gut*, *whalebone*, and ſome *woods*, and they exhibited a phænomenon which at firſt I could not underſtand: when they were expoſed with the *ſlips* in *damp* air; as, for inſtance, in open air during the ſecond period of *dew* above determined, or in a glaſs veſſel inverted over *water*; the *threads* had only very ſmall motions backwards and forwards round their point determined in *water*, while the *ſlips* had conſiderable motions within that point, without coming near it, if the *temperature* was ſenſibly above *freezing point*. Thence ariſes the objection againſt the general propoſition above ſtated: for theſe two claſſes of *hygroſcopes* contradicting one another on the changes of *moiſture*, nothing could be aſſerted in that reſpect, till there were ſufficient reaſons to exclude one of thoſe claſſes of informers, and to truſt the other claſs.

33. Proceeding to multiply the ſpecies of thoſe two claſſes of *hygroſcopes*, I found always the ſame fundamental *march* in *ſlips*, all of them conſtantly moving in the ſame direction; but in multiplying the ſpecies of *threads*, I found ſuch variety between them, that in their own claſs they created diſtruſt; ſome of them, as of *deal*, *aloes pitta*, *liber of lime tree*, *quill*, and thin *ſtems of gramen*, in coming out of *water*, increaſed in *length*: they went farther that way, to a certain point, as the air was dryer: they *retrograded* then with accelerated ſteps, when *dryneſs* increaſed, thereby returning to the point where they had ſtood in *water*; and they continued to move in the ſame *retrograde* way, with great acceleration, by a ſtill in-

creaſing

creaſing *dryneſs*. Moreover, they did not follow one another in thoſe motions contradictory to the evident *march* of *moiſture*: each of them changed direction at different periods, thereby often contradicting alſo each other, while the *ſlips* conſtantly agreed together in the direction of their motion, and alſo with all the other ſymptoms of *moiſture*. From theſe comparative phænomena I firſt concluded; that the motions of the ſame kind, which I had obſerved in the firſt-mentioned *threads*, were alſo *anomalies*, proceeding, only to a ſmaller degree, from a cauſe of the ſame nature as that of theſe laſt. After which, ſimilar ſymptoms, which I had formerly obſerved in the *water* thermometer near the *freezing point*, made me firſt conclude, from a general analogy; that the perceivable modifications of the *threads*, were the compound effects of two contrary operations of *moiſture* which followed different laws.

34. Another phænomenon led me ſoon after to a more determined theory in reſpect of thoſe two oppoſite effects of *moiſture* on *threads*. I have ſaid above, that *hemp* and *gut* have only a very little *retrogradation*; their greateſt difference from the *ſlips* conſiſting in their being *ſtationary*, while the *ſlips* have ſtill great motions. But when theſe ſame *threads* are *twiſted*, they acquire a very ſenſible *elongation* beyond their point of *extreme moiſture* ſucceeded by *retrogradation*. From ſeveral trials I have made in *twiſting* theſe *threads* more and more, I do not conſider as impoſſible, if ſome difficulties, which I only could obviate in part, were completely prevented, that they might be brought to ſuch a ſtate, as to have their point of *extreme* dryneſs coincide with that of *extreme moiſture*; by which means, in the progreſs of *moiſture* from one *extreme* to the other, they would move firſt in one direction with decreaſing ſteps, then in the oppoſite direction by increaſing ſteps; the whole, however,

with great irregularities. Here then we fee two *oppofite* effects of *moifture*; one which *lengthens* the *fibres*; the other which, by *fwelling* the *twifted* ftrings, *fhortens* them; and we fee thofe effects follow different *laws*, from which is produced a *retrogradation* that we may change *ad libitum*.

35. Now, the texture of animal and vegetable *fibrous* fubftances muft be a fort of *reticle*, which exifts in thofe which are naturally in thin *threads*, and in the moft minute *fafciculi* that we can feparate from a mafs; and we fee it in the laft cafe, for in fubdividing thofe *fafciculi*, there are always *fibres* breaking in the points where they were anaftomofed with others; confequently, the primary *fibres* of thofe fubftances form between them *mefhes* fimilar to thofe of a *net*; and thofe *mefhes*, which are *widened* by the introduction of *water*, muft produce in the *threads* the fame effect as the *twift* in the above *ftrings*.

