XXXVIII. *Account of a new Hygrometer. By* M. J. A. De Luc, *Citizen of* Geneva, *F. R. S. and Correspond. Member of the Academies of* Paris *and of* Montpellier.

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IN laying before the Royal Society an account of my attempts to find out a method for meaſuring the moiſture of the air, I think myſelf obliged to relate the gradual ſteps of my mind, the obſtacles I met with, the means by which I endeavoured to overcome them, the degree at which I flatter myſelf to have arrived, the hopes that may be entertained of farther advances, and the uſes which may be derived from my firſt experiments.

Attempts to invent an Hygrometer.

1. In order to proceed regularly in this inveſtigation, I began by examining the eſſential requiſites in a machine intended to meaſure humidity, which I found to be the three following:

1ſt, The ſettling of a fixed point, from which every meaſure of the ſame kind ſhould be taken, ſuch, for inſtance, as that of boiling water in a thermometer, when the barometer is at a certain height.

2d, Degrees equally determined, or comparable, in different hygrometers, ſuch as are in the thermometer,

meter, the ſcales of Fahrenheit, Deliſle, Reaumur, &c.

3d, Conſtancy in the variations produced by the ſame differences of humidity.

2. I perceived, moreover, that it were to be wiſhed, that the hygrometer ſhould give a true indication of the relation between the real quantities of the humidity, or at leaſt between their differences: but this laſt point I rather conſidered as a deſirable degree of perfection, than as an indiſpenſable requiſite; the eſſential point being, that obſervers might underſtand each other, when mentioning degrees of humidity; and this ſeemed to be ſufficiently provided for by the abovementioned conditions.

3. Having thus planned to myſelf the work I had to go through, I firſt attended entirely to the firſt point, and laid aſide all the others. This I again ſubdivided. I had ſoon perceived that I muſt begin by thinking much leſs of the hygrometer than of the different phænomena of humidity. For this purpoſe it was neceſſary to find out a fixed ſtate, either of bodies in general, or of ſome body in particular; and this fixed ſtate might either be extreme humidity, or dryneſs, or any intermediate point.

4. Knowing that the extremes in nature are commonly very difficult, and ſometimes impoſſible to hit, I conceived at firſt greater hopes of intermediate degrees. But in vain did my imagination fatigue itſelf in a road, which I was forced to abandon.

5. I then came to the extremes, and that of abſolute dryneſs was the firſt I was induced to try. But having found no other way to procure it but by fire, and fire not producing it in all bodies which appeared

to me susceptible of humidity, but by altering their nature, I reluctantly perceived that I should be obliged to look for my first point, where I had the least hopes of discovering it.

6. I remained a long time without discovering any thing in this new road; and very often turned back, but was always obliged to return to extreme humidity, as to the only part of my object, of which I could possibly get any hold.

7. The words, which are necessary for communicating our ideas to others, are often obstacles to the raising of new ideas in ourselves. They are by far too few to express distinctly every shade of intellectual objects. Humidity was a word which I constantly repeated to myself, and it constantly led me to a class of phænomena, in which I could find nothing settled.

8. Water at length presented itself to my mind; and in this fluid, which to all appearance ought first to have struck me, I beheld with surprize, what I had been labouring, through many a round, to discover, under the denomination of extreme humidity. I was not at that time considering humidity in any particular phænomenon; I only observed that it was constantly produced by aqueous particles disseminated through bodies; and I found in water the maximum of the approach, and consequently of the action, of these particles.

9. In order now to avoid the ambiguities from whence, in my opinion, the difficulties in these matters arise, let me be allowed for the future to employ no words but such whose meaning is well determined. Humidity will accordingly be no more

that

than an effect, or modification of bodies from a substance more or less abundant, but constantly consisting of aqueous particles under different forms. This substance, considered in its utmost extent and under all the appearances which it assumes in nature, I shall express by the Latin word *humor*. Thus ice, water in its different degrees of heat, hail, snow, icicles, rain, dew, clouds, fog, mist, invisible vapours, are no more than modifications of this same substance, different species of a determined genus; since aquosity, which is common to all, is its generic character.

10. The more humor there is in any body, the more humid that body is; and consequently if it be plunged in water, and soaked so as not to be able to receive any more, it is got to extreme humidity, the water which fills up all its pores being humor in the highest degree of intensity.

11. Not, however, but that discrete humor, or vapour of every kind, may in some respects produce as great effects as concrete humor or water: but there is always some difference in some other respect, and chiefly in regard of time. Bodies encompassed with air are continually discharging, by evaporation, part of the humor they imbibe from it. If the circumstances are such, that the humectation exceeds the evaporation, the body at length wets through *, more or less quickly as the quantity of humor which it receives in a given time is greater or less, and likewise in proportion as this quantity exceeds that

* By *wetting* here, I understand arriving at the greatest degree of humidity.

which

which evaporates. It is ſuddenly wetted, when the humor is ſo condenſed as to become water, becauſe the evaporation which takes place at the ſurface of the water, does not weaken its action on the bodies dipped into it; it is only wetted little by little, or, what happens ofteneſt, in part, when the humor is diſcrete or reduced into vapour; becauſe, while it is depoſited on particular ſpots, it evaporates from the interſtitial parts, and that more or leſs according to the ſtate of the air, and that of the moiſtened bodies.

12. This difference, however, in point of time, between the action of concrete and diſcrete humor, only takes place on the ſurface of bodies, or at a ſmall depth; it diminiſhes, and may even become oppoſite, as the depth of bodies increaſes, becauſe the diſcrete humor is then more eaſily introduced into their pores than water, which more than makes up for their different intenſity.

13. This conſideration ſolves a difficulty, which at firſt puzzled me. I had been told by bird-catchers, that the threads of thoſe nets which they caſt on the water-ſide, were leſs ſtretched from the action of water than of dew. Hence it might ſeem, that what I took for the extreme of humor had leſs effect than what is only a degree of it. But two particular cauſes accounted for this difference.

1ſt, The air contained within the fibres of the thread oppoſes the introduction of the water, which, preſenting itſelf in a body, ſhuts up the paſſages by which the air ſhould eſcape to give it room; but it yields to the drops of dew, which permit its eſcape while they penetrate through the threads.

2d, A ohe r

2d, Another particular cause, less obvious though not less probable than the preceding, is the difference in the mutual attraction of parts, in the concrete and discrete humor, and consequently in their respective facility to separate, and get one by one through the narrow pores. When this entrance is attempted by the humor, under the form of water, the mutual attraction of its parts, being greater than in dew, occasions a greater resistance to their introduction, than when they are already divided by some other cause, viz. when the humor is reduced to small drops, or vapor.

14. This phænomenon, therefore, does not contradict my principle; it is only a particular fact; and it remains true, that bodies surrounded with water are exposed to the extreme of humor. To remove this cause of exception from my hygrometer, it was sufficient to provide outlets for the air, and not to increase too much the thickness of the body, upon which the humor was to act.

15. Another difficulty, which presented itself, was that water might probably act with more or less energy in proportion to its heat. But this did not stop me long. As my present object was a fixt point for the hygrometer, and not the greatest power of water, considered as a cause of humidity, it was enough to employ it constantly at the same degree of heat; and, to fix this with greater precision, I determined to use water at the instant that it ceases to be ice. The basis therefore of my hygrometrical scale was to be the soaking power of melting ice.