36. If then *moifture*, in acting on vegetable and animal *threads*, natural and artificial, produces on their *length* two oppofite *effects*; one of which, fmall at firft but increafing gradually, *compenfates* at fome period the other which is firft vifible, and *furpaffes* it afterwards, fooner or later, according to the nature of the *threads*; it is evident, that they cannot be proper for the *hygrometer*; fince, from the indication of fome of them it might fometimes be concluded, that *moifture* changes in one fenfe, while it really changes in the contrary fenfe; and from fome others, that *moifture* is *extreme*, long before it is really fo. As for the *flips*; fince *moifture* has only one effect on their *length*, that of *widening* more or lefs the *mefhes* of the *crofs fibres*, I concluded; that all their *hygrofcopic* indications, in every part of their fcale, were *true* in refpect of *increafe* and *decreafe* of *moifture*; and that confequently, that clafs of *hygrofcopes* might be depended upon on that important point.

point. As for the exact *ratio* between the *indications* of thofe laft *hygrofcopes*, and the *changes* of *moifture*, that was to be the object of a particular inquiry, to which I now come.

Of the fcale *of the* hygrometer *between the two* fixed points.

37. The long attention I had formerly given to the comparative *expanfions* by *heat* of various kinds of *liquids* and *folids*, made me expect the variety I afterwards found in the modifications of *hygrofcopic* fubftances by *moifture*; therefore, as I expreffed it in § 2. of my firft Paper, my view was only at that time, to find fome means of producing a fteady *comparable* hygrometer; but afterwards I pointed out, in § 72, a means which had occurred to me, for attempting to find the *ratio* between the *expanfions* of fome determined *hygrofcopic* fubftance, and the correfpondent *increafes* of *moifture*; which was, to compare the firft with the correfpondent changes of *weight*, of the fame or of any other fubftance; an idea which I did not then much fcrutinize, not yet thinking of its execution.

38. From what I have faid above, I did not want any other motive of choice between the *flips* and the *threads* than their comparative *marches*; but though the *flips* agree always in the *direction* of their motions, there are differences in the progreffion of their comparative fteps; and that difference led me to examine more attentively the above means of finding which of thofe *marches* agreed beft with that of *moifture*. The refult of that examination was diftruft, at leaft in refpect of an immediate decifive means. With the view of rendering the changes of *weight* more eafily meafurable, I had firft thought of fome fubftance poffeffed of a ftrong *affinity* with *water*; but on con-

fidering

fidering the *hygrofcopic* phænomena of thofe fubftances, it appeared to me, that their changes of *weight* could not be proportional to the degrees of *moifture* in the *medium*; and that even the fenfe of the word *moifture* applied to them was very difficult to determine (§ 14.). As for the fubftance of the *hygrometer* itfelf, I did not find any reafon to think, that its changes of *weight* could be more proportional to *moifture* than its degrees of *expanfion*; fince on thefe laft depended in part the quantity of *water* that could be admitted into its pores at each degree of *moifture* in the *air*.

39. Being thus difappointed in my firft fcheme, I thought of a more direct method, which was to act on *moifture* itfelf, by firft producing, in a glafs veffel containing an *hygrofcope*, as much *drynefs* as I could conceive poffible at that time, and then introducing into it fucceffive equal quantities of *water*, for which I had found a fure means without opening the veffel. But then again fome previous experiments deftroyed my confidence in that method; having found,—1ft, That the evaporated *water* had a tendency to depofit itfelf againft the *glafs* by the fmalleft difference between the infide and outfide *temperatures*, even to the degree of becoming vifible on fome part of the veffel, long before I had any reafon to expect *extreme moifture*.—2d, That by the common *temperature* of my room, the *hygrofcope* in the veffel remained always at a confiderable diftance from its point of *extreme moifture*, though the bottom of the veffel was covered with *water*; and that it varied with the *temperature*; which could not have happened if *moifture* had been *extreme*.—3d, That when I endeavoured to increafe *moifture* in the veffel by cooling it, I produced very often the contrary effect, at the fame time that a quantity of the diffeminated *water* gathered over the glafs.

 40. As

40. As in thofe trials the contradiction between the *marches* of the *flips* and the *threads* was very evident, I was the more difappointed to find, that the uncertainty of the real degrees of *moifture* increafed at that very period: for inftance, fetting out from fome *dry* point, and *moifture* increafing, the *march* of a *thread* of *whalebone* was evidently in a *decreafing* progreffion, comparatively to that of a *flip* of the fame fubftance; and when at laft there was a fuperfluous quantity of *water* in the veffel, and the *temperature* was made to change, the *thread* was almoft *ftationary*, while the *flip* had confiderable motions, often contrary to the fmall motions of the other; while it was at that very period, that the real degrees of *moifture* in the veffel could not have been afcertained but by an already fettled *hygrometer*. I had no doubt that thofe anomalies were to be attributed to the *thread* of *whalebone*, not only becaufe of the excefs of the fame modification in other *threads*; but alfo confidering the analogy between the comparative *marches* of the two kinds of *hygrofcopes*, and thofe of the *thermometers*, of *water* and *quickfilver*: but as this was a very important object for natural philofophy, I would not decide it from thofe firft appearances; and that confideration led me to a very great number of various forts of experiments, which I made in view of multiplying at leaft the indirect facts with which my theory might be compared. However, as at laft I found a more direct means of verification, I fhall only mention that laft clafs of experiments.

Experiments on the comparative changes of weight *and* dimensions *of some* hygroscopic substances.

41. I have said above, that I could not find any solid reason to consider the changes in *weight* of a substance, as being more proportional, than its changes in *dimensions*, to the correspondent changes of *moisture* in the *medium*; which doubt had prevented me from undertaking that course of experiments. But it occurred to me at last, that if my theory on the comparative *marches* of the slips and the threads were true, it might be rendered certain by comparing those marches with the increase of *weight* of the same substance: for instance, taking a *slip* and a *thread* of *deal*, and having some *deal* hung to a scale, I was to find, that while the *slip* continues to lengthen, and the thread to shorten, the substances continue to receive *water*; and, in general, that the *march* of *slips*, in every part of their scale on which the experiment may be regular, was more proportional, than that of *threads*, to the correspondent changes in *weight* of every *hygroscopic* substance of the *elastic* kind.

42. This having struck me as a sure means of deciding the question, I set immediately to work; and since that time I have made a great number of experiments of that kind. They were not at first very accurate; but successively I have mended both the instruments and the apparatus; and, after having settled every part that I looked upon as essential, and made in consequence a new apparatus and new instruments, I have begun a regular course of experiments, of which I shall give here the first results.

43. The apparatus consists in two tin vessels; the first of which, and the most used, is $16\frac{1}{2}$ inches high, $15\frac{1}{2}$ wide, and 5 deep. The front of this vessel is a plate of glass, and the back a tin-plate *slider*, which, being taken off, leaves that side

 of

of the veſſel quite open. The ſecond veſſel has the ſame dimenſions as the firſt; but its back is ſoldered, and its front is of woven braſs wire. This veſſel may be applied to the back of the former, in ſuch manner as to make of both one ſingle veſſel, which, when the *ſlider* of the fore-part is taken off, is only divided by a vertical partition of woven braſs wire. The uſe of that ſecond veſſel is to produce *extreme dryneſs* in the other; for which purpoſe it is filled with large pieces of *quick-lime* taken from the kiln. When that veſſel is not uſed, it is kept in a tin box which it fills intirely; and when it is in, as well as while it is out for uſe, that box is kept ſhut with putty, by which means the ſame *lime* may ſerve many times in the following manner.

44. When I want to produce *extreme dryneſs* in the firſt veſſel, I apply to it the ſecond, faſtened with hooks; I then pull out the *ſlider* of the firſt, and ſtop with putty the chinks between them. When that firſt operation is completed, I put again the *ſlider* to the fore-veſſel, and take off the other. In this laſt operation, ſome *moiſture* might be introduced through the chinks of the ſlider, before they are again ſtopped with putty; eſpecially as the deſtruction of *moiſture* in the veſſel has made room for more *air* to come in; but I prevent it by making firſt the apparatus ſenſibly warmer than it was when I put on the lime-veſſel; by which means, in the little time employed for the operation, the motion of the air is from the inſide to the outſide, which prevents all acceſs of *moiſture* in the veſſel.

45. To that firſt operation ſucceeds that of a gradual introduction of *moiſture* into the apparatus. It would have been uſeleſs to apply to that proceſs the means I had imagined, for ſpreading ſucceſſive equal quantities of water in a cloſe veſſel; for the evaporated *water* depoſiting itſelf in part, more or leſs, on every ſurface, and the ſurfaces, eſpecially of *glaſs*, being very

very much multiplied in that apparatus when filled with inſtruments, no real correſpondence could be expected between the quantity of evaporated *water* and the motion of the inſtruments; therefore I again gave up the view of determining that *ratio*, and proceeded in the following manner. At the bottom of the apparatus, on one of its ſides, there is an opening, ½ an inch high, and 2½ inches broad, which uſually is kept ſhut with a tin plate and putty. The taking off, only for a moment, that tin plate, is the firſt operation by which I introduce *moiſture* into the apparatus; which being then reduced to the *temperature* at which I make all my obſervations, namely, 60° of FAHRENHEIT, permits the external air to enter the veſſel, and come to an *equilibrium* with it at the ſame temperature. By keeping off the tin plate longer and longer, I admit new quantities of *moiſture* into the veſſel; and when that means is become ineffectual, I introduce through the ſame opening, a braſs frame, which extends under all the inſtruments, on which is ſtretched a cloth that I *wet* by degrees more and more, as long as it may produce ſome effect at the above *temperature*. In order to have that *temperature* when I want it, I make thoſe experiments in a ſeaſon when I may have fire in a ſtove at a proper diſtance from the apparatus. The time when that equal *temperature* is neceſſary in many reſpects, is that of the obſervations, which I make twelve hours after each new introduction of *moiſture*.