16. This principle, being thus unfolded, appeared so simple, that I was at first surprised how it could

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have been fo long overlooked. But I afterwards accounted for this, from the difficulties which I met with in the difcovery. The notion of an hygrometer being both complex and unfettled, all the obftacles prefented themfelves at once, and this multitude of ideas exceeded the power of attention. The very firft fteps were apt to miflead. On the one hand, I looked for an hygrometer with a head full of the matters already ufed for hygrofcopes, which always are more or lefs altered by water; and, on the other, the name of humidity was applied to that caufe, the effects of which I wifhed to meafure; and both points of view turned afide the mind from the idea of water, as being proper to afford the required fixed point in an hygrometer.

17. The firft difficulty had not efcaped me; but, confidered in itfelf, it did not appear unfurmountable. I was in hopes that a fubftance might be found capable of being affected by the foaking power of water, without being altered by it. As the nature of this fubftance was to determine not only the form of the hygrometer, but alfo the fpecies of the degrees, which were to indicate the different quantities of humor, I concluded that my fecond object ought to be the difcovery of this fubftance.

18. In this refearch, I again divided the objects, by confidering feparately the three kingdoms, viz. the mineral, the vegetable, and the animal. The two firft offered no fubftance fit for my purpofe, viz. none that would obey the impreffions of humor, without being altered either by it or by other caufes. But in the animal kingdom bones drew my attention; and ivory, in particular, feemed to poffefs the

required

required qualities. I had obſerved that the key of an ivory cock was tighter or ſlacker, as there was more or leſs humor in the air. Ivory pallets, uſed for water-colours, ſhewed no alteration, at leaſt none that was laſting. I knew alſo the elaſticity of this ſubſtance, which ſeemed to ſecure its coming back to the ſame ſtate, on its return to the ſame degree of moiſtneſs.

19. There ſtill remained on this ſecond head another object of inquiry, which was almoſt neceſſarily connected with the third, viz. the ſpecies of the degrees to be given to the hygrometer. The beſt form to be given to the ivory, in order to receive with eaſe the impreſſions of the humor, and to have its effects meaſured upon it, was to be determined. I firſt thought of ivory rods, the lengthening of which ſhould be meaſured by a machine ſimilar to the pyrometer. I likewiſe had ſome notion of a large nonius, formed of an ivory and a metallic rod. Either of theſe machines would admit of a fixed graduation, as both the dimenſions of their parts and proportions to one another could be determined. But then I apprehended that ivory might perhaps, like wood, have its longitudinal fibres but little liable to be extended by the humor, and that the imperfections of theſe two kinds of micrometers would occaſion a conſiderable irregularity in the hygrometrical degrees. I alſo feared that if ivory rods were made thick enough to prevent their bending, ſuch a thickneſs might become an obſtacle to their intire penetrability by the humor (14). I therefore concluded that the ivory ſhould have ſuch a form, that, though very thin, it might not warp; and that the meaſurable varia-

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tions were to be the removal or approach of its fibres to each other.

20. Being thus guided by these necessary conditions, I thought of different thin ivory cups, the capacities of which should be measured by quicksilver; and at last imagined a hollow cylinder, in which the variation of its capacities, when more or less moist, might be measured by the quicksilver it should be filled with; and which putting into a glass tube joined to the ivory one, would of course rise more or less, as that vessel was more or less deprived of humor.

21. Nothing now remained but to find out a way of estimating the changes of capacity of the ivory tube, by means of the variations in the height of the mercury in the glass tube. I thought, at first, that by using very nice scales, in order to compare the weights of the mercury contained in the cylindrical vessel, with that of a column of the same liquid in the tube, I might obtain the proportions of these weights with a sufficient exactness, to be able to measure the variations of the mercurial column, by degrees representing aliquote parts of the whole mass.

22. This in itself was undoubtedly an exact method; but then it required in the execution such a nicety in the scales, that I durst not employ it in the construction of an instrument of so extensive an use. Such scales are always scarce from their high price. I remembered to have myself found that inconvenience in the construction of a Delisle's thermometer, and concluded I must hit upon some method to avoid it.

23. The idea of a thermometer, which struck my mind, was a lucky one. I was led to it by a kind

kind of connexion between the ſcale of that inſtrument and that of my hygrometer. I ſoon perceived, that, by applying to my hygrometer a thermometrical tube, already graduated by means of two fixed points of heat, it would only be neceſſary to know the proportion of the weights of mercury in this thermometer, and the hygrometer, to which its tube was to be applied, to have in this laſt inſtrument degrees as well determined as in the firſt. Scales of a common degree of exactneſs were ſufficient to eſtabliſh between the reſpective degrees of both inſtruments a proportion equal to that of their mercurial weights (42 and 43).

24. Beſides the eaſe in the execution, this contrivance afforded me a very ſimple method to correct the effects of heat upon the mercury contained in the hygrometer. It is indeed obvious, that, abſtractedly of the effects of the humor, the new inſtrument muſt in itſelf be a regular thermometer; and that conſequently the variations of an adjoint thermometer were immediately to point out this correction.

25. Every principle being thus ſettled, nothing remained but to contrive its conſtruction. I began by making ſome experiments, on the nature and quantity of the action of water upon ivory. I made for that purpoſe a ſmall cylindrical ivory veſſel, of an inch in diameter, and eight lines in length, and reduced its thickneſs to leſs than ¼ of a line. I likewiſe prepared a wooden cylinder, equal in its diameter to the internal one of the veſſel. I then put this veſſel into water, in ſuch a manner that it only wetted it outwardly to the rim. In a very ſhort time the wooden cylinder, which at firſt filled the

cup exactly, no longer filled it. After a few hours, I perceived that the internal ſurface grew wet, and by means of a magnifying glaſs, found it covered with a very fine dew. This dew did not encreaſe by the veſſel remaining any longer in the water; the evaporation being doubtleſs equal to the tranſudation; and the capacity of the veſſel, which encreaſed till the appearance of the dew, ſeemed afterwards at a ſtand.

26. This tranſudation puzzled me a little; it ſhewed me that the water would get into my hygrometer, which at firſt appeared an inconvenience. I ſoon, however, found an advantage in it. The water, after having ſoaked through the ivory, would immediately puſh back the mercury, which having by degrees ſunk in the tube, during the penetration of the water through the pores of the ivory, muſt thus riſe again. Hence I might expect a maximum for the fall of the mercury, very eaſy to be determined. As for the water introduced into the ivory veſſel, I was in hopes that it would go back as ſoon as the outſide of the cup ſhould be dry.

27. Having thus aſcertained that ivory was very eaſily affected by the impreſſions of the humor, it ſtill was neceſſary to know, whether the variations of the one would always equally anſwer to thoſe of the other. Having accordingly taken my ſmall cup out of the water, and expoſed it to the air, I ſoon found that its capacity diminiſhed, but that even after ſeveral days it did not return to its former ſtate. This again puzzled me; but I ſuſpected that the external preſſure of the tool upon the ivory might ſomewhat have compreſſed it, and that the water having reſtored the ivory fibres to their original pitch, the abſolute

abſolute capacity of the cup remained larger than it was before.

28. To ſatisfy myſelf about this, I got another wooden cylinder, which filled the capacity of the veſſel in its preſent ſtate. This I again put into the water, and left it there a ſufficient time; I then expoſed it to the air to be dried; and after that found that the wooden cylinder filled it as before. Hence I concluded, that in the conſtruction of my hygrometer the ivory cup ſhould be dipped for ſome time in water, and afterwards dried, before it was uſed.

29. Thus having cleared up my conjectures, as much as they could be, by theſe preliminary experiments, and got ſome inſight into the proportions of the different parts of the machine, I proceeded to its conſtruction, and finiſhed it in the following manner:

Deſcription of an Hygrometer.

30. Tab. XVIII. figure 1. ſhews the ſection of the interior part of the inſtrument, of its true length, in the direction of its axis.