46. The inſtruments I place in that apparatus are of two kinds; the firſt of which are *beams*, made on the principle uſed by Mr. JOHN COVENTRY in his *paper hygrometer*, which principle I have found of great uſe; for, with *beams* of that ſort, as delicate as mine are, if the total change of weight in an experiment is not above 1 grain, $\frac{1}{1000}$ part of it may be diſtinctly obſerved on a weight from 3 or 4 to 20 grains; but in my experiments,

in which the total variation was from 5 to 6 grains, the obſervable part was only $\frac{1}{100}$ part of a grain. Theſe two *beams* are placed on the ſame line through the middle of the depth of the veſſel, and their indices move in that plane; their motions are in an oppoſite ſenſe by the ſame changes of weight, becauſe I wanted the two ſubſtances ſuſpended to the *beams*, to hang near one another in the middle of the veſſel. The other inſtruments are *frames*, in which an *index* is moved by the variations in *length* of a very thin *ſlip* or *thread*. Theſe frames are placed before and behind the *beams*.

47. In the firſt experiment that I made with that apparatus, the ſubſtances ſuſpended to the *beams* were *deal* and *quill*, reduced to very thin *ſhavings*, which were ſtretched edgewiſe in thin braſs-wire frames. The weight of each kind of theſe *ſhavings* was 12 grains, at a certain degree of the thermometer and of my hygrometer. The other *hygroſcopes* were *ſlips* and *threads* of the ſame ſubſtances as the *ſhavings*, and alſo of *whalebone*. Theſe ſix laſt inſtruments have their point of *extreme dryneſs* taken in my *lime-veſſel*, and their point of *extreme moiſture* in *water*; the interval between theſe points is divided into 100 parts; and on the *ſcales* of the *threads* the *degrees* are prolonged beyond this laſt point. The *hygroſcopic* ſcale of the *ſhavings* could not be fixed before the operation; therefore the ſcales of the beams ſerved only to indicate the comparative motions of the index; but afterwards, taking for 0, the point where the *index* ſtood by *extreme dryneſs*, and for 100, the point of *extreme moiſture*, which I ſhall explain, the interval between theſe two points became a *modulum* by which I have alſo reduced into 100 parts of the *whole* the changes obſerved in the *weight* of the *ſhavings*. I ſhall not give here the abſolute quantities, either of the changes of *weight*, or

of

of thoſe in *length* of the other *hygroſcopes*, having not had time to make the neceſſary calculations, of which however I have the *data*.

48. The neceſſary time for a complete diffuſion of the newly introduced *moiſture* in the veſſel renders it impoſſible to proceed, in that introduction, by regular ſteps. The method I uſe is, to obſerve the motion of my uſual *hygrometer*, which is a *ſlip* of *whalebone*, and to remove the cauſe of increaſe of *moiſture* before it has moved 5 degrees. In that manner the ſteps of the increaſing *moiſture* have been in general leſs than 5 degrees of that inſtrument; but, by interpolation, I have reduced them to what they would have been if the ſame inſtrument had been moved ſucceſſively 5 degrees.

49. I have ſaid before, that when the *maximum* of *evaporation* is produced in a cloſe veſſel by a *temperature* ſenſibly above the *freezing point*, there is no regularity to be expected in any farther attempt to increaſe *moiſture*; the diſſeminated *water* being then abundant, the ſmalleſt difference of temperature between different parts of the apparatus makes it depoſit itſelf on ſome ſurface, and paſs from one to the other (§ 39.); which circumſtance is alſo mentioned by Profeſſor PICTET of Geneva, in his late work, *Eſſais de Phyſique*. For that reaſon, when the *evaporated water* in the veſſel was near its *maximum*, my laſt operation was, to put in again the *wet* cloth, while I kept the *temperature* at 60°, and to take it out when the *indices* of the *beams* were fixed. My uſual *hygrometer* was then at 87°; and as it had ſtill 13 *degrees* to move towards its point of *extreme moiſture*, and all the others in proportion of their known *marches*, I have added to the obſerved increaſes of *weight* in the *ſhavings*, a quantity proportional to their former marches

comparatively to that of their refpective *flips*; and thus are completed their *hygrofcopic fcales*. Repeated trials have fhewn me, that the *weight* of the *fhavings* increafes as long as their *flips* increafe in *length*; but as there is no regularity in their comparative *marches* at that period of *moifture* in a veffel, nor any poffibility of making thefe experiments in open air, becaufe of the *beams*; the addition mentioned, which forms the three laft *terms* of the columns of the *fhavings* in the following table, is to be confidered only as having determined the *modulum* of the *obferved* terms; fince it has not changed the *ratio* between them, nor confequently the correfpondent *marches* of *weight* and *length* fo far; which were the only object of the experiment.

50. Before I come to the general refult of that experiment, I fhall place here a comparative view of the two kinds of phænomena, which, by their analogy, led me firft to the theory I have expofed: I mean the comparative *marches* of the *flip* and *thread* of *whalebone* on one fide, and thofe of the *thermofcopes* of *quickfilver* and *water* on the other fide. In this table the correfpondent *terms* of the *hygrofcopes*, from 0 of both to 85 of the *flip*, have been obferved in the above experiment; the four following are the refults of obfervations in time of increafing *dew*. The correfpondent *terms* of the *thermofcopes* are deduced from the *table* of their comparative *expanfions* which I have given in § 418. *m.* of my work, *Rech. fur les Mod. de l'Atmofphère*; from which *table* this only differs, 1ft, by a change of the *modulum*, in the *ratio* of 80 to 100; 2d, by an *inverfion*, which brings the *terms* of this table to exprefs comparative *condenfations* of the two *liquids*.

HYGRO-

	HYGROSCOPES.			THERMOSCOPES.	
	Whalebone Slip.	Whalebone Thread.		Quickfilver.	Water.
Extreme	0	0,0 *dryneſs.*	*Boiling*	0	0,0 *point.*
	5	12,1		5	9,3
	10	30,1		10	18,3
	15	41,1		15	26,3
	20	51,1		20	35,0
	25	59,1		25	42,7
	30	65,6		30	49,2
	35	71,1		35	56,7
	40	76,5		40	63,1
	45	81,8		45	69,0
	50	85,8		50	74,5
	55	88,8		55	79,1
	60	91,3		60	83,8
	65	93,3		65	87,9
	70	95,6		70	91,8
	75	97,6		75	95,0
	80	98,6		80	97,5
	85	99,6		85	98,9
	90	100,1		90	99,9
	95	100,5		95	100,5
Extreme	100	100. *moiſture.*	*Freezing*	100	100. *point.*

51. The firſt part of that table ſhews, the great ſteps of the *thread* of *whalebone*, comparatively with thoſe of the *ſlip* of the ſame ſubſtance, at the beginning of their correſpondent increaſe in *length*; but the *thread* relents by degrees, and a nearly *ſtationary* ſtate ſucceeds, in which, while this *hygroſcope* moves firſt 1,9 *degree* from 98,6 to 100,5, then *retrogrades* 0,5

 degree

degree to come to its point of *extreme moiſture*; the *ſlip*, continuing to move in the former direction, goes over 20 *degrees*. The phænomena are the ſame in the part of the table relating to the *thermoſcopes*; that of *water* proceeds alſo at firſt by great ſteps, comparatively to that of *quickſilver*; after which it relents, and while it moves only .3 degrees, and *retrogrades* 0,5 degree to come to the *freezing point*, the *quickſilver* one, continuing to move in the ſame direction, goes over 20 degrees. It was from that phænomenon of the *water thermoſcope* that I concluded formerly, that there was in its *march* a *ſtationary* ſtate, during which the *heat* decreaſed nearly as indicated by the *quickſilver* one; and that concluſion was afterwards confirmed by direct experiments. From that aſcertained fact, I was led to conclude, with reſpect to moiſture, that there was alſo a *ſtationary* ſtate in the *march* of hygroſcopic *threads*, even in thoſe which had the ſmalleſt *retrogradation*, as that of *whalebone*; and this theory will be confirmed by the reſults of the above-deſcribed experiment.

52. The following table contains thoſe reſults, namely, the correſpondent *marches* of all the mentioned *hygroſcopes*; the *ſhavings* increaſing in *weight*, and the *ſlips* and *threads* in *length*. The 3 laſt comparative *terms* do not reſult from that particular experiment; for the *ſhavings* (as I have ſaid above) they are concluded from the former comparative ſteps; for the other inſtruments, they have been obtained by obſervations in the open *damp* air.