The firſt part to be deſcribed, being in ſome meaſure the ſoul of the hygrometer, is an ivory tube, *a a b*, open at the end *a a*, and cloſed at *b*. It is made of a piece of ivory, taken at the diſtance of ſome inches from the top of a pretty large elephant's tooth, and likewiſe at the ſame diſtance from its ſurface, and from the canal which reaches to that point (68). This piece is to be bored exactly in the direction of its fibres; this hole is to be very ſtreight, and its dimenſions are $2\frac{1}{2}$ lines in diameter, and two inches 8 lines in depth from *a a* to *c*.

31. Prepare

31. Prepare after this a braſs cylinder, about $3\frac{1}{4}$ inches long, and to one of its extremities fix the pully proper to receive the ſtring of the bow when the piece is turning. This cannot be done too carefully, both to make it perfectly round, and to fit it exactly to the hole of the ivory tube; its extremity muſt even be rounded, that it may be applied cloſely to the bottom of the hole. Having then roughly prepared the outſide of the ivory tube, and introduced into it the braſs cylinder, put both pieces thus united upon the turning wheel, and find out on the outſide bottom of the ivory tube, the point which anſwers to the axis of the braſs piece, in order that this may turn exactly upon its axis. It is with this view that the braſs cylinder is made longer than the ivory tube.

32. All theſe precautions are deſigned to make the ſides of this tube of an equal thickneſs, viz. $\frac{3}{16}$ of a line, except at the two extremities. At the bottom *b* the tube ends in a point, and at the top *a a*, it muſt for about two lines be left a little thicker, in order to enable it to bear the preſſure of another piece, which is to be put into it. Thus the thin or hygrometrical part of the tube will be reduced to $2\frac{1}{2}$ French inches, including the concavity of the bottom.

33. Before this piece is uſed, put it into water ſo as that the external part alone be wetted by it, and leave it there till the inſide be every where covered with the dew I mentioned before (25). This will take place in a few hours; I have given the reaſons for this operation (28).

34. The glaſs tube intended for this hygrometer muſt be about 14 inches long. Its lower end is ſeen in

in *d d e e* (fig. 1.). Its internal diameter is about $\frac{2}{5}$ of a line. The reaſon why it ſhould not be ſenſibly leſs will be given hereafter (52); and if it was ſenſibly larger, the variations of height in the mercurial column would not be conſiderable enough. On the dimenſions that I propoſe, when the hygrometer is put into melting ice, in a fine ſummer day, the mercury falls about ſix inches in the tube. The outſide diameter of this tube ſhould be about two lines, in order that the part *g g* of a braſs piece through which it paſſes, and which is to enter into the ivory pipe, be as thin as poſſible.

35. The glaſs tube, as I ſaid before, ſhould have belonged to a thermometer. Its extremity widens of courſe towards the ball; which will be of uſe, when the mercury is poured into the hygrometer, in order that it may drive the air before it, by riſing from the ivory pipe into the glaſs tube. To preſerve this widening, break the ball of the thermometer by ſtriking againſt the bottom; and with pinchers take off the reſt little by little, and make the extremity cylindrical by grinding it upon the wheel. The ſame muſt be done at the top, which I ſuppoſe to have been made to end in an olive or ſmall reſervoir for the filling of the thermometer. This widening is likewiſe to be ſaved for the reaſons hereafter to be mentioned (52).

36. The piece *f f g g* is intended to join the ivory with the glaſs tube. It is of braſs, ſhaped as in the figure. A cylindrical hole is bored through it, which holds the glaſs tube as tight as poſſible, without danger of breaking it; and its lower part is to enter with ſome degree of difficulty into the ivory pipe.

37. To

37. To hinder the part of that tube, which encloſes the braſs piece, from being affected by the variations of the humor, which might ſometimes prevent a ſufficient preſſure, I cover this part of the tube with a braſs verrel, repreſented in *h h i i*. It muſt enter with force, and will henceforth be conſidered as part of the ivory pipe.

38. To unite thoſe pieces together, I make uſe of gum lac, or of maſtich, which melts by the heating of the glaſs and the braſs. I firſt cement the braſs piece with the glaſs tube by introducing the tube, and leaving it at firſt at an inch diſtance from the place where it is to be fixed; I then hold this end of the tube over live coals, by bringing it nearer and nearer, and turning it, that both that and the braſs piece be every where equally heated; and when they are hot enough to melt the gum lac, I rub the glaſs tube with it, and puſh the braſs piece to its place by means of a hollow bit of wood, drawn beforehand over the tube for this purpoſe. As the braſs piece advances, the lac accumulates towards the end of the tube; I take away the ſuperfluous part, but leave a ſlight coat of it over the end of the braſs piece, in order to preſerve it from the contact of the mercury that might corrode it. When this piece is properly placed, and ſtill warm, I cover with lac its cylindrical outſide, and introduce it into the ivory tube, which has been ſomewhat warmed by holding it near the fire, in order that the lac may ſtick more cloſely to it. As ſoon as theſe pieces are cold, they are found very ſtrongly cemented together, and neither mercury nor water can make their way between them.

39. The introduction of the mercury is the next operation. I first roll a slip of paper three inches wide over the glass tube, and tie it fast to the extremity which is nearest to the ivory pipe. I then introduce into the tube a horse hair long enough to enter the cylinder by one end, and to have the other rise three or four inches beyond the orifice of the tube. I then raise the paper which has been shaped round the tube, and use it as a funnel to pour the mercury into the instrument, which I hold upright. The purest quicksilver ought to be employed for that purpose, and it will therefore be proper that it should be revivified from cinnabar. I poured it then into the paper funnel, from whence it easily runs into the tube, with the assistance of some gentle shakes. The air which it drives before it comes out along the horse hair. Fresh mercury must from time to time be supplied, to prevent the entire emptying of the paper tube, and the running in of the mercurial pellicle, which the contact of air always produces upon the surface.

40. Some air bubbles generally remain in the tube; they may be seen through the ivory pipe, which is thin enough to have some transparency. These being collected together by shaking, must be brought to the top of the tube, and expelled, by means of the horse hair. To facilitate this operation, some part of the mercury must be taken out of the tube, in order that the air may be less obstructed in getting out, and the horse hair have a freer motion to assist it.

41. Air, however, cannot be entirely driven out in this manner. It is the weight of the mercury, with

with which the tube is for that reaſon to be filled, that in time completes its expulſion, by making it paſs through the pores of the ivory. To haſten this, I place my hygrometers in a box made on purpoſe; and this I fix pretty nearly in a vertical direction, to the ſaddle of a horſe, which is ſet a trotting for a few hours. The ſhakes ſometimes divide the column of mercury in the glaſs tube, but it is eaſily reunited with the horſe hair. When, upon ſhaking the hygrometer vertically, no ſmall tremulous motion is any longer perceived in the upper part of the column, one may be ſure that all the air is gone out.

42. I now come to the operations requiſite to make the ſcale of the hygrometer, and firſt of all to that which determines the baſe (15). This may be done as ſoon as the air is gone out. I then ſuſpend the inſtrument in a veſſel filled with ice mixed with the water it produces in melting. I take care to ſupply the melting by recruits of freſh ice, during the courſe of this proceſs, which laſts ten or twelve hours. In the firſt hour, the mercury ſinks above one third of the ſpace it has to go through; it advances leſs in the ſecond; and its motion leſſens thus gradually, till it appears ſtationary, which frequently happens after ſeven or eight hours, and it remains two or three hours in that ſituation. The ivory being then become more tranſparent on account of humidity, a very thin dew is perceived by a certain play of the light on the ſurface of the quickſilver. Laſtly, the mercury begins to reaſcend; the operation is terminated; and ſmall drops of water, as I expected, are at that inſtant ſeen upon its ſurface (26).

43. I follow the laſt ſteps of the mercury in its fall, by means of a fine ſilken thread fixed very tight around the tube. This is left at the loweſt point it has been brought to. If this point be too low, relatively to the frame of the hygrometer, freſh mercury is poured in, and the thread proportionally drawn up higher; if too high, I take off ſome of the mercury and lower the thread; and in both caſes make uſe of the horſe hair. This muſt be done when the mercury ceaſes to fall, in order that the place where the thread is to remain may be immediately determined by this operation.

44. This point thus fixed is named o in my hygrometer; it is that in which dryneſs is nothing (if I may be allowed to expreſs myſelf ſo), ſince it is that of extreme humidity, in a given heat; viz. that of melting ice. From this point are reckoned all the degrees I am now going to ſpeak of; which thus become degrees of exſiccation.

45. The laſt eſſential operation is that by which the ſize of the hygrometrical degrees are determined; and this I ſhall deſcribe by an example. It muſt be remembered that the hygrometer's tube was originally a thermometer (23). I take it in this firſt ſtate, in the inſtance I am going to give. The diſtance between the thermometrical points of melting ice and boiling water, at twenty-ſeven French inches of the barometer, was found to be 1937 parts of a certain ſcale. I broke the bulb of this preparatory thermometer, in a baſon, in order to receive carefully all the mercury that it contained. This being weighed in nice ſcales, amounted to 2 *on.* 11 *dr.*

 12 *gr.*

12 *gr.* or 1428 grains. All the pieces of my hygrometer being put together, it weighed 373 grains, and when filled with the proper quantity of mercury 833. It consequently contained 460 grains of mercury.

46. By the rule above given (23), the extent of the hygrometer's degrees, ought to be to that of the degrees in the preparatory thermometer, in proportion of the respective weights of mercury in the hygrometer and thermometer; and consequently as the weight of the mercury in the thermometer is to the weight of the mercury in the hygrometer, so is any given interval in the thermometrical scale, to the corresponding interval in the scale of the hygrometer. Consequently in our example as 1428 : 460 :: 1937 : 624 (nearly); and the corresponding intervals on the scales of the thermometer and the hygrometer, ought to follow the proportion of 1937 to 624.

47. I call the distance between the two fixed points of heat in the thermometer the *fundamental interval*; and I shall call the *fundamental line* in the hygrometer that of which the length corresponds to this interval. Thus the *fundamental interval* in the preparatory thermometer, being 1937 parts of a certain scale, the *fundamental line* of my hygrometer consisted of 624 parts of the same scale. This example may so easily be applied, that it will be unnecessary to dwell any longer upon this subject.

48. Having thus got a *fundamental line* in the hygrometer, I had it in my power to divide it into as many parts as I thought proper: my choice was naturally

naturally to be determined by the ſimplicity of a proportion between the degrees of the thermometer, and thoſe of the hygrometer, becauſe this laſt was to be corrected by the firſt, on account of the effects of heat (24). My firſt thought was to divide this *line* into 80 parts, agreeably to the diviſions of the fundamental interval in what I call the *common thermometer* in my book upon the air, which I ſhall always be underſtood to mean in this paper. But as the minuteneſs of theſe degrees was found to be inconvenient and ſuperfluous, I determined to make them double, by putting only 40 in the length of my hygrometer's fundamental line. It is eaſily underſtood that theſe degrees, thus ſettled, begin to be reckoned from the place of the thread, which indicates upon the tube of the hygrometer extreme humidity, by the heat 0 of the *common thermometer*, or of melting ice.

49. The inſtrument with its frame is ſeen fig. 2. the dimenſions of which are every way one half of thoſe of the original. It is mounted on deal, that being the wood, which ſuffers the leaſt change in the length of its fibres. The lower part of the frame is ſlit through the whole length of the ivory pipe, in order that the air may circulate freely round this pipe, and the bulb of a thermometer which I ſhall mention preſently. The hygrometer is faſtened in three parts; viz. at bottom on a ſmall bracket, at top by a tube paſſing through a piece either of hard wood or of metal faſtened by ſcrews; but chiefly by means of a braſs wire on the neck of the braſs piece, which unites the glaſs with the ivory pipe. This piece is laid in a ſmall plate of a hard wood,

wood, which in that place fills a groove originally made throughout the whole length of the lead-board.

50. To keep the duſt from getting through the opening of the tube, I ſhut it up in a ſmall ivory caſe. It cannot be ſealed up, becauſe if air was left in, it would obſtruct the riſing of the mercury; and if it was exhauſted, the mercury would be puſhed to the top by the preſſure of the atmoſphere upon the ivory pipe; as I have experienced it.

51. Hence however ariſes a ſmall inconveniency; which is, that as the upper part of the column of mercury communicates with the air, if it remains long in the ſame part of the tube, or moves but little in it, ſome dirt may be left on the ſides. This I eaſily remedy, by means of a braſs wire, the extremity of which is dentated in the form of a file, in order to hold ſome bits of cotton, which I put round it. The wire is eaſily introduced into the tube, by means of the widening mentioned before (35). I put it in, when the mercury is below the part it has ſoiled, and eaſily clean it by this means. It is on this account that the tubes to be employed are to be of about $\frac{3}{8}$ of a line internal diameter.

52. The ſcale of the hygrometer is marked upon a deal ſlip, which ſlides along the groove I mentioned before (49). This, as well as all the other parts of the frame, muſt be lined with paper, to mark the neceſſary ſcales; and this paper is afterwards varniſhed over. Thin plates of ſilvered braſs can be employed for the ſame uſe.

53. The mobility of the ſcale of the hygrometer ſerves to correct, in the obſervation itſelf, the effect

 of

of the heat on the mercury. At the top of this ſcale is ſeen an index, over-againſt another ſmall ſcale, marked upon the unmoveable part of the frame. The degrees of this ſmall ſcale are eightieth parts of the fundamental line, and conſequently immediately anſwer to the degrees of the thermometer on the ſame frame (48). When the index points to o of the ſmall ſcale, the thread which indicates upon the tube of the hygrometer the point to which the mercury ſunk in the melting ice, anſwers likewiſe to o in the ſcale of the hygrometer. This is the caſe expreſſed in the figure wherein the thermometer is likewiſe repreſented at o of its ſcale. By firſt obſerving the heat therefore, and conducting the index to the point of the ſmall ſcale, which anſwers to the actual degree of the thermometer, the hygrometer will only indicate upon its ſcale the degrees of the humor. For this ſcale going through the ſame variations that the heat occaſions in the height of the column of mercury, the indications of the hygrometer become juſt the ſame as they would be, if the heat always continued that of the point at which extreme humidity was fixed, viz. o. of the common thermometer.

The ſcale of the hygrometer is carried to the proper point, by means of a knob fixed on a ſmall piece of hard wood or metal, ſcrewed to the bottom of the board, and which affords a free paſſage to the tube of the hygrometer.

Account of the first OBSERVATIONS *made on the going of this* HYGROMETER.

Read June 10, 1773. 54. My first hygrometer was ready for observation at the beginning of last February (1772), in a rainy season. A few hours after it was taken out of melting ice it was already at 54 degrees of its scale. The next morning it was only at 50, but towards noon it rose again to 54. I carried it down to my cellar, which being a considerable depth under ground is commonly very damp. As I went down the stairs, I perceived that my hygrometer continued falling, so that when I hung it up in the cellar it was as low as 35.