WHALE-

	WHALEBONE.		QUILL.			DEAL.		
	Slip.	Thread.	Shavings.	Slip.	Thread.	Shavings.	Slip.	Thr.
Extr. dryn.	0	0,0	0,0	0,0	0,0	0,0	0,0	0,0
	5	12,1	7,0	4,8	40,0	6,2	5,4	42,0
	10	30,1	13,0	9,7	72,0	9,4	11,2	69,4
	15	41,1	20,0	14,4	85,0	15,6	16,5	94,8
	20	51,1	26,0	19,2	95,0	22,6	21,9	107,0
	25	59,1	31,0	23,9	101,0	27,0	27,2	113,6
	30	65,6	36,0	28,5	105,0	33,2	32,7	118,6
	35	71,1	42,0	33,3	107,0	36,0	38,3	122,6
	40	76,5	43,8	38,3	102,0	41,2	43,7	120,6
	45	81,8	48,3	42,9	104,0	46,7	49,2	123,6
	50	85,8	52,3	47,4	107,0	49,7	54,6	126,6
	55	88,8	56,5	52,4	103,0	56,1	59,9	119,7
	60	91,3	60,5	56,9	105,0	59,9	64,9	122,7
	65	93,3	64,4	61,9	106,0	63,7	69,7	119,7
	70	95,6	69,4	67,2	108,0	67,1	74,5	117,6
	75	97,6	74,0	72,2	107,0	73,4	79,0	115,6
	80	98,6	78,0	77,8	106,0	78,1	83,5	112,6
	85	99,6	84,0	82,8	105,0	83,8	87,5	110,0
	90	100,1	* 88,0	88,2	103,6	* 88,8	92,0	107,0
	95	100,5	* 94,0	94,0	102,0	* 93,8	96,0	103,6
Extr. moiſt.	100	100.	*100.	100.	100.	*100.	100.	100.

From the 18 firſt *terms* of that table, which are the immediate reſults of the experiment, we are now to examine my opinion, that the *lengthening* of the *ſlips* of *whalebone*, *quill*, and *deal*, beyond theſe *terms* is a ſure ſign that, till they have attained their point 100, *moiſture* continues to increaſe in the *medium* where they are placed.

53. There could not be any doubt on that propoſition, if it were not for ſome *threads* ſimilar to that of *whalebone*; which *threads*, having ſenſibly attained their utmoſt *length* at the period when the experiment was ſtopped, ſeem to indicate, that

moisture is then at its *maximum*. But if the *lengthening* of *hygroscopic threads* in general, is the compound effect of two opposite causes which follow different laws; it may be that, in some *threads*, those causes happen to compensate each other at that very period; by which means they are *stationary*, though *moisture* continues to increase. This was my opinion, and the above experiments were undertaken to verify it, first, by comparing the *march* of the *slips* with the increase in *weight* of their own substance; secondly, by comparing the *marches* of different kinds of *threads* with each other, and also with the increase in weight of their substance: and from that now we are to examine the above proposition.

54. In respect of the *slips* my theory is, that as *moisture* cannot act on their length but by widening the *meshes* of their transversal *fibres*, they cannot go on *lengthening* but by imbibing more and more *moisture*, from its increase in the air; and this we see to be the case, by comparing the *marches* of the three kinds of *slips* with the correspondent increases in *weight* of the *quill* and *deal* shavings, during the whole progress of the experiment. There are differences in those *marches* as I expected (§ 37.); but they are not such as to give the smallest reason to suspect, that afterwards, during the period of the three last *terms* of the table, in which we have no correspondent observations of increases of *weight* in the *shavings* of *deal* and *quill*, the same law does not take place as in the 17 antecedent *terms*. If the experiment was only made with one kind of *slip*, it might be objected, that though that *slip* lengthens regularly during the whole increase of *moisture* from its *minimum* to its supposed *maximum*, it is not impossible but that immediately after, by some peculiarity of its nature, it will *lengthen*, without any farther increase of *moisture* in the

medium.

medium. But that furmife cannot be admitted when the *flips* of fuch diffimilar fubftances as *whalebone*, *quill*, and *deal*, fenfibly agree in their *motions* at that period, and when a number of other *flips* of the vegetable and animal kinds follow alfo the fame general *march*.

55. In refpect of the *threads*, which are the only caufe of the above doubt, my theory, which removes it, is alfo confirmed by that experiment. That caufe of doubt is exemplified in the table by the *thread* of *whalebone*, which has almoft no motion, while its *flip* moves from 85 to 100. At that period of *moifture*, no regularity can be expected from experiments made in *clofe veffels*; by which circumftance, not having the correfpondent obfervations of *weights*, it cannot be demonftrated by immediate experiments, that the *thread* of *whalebone* is then *ftationary* from its nature; but the *threads* of *quill* and *deal*, which are in that ftate during the regular courfe of the experiment, will guide us in that enquiry. The *thread* of *quill* is *ftationary* during that great part of the obferved increafe of *moifture*, by which the *thread* of *whalebone* moves from 71 to 97,6: the *thread* of *deal* is alfo *ftationary*, while the fame *thread* moves from 71 to 91,3. They both afterwards *retrograde*; the *quill* from 107 to 100, and the *deal* from 122,6 alfo to 100; and it is during the latter part of that *retrogradation*, correfpondent to a continued *direct* motion of the *flips*, that the *thread* of *whalebone*, and fome more of its clafs, after a very *decreafing* march comparatively with all the *flips*, are at laft *ftationary*, and then a little *retrograde*. In that *ftationary* ftate of the *threads*, while moifture proceeds in the fame direction, they move backwards and forwards, more or lefs, according to the duration of that ftate, and to the quantity of the *retrogradation*. This may be feen by the table, in