55. In the evening of the same day it was at $28\frac{1}{2}$, and the next night at $21\frac{1}{2}$. It continued falling imperceptibly during the rest of this month, throughout the whole of the next, and till the 19th of April. On that day it was at $3\frac{1}{2}$, and consequently very near extreme humidity: but in this interval it had very often rained, and snowed, and even when the sky was clear over head, the streets had always remained wet, so that it was evident from all the common appearances, that the humidity had gone on considerably increasing in the cellar.

56. I was impatient to see the hygrometer rise again in the cellar itself, which I could not however expect but with a north wind. At length, on the 20th of April, though the rain still continued, the hygrometer rose half a degree. In the night of the 20th to the 21st the wind came about to the north, and when I looked at my hygrometer in the morning, I found it at $6\frac{1}{2}$. It continued rising imperceptibly

ceptibly the whole of that day, and the next morning stood at 9½.

57. Another circumstance I was anxious to know, was, whether the hygrometer, after having been kept in the cellar so long, would rise, upon being carried up stairs again, to the point from which it had fallen. The importance of this new observation prevented me from pursuing that I had begun in the cellar. I therefore took my hygrometer out of it, and while I was going up the stairs it rose three degrees. This was at six o'clock in the morning. At seven it was already at 17, and at eight at 23¼. From eight to eleven it rose to 43, and at one o'clock stood at 63. After this it fell again, and at half an hour after five was no higher than 50. The sky had been clouded during the last interval.

As the preceding observations relate only to the hygrometer, and not to humidity, I shall confine myself to them. They are sufficient to give an idea of the going of the instrument in the season they were made. I shall hereafter give an account of some observations taken at other seasons.

First Experiments *made to discover the Accuracy of this* Instrument.

58. The most important thing after the preceding observations, was, to try whether the instrument was in reality comparable. To ascertain this, I immediately constructed four more upon the same principles, which were finished on the 23d of August.

59. I could not use my first hygrometer to make comparative observations with the new ones; its tube

being either too narrow or too ſhort. The proportion I had ſettled between the capacity of this tube, and that of the ivory pipe, was deduced from the preliminary experiments I had made in the month of December (29); and had of courſe been found juſt, as long as the ſpring laſted. But even before the new hygrometers were completed, the quickſilver had riſen in the firſt ſo as to run out of the top of the tube. This, joined to ſome other previous obſervations, which had convinced me that the diminution of the humor is much more conſiderable on mountains than in plains (76), induced me to fix the dimenſions of the tube of the hygrometer in the manner laid down in the deſcription of the inſtrument. I had been in time to follow theſe dimenſions in the conſtruction of my new hygrometers, ſo that when they were brought from extreme humidity to the ſtate of the air in my apartment in the month of Auguſt, the quickſilver did not riſe too high in them; that is, it remained ſufficiently below the top of the tube, to indicate leſſer degrees of humidity afterwards.

60. The four new hygrometers have been conſtructed with as little reference to each other, as if they had been made in different countries. By comparing them therefore, I have been enabled to judge of what might be expected from the agreement of inſtruments of this kind. This is what I have found.

When I have obſerved them in places where it appeared likely that the humor would be equally diſtributed among them, the utmoſt of their difference has been uſually from 19 to 21. Their greateſt height, for inſtance, in my room with the windows

ſhut,

ſhut, has hitherto been 94, 99½, 100½, 105½, in the ſame moment; which is pretty nearly in the proportion of 19 to 21, between the hygrometer which remains at the loweſt, and that which is at the higheſt.

61. Beſides this difference between the relative altitudes of theſe four inſtruments, I have obſerved another kind of irregularity in them, which is, that they do not always preſerve the ſame proportion to each other. Theſe variations are undoubtedly in part owing to the cauſe itſelf of their motions; that is, to the unequal diſtribution of humor even in places very near each other; but I have reaſon to aſcribe part of them to ſome defect in the inſtruments themſelves. I ſhall hereafter return to theſe cauſes, and give them a cloſer examination.

CONSIDERATIONS *on the Degree of* ACCURACY *that has been obſerved.*

62. Notwithſtanding the defects I have mentioned were rather evident, I was not diſſatisfied with this firſt trial. I never imagined that I had foreſeen every thing, and conſequently could not expect to arrive at a ſufficient degree of exactneſs without the help of experience; the irregularities therefore which appeared in the execution, did not make me deſpair of being able to perfect that inſtrument.

63. My hopes in regard to this were at firſt only grounded upon general reflections. I recollected what the barometer and thermometer had been when they firſt came out of the hands of their inventors; and obſerved that in ſome reſpects they were more

 irregular

irregular than my hygrometer is at preſent. Though the firſt of theſe inſtruments was very ſimple in itſelf, yet barometers hung up in the ſame places uſed to vary three or four lines from each other. Some of the members of the French Academy have been themſelves engaged in conſidering a barometer that always kept 18 lines below the reſt, and they have formed various hypotheſes to account for this difference. The variations therefore of the barometer, though obſerved only in the ſame place, were much greater than thoſe between my hygrometers.

64. Nor was the thermometer itſelf, which is now brought to ſuch a degree of accuracy, much ſuperior at firſt to our hygroſcopes, for the purpoſe of comparative obſervations. The firſt philoſophers who treated of it knew nothing of any fixed point or determined degree in it; they knew nothing even of the effect produced by the difference of liquids. In this ſtate of uncertainty the Royal Society adopted the moſt prudent plan that could be thought of; by giving its ſanction to a thermometer to ſerve as a ſtandard for the conſtruction of thoſe which philoſophers ſhould make uſe of. After this ſome men of genius endeavoured to eſtabliſh fixed principles for the making of this inſtrument. Sir Iſaac Newton led the way, but the utility of his firſt attempts was not ſufficiently attended to. Fahrenheit and Reaumur then laboured with great care to ſettle this point, and we are much indebted to their inquiries. But Fahrenheit's principles were ſoon rejected, as being too uncertain, though his ſcale was preſerved; and Mr. de Reaumur's, though in appearance admitted for a longer continuance, were in fact ſo indeterminate,

 that,

that, without perceiving it, a deviation from 80 to 104, was made in the ſpace between the two fundamental points of his thermometer.

65. If in the ſame manner we trace the origin of all inſtruments deſigned for nice menſuration, we ſhould find that have all been defective at firſt, and gradually brought to perfection, when men of genius have thought them worthy of their attention. Thus from the firſt watch, which depended entirely upon the unequal and uncertain action of a ſpring, a ſucceſſion of attempts has produced Mr. Harriſon's valuable time-keeper; and from the firſt balances, which were either too heavy, or too light, we have attained to thoſe ſcales of Mr. Matthey * as eaſily turned as they are accurate. What however is ſtill more aſtoniſhing is, that, notwithſtanding the importance of having fixt meaſures for the dimenſions of bodies, we have not as yet uſed any in practice, but ſuch as muſt be modeled immediately from others.

It is true, that in the conſtruction of an Hygrometer, I was aſſiſted by the general notions of regularity, gathered from the conſtruction of other meaſures of the ſame kind; and of courſe my hygrometer is much forwarder in this reſpect than the thermometer, for inſtance, was in its origin. I therefore only compare the difficulties peculiar to the hygrometer,

* An excellent mechanic, whoſe death is a loſs to a king who knows the value of men of merit. He was a native of Vale-Orbe, in the Pays de Vaud, and in the ſervice of his Sardinian Majeſty; and has written a *Treatiſe on Balances*, which ſerves as a law to all the ſcale makers in the dominions of that prince.

grometer, to thoſe that firſt occurred in all meaſures of phyſical cauſes; and I think that as the latter have been ſurmounted, we ſhould not deſpair of conquering the former. In a word, it is certain that all our inventions only approach towards perfection by degrees, without ever attaining to it entirely; and for this very reaſon, we have a right to expect they will always be drawing nearer and nearer towards it.

Upon theſe notions chiefly, I have raiſed my hopes, either that my hygrometer will in time become more perfect, or at leaſt that it may excite new ideas, which, will at length, though perhaps by ſome other road, lead us to a true meaſure of the humor. As the hope of attaining an end, is one of the moſt powerful aſſiſtants towards really arriving at it, I flatter myſelf at leaſt that I ſhall have given birth to a reaſonable one upon this ſubject.

Firſt Views to improve the HYGROMETER.

66. The idea I entertain, that it is neceſſary a number of attentive men ſhould concur, to improve the human inventions; has induced me firſt to mention the general reaſons I had, for hoping that the hygrometer would be perfected. I ſhall now proceed to give ſome particular reaſons on which this hope is founded, and which are collected from the remarks I have already made upon my inſtrument, during the little time I have had to obſerve it.

The firſt, and one of the moſt important of theſe remarks, is, that the ivory pipe belonging to that hygrometer which is always the higheſt upon its

ſcale,

ſcale, happens at the ſame time to be the thinneſt of them all. What connexion there may be between theſe two circumſtances, muſt be determined by experience. But in the mean time it appears to me that if the fibres of the ivory are interwoven with each other; they will make ſo much the leſs reſiſtance either to the being ſeparated or brought cloſer to each other, in proportion as the bundles of theſe fibres have a leſs degree of thickneſs. Whether this remark is of conſequence, or not, we ſhall at leaſt run no riſque in making theſe ivory pipes always exactly of the ſame thickneſs. This indeed was my intention in thoſe which I have made, but unfortunately I thought I ſhould have been able to turn them upon cylinders of hard wood; and found too late, that no accuracy could be expected from this method. It was to remedy this inconvenience, that, in ſpeaking of the manner of turning this piece, I have recommended braſs cylinders (31).

67. The ſame precaution is likewiſe neceſſary to be taken, that we may be certain of giving to every pipe an equal degree of thickneſs throughout the whole of its circumference: a circumſtance no leſs eſſential than the former; ſince I have obſerved in thoſe of my hygrometers whoſe pipes have not an equal thickneſs, that they bend, more or leſs according to the degree of humor to which they are expoſed.

This is probably the principal cauſe why theſe inſtruments do not always preſerve the ſame proportions to each other (58). For the pipes not bending according to the ſame law, there muſt be an ir-

regular

regular change in their capacity, and consequently in the height of the mercury in the tubes. The differences of this kind which I have had occasion to observe, are not indeed very considerable; but, however trifling the cause of an imperfection may be, it is still useful to remove it; were it only to assist us in discovering causes of greater imperfections, by making their effects the more evident.

68. But to make the ivory pipes keep straight, we must attend to a circumstance still more important; which is, that the texture of the ivory be the same in the whole circumference of the pipe. There is a sensible difference in the organization of the external, middle, and internal parts of the same elephant's tooth: nor is it impossible that, besides this difference in the nature, and visible arrangement of its fibres, there may be another arising from their degree of tension; so that some fibres may be more disposed than others to relaxation, after the tooth has been cut to pieces. Suppose then that any of these differences should exist in a pipe, that is, if one of its sides should be more porous, or of a weaker texture, than the other; or if its fibres should be more disposed to relaxation; this pipe will take a bend, either for a constancy, or for a time; and the hygrometers in which it is used, will not of course agree with the others. We must therefore endeavour to make these pipes with a part of the tooth that is homogeneous: that which I believe to be most so, within a certain extent, and which for that reason I have advised, is, the part which is between the center and the surface, and at some inches distance from the apex of the tooth (30).

69. There

69. There is another reaſon why this different organization of the different parts of the elephant's tooth makes it neceſſary to determine exactly the parts that are to be made uſe of in hygrometers. Without this precaution it might happen that the pipes, which ought to be ſimilar in every reſpect, might be made of ſubſtances that really differed in their dilatability and ſenſibility; that is, of ſubſtances which the humor might affect more or leſs ſtrongly, or more or leſs quickly. This conſideration will perhaps oblige us to determine both the ſize of the tooth, and the diſtance at which the piece ought to be cut off from its apex: for the organization may with equal probability vary in teeth of different thickneſs, and from the apex to the baſe; as it does in the breadth of the ſame tooth. I was not ſufficiently certain of the ſucceſs of my inſtrument, to take all theſe precautions when I firſt ſet about it, but at preſent I believe them to be important.

70. There is ſtill another precaution, which indeed I thought neceſſary from the beginning, but which I could not manage as I wiſhed for want of proper tools; that is, to perforate the ivory pipe exactly in the direction of its fibres. For let the channel have ever ſo ſmall a degree of obliquity with reſpect to this direction of the fibres, theſe fibres will neceſſarily be cut in different places; which weakening the pipe where it happens, neither its dilatations nor its contractions can of courſe be regular.

71. I own here are a great number of precautions; but they will not ſurprize true philoſophers. They are accuſtomed to obſerve the operations of

 nature

nature closely; and know that the regularity of her proceedings is connected with a foresight whch is limited to us, by nothing but the limits of our abilities in tracing it; and consequently, when art attempts to imitate nature, it can only succeed in as much as it is attentive to imitate her care.

72. I believe that the hygrometer may farther acquire a perfection of the same kind as that which, in conformity to an idea of my worthy friend Mr. Le Sage, I have given to the thermometer; that is, that we may make its degrees correspond with equal differences in the humor; as I have made those of the thermometer correspond with equal degrees of heat. The way in which I think this might be done, would be to suspend near one of the hygrometers, in a proper vessel which should be placed in one of the scales of a balance that turns very easily, some substance remarkably greedy of the humor; the augmentations or diminutions of weight in which substance, might be compared with the going of the hygrometer, first in the same, and afterwards in different degrees of heat. I hope too, that by a frequent repetition of these observations, at times when the variations of the humor are more or less sudden, we shall at last succeed in correcting the errors that may attend them, from the loss of its own matter the substance made use of may probably suffer by evaporation.

73. These are not the only remarks I have made upon my instrument, but I did not care to mention any but such as have appeared to me most certain. The others are uncertain, and require longer observations. I shall only add therefore, that it will still

be neceſſary to make freſh experiments, in order to determine the length of time that the ivory pipes ought to remain in water, and how long they muſt afterwards be expoſed to the viciſſitudes of the air (28), or in general to what preparation they muſt be ſubmitted, in order to acquire a laſting degree of conſiſtence before they are made uſe of. For this purpoſe it will alſo be expedient to compare hygrometers recently made, with older ones, both to aſcertain whether they have undergone any alterations, and in what degree. I likewiſe am of opinion, that when we wiſh to fix the point of extreme humidity, we muſt be very careful not to make uſe of any ice but what is very clean, as well internally as externally; leſt any duſt ſhould ſtick to the ivory pipes, which might hinder the water from penetrating into the pores: this is what I thought of myſelf too late. I do not know whether for the ſame reaſon it would not be right to waſh theſe tubes with ſpirits of wine before we put them into the water, to remove any greaſy ſcurf they may have gathered by handling; and afterwards to repeat this at times, in order to carry off any little depoſit of various kinds, which may in courſe of time have been left upon them by the air. Moreover it will be right to inquire whether there is not a difference between the effects of the heat upon the ivory of the hygrometers, and upon the glaſs of the thermometer, ſenſible enough to be attended to, in correcting the effects of this cauſe upon the hygrometer.