refpect

refpect of the *thread* of *deal* and *quill*; and I have obferved it conftantly, in a fmaller proportion, in all the *threads* which have their *ftationary* ftate at the laft period of *moifture*, with this particular circumftance in all thofe motions backwards and forwards, that they are never the fame in two different experiments. That fymptom already points out a complication of caufes; but we fhall foon fee a more diftinct proof of their exiftence.

56. My theory on the *march* of hygrofcopic *threads* is founded on this general principle, that a *retrograde* effect, how fmall foever it may be, if it is not produced by a correfpondent change in the caufe itfelf, is preceded by a *ftationary* ftate, during which, and the *retrogradation*, the intenfity of the caufe continues to *increafe*: and this alfo is exemplified in the experiment. The *ftationary* ftate of the *thread* of *deal* begins, when its *fhavings* have only imbibed from the air a quantity of *water* = 36; it is ftill at the fame point when the quantity of imbibed *water* has increafed to 59,9; and in the part of its *retrogradation*, which is ftill contained in the regular courfe of the experiment, that quantity of imbibed *water* increafes to 88,8. The fame, with only fome differences in the degree, is feen in the *thread* of *quill*; therefore, as we fee alfo fome, though a very fmall, *retrogradation* in the *thread* of *whalebone*, as well as in other *threads* of the fame *clafs*, we have reafon to conclude, that their apparent immobility before they come to that point, while the *flips* continue to move, is alfo a *ftationary* ftate, during which they continue to receive water, by the increafe of *moifture* in the air.

57. The experiments I have now analyfed are only one fet amongft others which, though made with lefs accuracy, have given the fame general refults. Thefe, relating to various

kinds of ſubſtances, I intend to repeat, and to have the honour of communicating their reſults to the Royal Society; and I ſhall conclude this Paper, with an immediate demonſtration, that the *hygroſcopic* motions of the *ſlips* are ſimple, while thoſe of the *threads* are the combined effects of two oppoſite cauſes: which will be a further confirmation of the whole of the above theory.

On the recoil *of hygroſcopic* threads.

58. When formerly I concluded from the phænomena of the *water thermoſcope*, that its *condenſations* were the combined effects of two oppoſite cauſes, which followed different laws, it was not for having diſtinguiſhed thoſe two effects; but only becauſe of a ſmall *retrogradation* near the *freezing point*, preceded by a *ſtationary* ſtate, comparatively with the *march* of *quickſilver*; but in the caſe of hygroſcopic *threads* and *ſlips*, in which we have the ſame phænomenon, the two oppoſite effects are diſtinguiſhable in the *threads*, by one being operated more rapidly than the other. If, for inſtance, I tranſport from a drier to a damper place (or inverſely) the two kinds of *quill* hygroſcopes, the *ſlip* proceeds in an *even* courſe to a certain point, where it remains *fixed*; but the *thread* moves in an *interrupted* manner alſo to a certain point, whence it *recoils*. If that experiment is made within the limits of the *ſtationary* ſtate of the *thread*, it may *recoil* as much as it has gone the other way, and be fixed at the ſame point in both places. The caſe of the *ſlip* of *quill* is common to every *ſlip*, and that of its *thread* to all others which have a quick motion. Here then we have *ſeparately* the *two* effects of *moiſture* on the *threads*; that on the *fibres* themſelves is the *ſooneſt* produced, and at firſt predominates: the *ſloweſt*, by

by which afterwards the firft produced is more or lefs *compenfated*, is that operated on the width of the *mefhes*; and it is becaufe the laft of thofe effects is the only one that can affect the *length* of the *flips*, that, in every change of *moifture*, they move *evenly*, without any *recoil*.

59. To that demonftration of the exiftence of *two* oppofite *effects* of *moifture* in the *threads*, I fhall now only add an example of a fimilar phænomenon, in which the *caufes* alfo are vifible. The *compound frames*, mentioned in § 24. are formed of two *glafs* rods 4 feet long, fixed together at the bottom and the top. A thin flip of *brafs* of determined length, fixed to one of the *glafs* rods towards the top, comes down from thence, paffes over a pulley at the bottom, and turns up for half an inch. To this end of the *brafs* flip is fixed the lower end of the hygrofcopic fubftance, the upper end of which is connected with the *index* of the inftrument. It is by that means that, whatever be the changes of *heat*, provided they are *flow*, the lower end of the hygrofcopic fubftance remains fenfibly at the fame diftance from the axis. But if I take that inftrument out of water at a low *temperature*, and plunge it immediately into *warmer* water, the *index* inftantly moves as if the hygrofcopic fubftance had lengthened; which is the *effect* of the *brafs* flip dilating fooner than the *glafs* rods; then the index *recoils*, and this is the *effect* of a *flower* dilatation of the *glafs*.