74. Having already diſcovered ſo many cauſes, more or leſs probable, of the differences I obſerved

in my hygrometers, I think it reaſonable to hope that this inſtrument will receive a ſenſible degree of perfection on a ſecond trial; and that in time it will be brought to a ſufficient degree of accuracy. It is true there are ſome difficulties in the way of this: but have we not ſufficient motives for endeavouring to overcome them? The air we breathe, and that which ſurrounds us; the places we inhabit, and thoſe which ſerve either to encloſe or to preſerve ſo many different bodies intended for our ſeveral occaſions, are all of them more or leſs filled with that ſubſtance, differently modified, to which I have given the name of *humor*. It alſo produces very ſenſible effects in them; ſome of which very properly excite our curioſity, others may be turned to our advantage, and many of them eſſentially affect our health. It is therefore of great conſequence to natural philoſophy in general, and to œconomy, and medicine, in particular, that we ſhould obtain a meaſure by which we may, with ſome degree of certainty, eſtimate the local and actual qualities of this ſubſtance, and by this means foreſee its effects; which for the generality we only become acquainted with after they are produced. Theſe ſciences are not perhaps leſs concerned that we ſhould diſcover the nature itſelf of this agent, and the different manners in which it operates: the knowledge of which may enable us to avail ourſelves of reaſon in the inveſtigation of certain effects, which, without ſuch helps, might eſcape our obſervation. As theſe are the ſeveral uſes of an exact hygrometer, we may readily perceive how many new tracks ſuch an inſtrument may open to us, in our inveſtigations of nature, which however

we ſhall not owe to one man alone, but to the joint labours of ſeveral.

Account of ſome of the firſt Phænomena of the HUMOR *obſerved with the* HYGROMETER.

75. Though my firſt advances in this new track of obſervation are as yet very uncertain, yet I will not omit giving ſome account of them. They will at leaſt ſerve to give ſome idea of the going of the inſtrument, as well as of the nature of the agent by which it is governed.

The firſt obſervation I attempted of this kind was with a view to one of the objects which made me deſirous of having an hygrometer. Theſe objects are all comprized in a general ſyſtem concerning vapours, which I have given in my work upon the *Modifications of the Atmoſphere.* I ſhall therefore only mention here one particular conſequence of that ſyſtem, which it was my immediate point to verify; namely, that a certain augmentation of heat, we always perceive at every ſeaſon upon the approach of rain, is owing to a more than ordinary quantity of vapour; and that, on the contrary, it is to their diminiſhed quantity that the leſſer heat of the upper parts of the atmoſphere is in great meaſure to be aſcribed.

76. This latter conſequence was ſupported by an accidental obſervation I made in September, 1770, upon a mountain of the Faucigny, at the height of 1560 toiſes above the level of the ſea. An iron ferule, which ſerved to unite the ends of a cleft ſtick, and which had been fixed on the ſtick with a

hammer

hammer upon the plain, in fine weather, came off of itſelf upon the top of the mountain. When this happened, the thermometer, which I called common, though expoſed to the ſun, was only three degrees above 0; while on the plain it was at 18 in the ſhade. This phænomenon, joined with ſeveral others I obſerved at the ſame time, confirmed me in my opinion, that one of the reaſons why the upper parts of the atmoſphere have leſs heat than the lower, is, that they contain leſs humor.

77. With this notion, it became a very intereſting inquiry to know the different degrees of humidity habitual to the different heights of the atmoſphere. Of courſe therefore this was the firſt obſervation I thought of, as ſoon as I had added an hygrometer to the other inſtruments contained within the box of my portable barometer. I undertook therefore to aſcend Buet (the name of that high mountain) a ſecond time. My companions in this expedition were Mr. Dentan, a very intelligent young philoſopher, and my brother, who, having aſſiſted me in all undertakings of any difficulty, had been a witneſs of that fact which was the object of my preſent reſearches.

78. At our ſetting out, on the 29th of laſt Auguſt, the hygrometer was at 86 in my apartment, and the barometer at 27 p. 1 line. We were in hopes of fine weather, becauſe it is generally fair in this country when the barometer at Geneva is above 27 French inches. Soon after we ſet out, we began to perceive that the power of the ſun was greater than might have been expected for the ſeaſon. From this circumſtance I concluded that the barometer muſt fall;

fall, and in fact we found it lower at every place in our way, where we had before obſerved it in fine weather. The ſky, notwithſtanding, was ſtill clear, and continued ſo the next day, when we began to aſcend the mountain, about two o'clock in the afternoon, in order to paſs the night in the higheſt cottages, that we might have more time to gain the ſummit the next day.

79. Before we left Sixt (an abbey at the foot of the mountain), I expoſed the hygrometer in open air, and in the ſhade it ſtood at 94. The thermometer at the ſame time was at 19 in the ſhade, and at 24 in the ſun. At five o'clock we reached a place above 300 toiſes above the abbey; commanded on all ſides by mountains, and on that account called *Les Fonds* (or The Bottoms). Here we obſerved the thermometer and hygrometer. The former, when expoſed to the ſun, ſtood at 15$\frac{1}{4}$, and the latter roſe to 96 in the ſhade. We obſerved them again in the ſame manner about half an hour after ſix, in a place that was pretty open, and higher by 160 toiſes than the former. The thermometer ſtood at 15, and the hygrometer at 106. It wanted but a quarter of nine, when we came to the cottages where we were to paſs the night; though they were not above 30 toiſes higher than the place we ſtopped at laſt. The higher we went, the clearer the ſky appeared; in ſo much that, notwithſtanding the uſual augmentation of humor in the air after ſunſet, when the ſky is not clouded, upon expoſing the inſtrument to the air, about $\frac{1}{4}$ after ten at night, we found the hygrometer at 123, and the thermometer at 13$\frac{3}{4}$. They both fell in the night, and on our

ſetting

setting out the next morning, the former had got down to 109, and the latter to 12.

80. In the two laſt mentioned obſervations the hygrometer had been expoſed long enough to the open air, to conform itſelf to the degree of humor prevalent in the place; but we had not time for the obſervations I was moſt deſirous to make with accuracy. The hygrometer being uſually ſhut up in the box of my barometer, it would have been neceſſary to have left that open ſome time, in order that it ſhould adapt itſelf to the ſtate of the air, and we could allow but a very ſhort time for theſe obſervations.

81. The firſt of them was made at nine in the morning, at the height of about 1000 toiſes above the plain. The ſky appeared clear over head, but the plain was darkened with vapours. The thermometer in the ſun ſtood at 13¾, and the hygrometer roſe to 115 in the ſhade.

82. It was two in the afternoon when we reached the top of the mountain, which is always covered with an enormous maſs of ice and ſnow. We found there a very ſtrong ſouth wind, which is the warmeſt wind in our plains: beſides this, we were nearly at the hotteſt time of the day: and yet the thermometer, upon being expoſed to the ſun, ſhewed only 6. The wind, and the coldneſs of this region, obliged us to quit the ſummit in a quarter of an hour, during which the hygrometer had riſen only to 119; but we judged that it was not yet ſtationary.

83. In this ſhort time we experienced a new effect of the diminiſhed humidity of the air, which ſurprized us all three very much. We found our ſkin withered

withered and pale, ſo that both to the ſight and to the touch, it reſembled much a dry and ſhriveled bladder. Notwithſtanding this we were ſenſible of no other inconvenience but what aroſe from the wind and the cold: the action of the lungs and the functions of all the other parts of the body were perfectly free, though the barometer was only at 19 inches, 6 lines and a half.