Conclufion.

60. I have concentrated in thefe pages an account of twenty years affiduous labour in *hygrometry*, moftly occafioned by the anomalies of the *hygrofcopic threads*; and the principal refults have been, fome determinations of the four principles that directed

directed me from the beginning, which now are as follows. 1ſt, *Fire*, as cauſe of *heat*, is a ſure, and the only ſure, means of obtaining *extreme dryneſs*: this is produced by *white heat* in every *hygroſcopic* ſubſtance that can bear it; and it may be thus tranſmitted to the *hygrometer*. 2d, *Water*, in its liquid ſtate, is a ſure, and the only ſure, means of determining the point of *extreme moiſture* on that inſtrument. 3d, It is not to be expected, *à priori*, of any *hygroſcopic* ſubſtance, that its *changes* be proportional to thoſe of *moiſture*; but it may be affirmed, that no *fibrous* or *vaſcular* ſubſtance, taken *lengthwiſe*, is proper for the *hygrometer*. 4th, A means of throwing light on the *march* of a choſen *hygrometer*, may be, to compare it with the correſpondent changes in *weight* of many *hygroſcopic* ſubſtances.

61. From thoſe determinations in *hygrometry* ſome great points are already attained in *hygrology*, *meteorology*, and *chemiſtry*, of which I ſhall only indicate the moſt important. 1ſt, In the phænomenon of *dew*, the *graſs* often begins to be *wet* when the *air*, a little above it, is ſtill in a middle ſtate of *moiſture*; and *extreme moiſture* is only certain in that *air*, when every ſolid expoſed to it is *wet* (§ 28.). 2d, The *maximum* of *evaporation* in a cloſe ſpace, is far from identical with the *maximum* of *moiſture*; this depending conſiderably, though with the conſtant exiſtence of the other, on the *temperature* common to the *ſpace* and to the *water* that evaporates (§ 30.). 3d, The caſe of *extreme moiſture* exiſting in the open tranſparent air, in the day, even in time of *rain*, is extremely rare: I have obſerved it only once, the *temperature* being 39°. 4th, The *air* is *dryer* and *dryer* as we aſcend in the atmoſphere; ſo that in the upper attainable regions, it is conſtantly very *dry*, except in the *clouds*. This is a fact certified by M. DE SAUSSURE's obſervations and mine. 5th, If the whole atmoſphere paſſed from

from *extreme dryneſs* to *extreme moiſture*, the quantity of *water* thus *evaporated* would not raiſe the *barometer* as much as half an inch. 6th, Laſtly, in chemical operations on *airs*, the greateſt quantity of *evaporated water* that may be ſuppoſed in them at the common *temperature* of the atmoſphere, even if they were at *extreme moiſture*, is not ſo much as $\frac{1}{100}$ part of their maſs. Theſe two laſt very important propoſitions, have been demonſtrated by M. DE SAUSSURE.

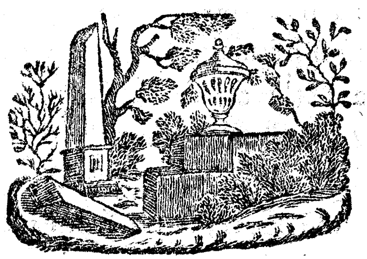


Fig. 1.

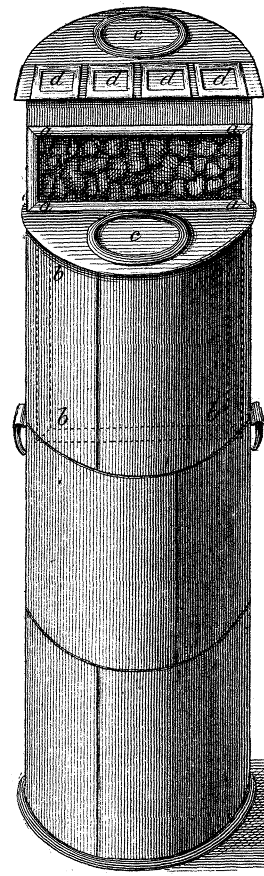
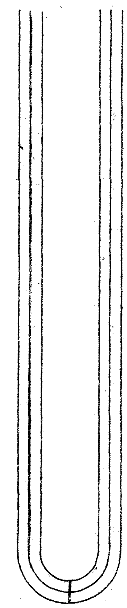


Fig. 2.



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