84. We quitted the ſummit at about a quarter after two, to ſhelter ourſelves from the wind behind ſome rocks, which were nearly 50 toiſes lower. Here we ſtayed about an hour. During this time the hygrometer, expoſed to the air, but always in the ſhade, roſe by imperceptible degrees to $132\frac{1}{4}$. It would probably have riſen higher, had not we been obliged to quit this place, where the clouds began to gather, in order to reach the cottages before night. It was indeed already too late before we thought of retiring; for we were overtaken by the night, and a thunder ſtorm, at a ſufficient diſtance from our hut to expoſe us to the greateſt danger of being loſt, notwithſtanding our guides, but for the aſſiſtance of two women, whoſe humanity deſerves the higheſt commendations. Theſe women, who lived in our cottages, being apprized of our diſtreſs by our cries, notwithſtanding the ſtorm, and the ſcarcity of wood in theſe places, came out to kindle a great fire at the foot of the rocks on which we were wandering amidſt the precipices, in total darkneſs; and ſometimes with great difficulty keeping the fire alive, ſometimes advancing towards us with fire-brands till the wind and rain extinguiſhed them, and endeavoured, with the moſt unaffected coucern, to point out to us the

path we ought to keep. At laſt, animated by the courage of theſe women, directed partly by their light and partly by their cries, we at length reached the cottage, much more affected with the humanity of theſe good people, than hurt with the dangers and fatigues we had undergone.

85. The ſtorm laſted a great part of the night, and it rained almoſt without intermiſſion. Notwithſtanding this, the hygrometer, when expoſed to the air the next morning, ſtood at 105, and the thermometer at 10. As we were uncertain how long the rain would continue, we ſet out at eight in the morning on our way down. The rain hardly ceaſed the whole morning, and was ſometimes accompanied with hail; it ſtill continued raining when we arrived at the abbey about noon, notwithſtanding the hygrometer ſtood there at 99, that is to ſay, five degrees higher than when we ſet out; but the barometer, which had fallen the two preceding days, was now beginning to riſe; the thermometer was at 14.

86. We learnt at Sixt, that, at the very time we were driven from the ſummit of the mountain by the diſagreeable coldneſs of the air, they had felt an exceſſive degree of heat, and likewiſe that the ſtorm had been very violent in the night. This ſtorm, as we found two days after at Geneva, had extended itſelf all over the plain. We found likewiſe, from the obſervations that had been made there in our abſence, that a thermometer expoſed to the north, conſequently out of the ſun, had been at 23½, at the very time that ours, at the top of the mountain and in the ſun, had been only at 6.

87. As,

87. As, in mentioning the particular purport of the foregoing obſervations, I have not explained my ſyſtem concerning vapours, I ſhall not here ſtop to draw the conſequences that may be deduced from them in favour of this ſyſtem. Indeed, to ſay the truth, I think them too few and too imperfect to conclude any thing from them as yet. I have only related them, as I declared at firſt, to give a general notion, both of the going of my hygrometer, and of the inquiries that may be purſued with its aſſiſtance. It is with the ſame intention that I proceed to relate ſome obſervations of another kind.

88. Some accidental obſervations had made me ſuſpect that the immediate action of the ſun upon my hygrometer produced a drying, which might not be wholly occaſioned by the real ſtate of the air with reſpect to the humor, but might depend in ſome meaſure upon ſome ſingular property of the ſolar rays, which we ſee produce effects upon ſome bodies, not immediately to be accounted for by the ordinary laws of heat. This firſt remark induced me, as I have taken care to mention, always to obſerve the hygrometer in the ſhade upon the mountains of Sixt. At my return, I determined to examine more accurately whether my conjecture in this reſpect had any foundation.

89. The firſt thought that occurred to me for this purpoſe was, to obſerve two hygrometers at the ſame time, one in the ſhade and the other in the ſun, very near each other, that the ſame air might circulate freely round them. The air of the country having appeared to me more proper for this obſervation than

that of the town, I determined to obſerve at the ſame time the variations of the humor in the open air for a whole day together. There are doubtleſs many varieties in this reſpect; nor indeed ſhall I determine from this obſervation, any thing more than the ſtate of the open air during one day, and in one particular ſpot.

90. I made my obſervation the 13th of September 1772, in a garden ſituated to the weſt of our lake, and only ſeparated from it by another garden and ſome buildings. There I hung up a couple of hygrometers which I kept perfectly inſulated, one of which had no other frame but a ſcale fixt to its tube, and the other was in a frame whoſe opening at the height of the ivory tube was of a conſiderable ſize. They were four feet and a half above the ground, and at the diſtance of a foot from each other. A piece of paſteboard about 12 inches in breadth, placed at a foot's diſtance from the hygrometer which was not mounted, was intended to ſhelter it from the ſun. Each hygrometer had a thermometer cloſe to it, the ball of which was not in contact with any thing. I have proved in my work, that it is neceſſary to keep this ball inſulated, in order to obſerve the heat of the free air.

91. One of the hygrometers I made uſe of for this obſervation, was at 93, and the other at 96½ in my room the night before. In order to correct this difference, which I ſhould ſuppoſe to be in proportion to their height, I would always add about $\frac{1}{27}$ to the height of that one which kept itſelf the loweſt, that there might be no difference between them, but ſuch as ſhould be produced by the difference

ference of the quantity of the action of the humor. The hygrometer which remained at the lowest was the one that was always in the shade, and was not mounted. It was the same upon which I had made my observations in the mountains of Sixt. I suspended them both in the garden I have been speaking of, about 6 in the morning; the plants were covered with dew; the sun, being just risine, could not yet shine on the garden. As soon as the hygrometers were exposed in the open air, they both fell very rapidly, but the one which was without the frame fell much faster than the other. They both were continuing to fall, when the sun began to shine in the garden. The following is an account of their progress, and of that of the thermometers during 19 hours. The action of the heat upon the mercury of the hygrometer is corrected upon each of them, from the observation of the thermometer joined to it, so that there only remains that of the humor.

A Table of Observations made on the 13th of September, on two Hygrometers, the one in the Shade, and the other in the Sun, each of them accompanied with a Thermometer.

	Hour of the Day	Therm. in the Shade.	Hygrom. in the Shade.	Hygrom. in the Sun.	Ther. in the Sun.
The Bar. at 27 Fr. inch. 1 line. The ſun did not ſhine yet on that part of the garden.	7	8	29	36½	8
The ſun has now ſhone for ¼ of an hour on the Hygr. and Therm. which are to ſtand expoſed to it.	7½	11¾	36½	66½	12
	8	12¼	43½	82	12½
	9	13	67	102	13½
	10	14½	76½	109	15½
	11	15	87½	116	16¾
	Noon	15½	96½	120½	17¼
	1	16¾	103	126	18
The vapours condenſing in the air weaken the action of the ſun.	2	16¼	103	125	17¼
Barom. 27. inch. A South wind begins to blow.	3	16¾	102½	123	17¼
The clouds riſe.	4	15¾	107	133	16
The clouds meet, and the ſunſhine withdrawn.	5	13¼	88½	106	13¼
The ſun is ſet, and the weather quite overcaſt.	6	12	64½	81	12
Barom. 26 inches 11 lines. . .	7	11¼	50	65	11¼
	8	11	37	50	11
	9	10¾	31	41	10¾
The clouds break, and the dew begins to appear on the plants.	10	10½	24	35	10½
	11	10	20½	26½	10
The clouds meet again. . . .	Mid.	10½	24½	28½	10½
	1	11¼	23	27	11¼
It begins to rain.	2	11½	27	32	11½

92. The

92. The firſt circumſtance in theſe obſervations that deſerves to be noticed, is the difference in the ſinking of the two hygrometers when they were expoſed to the air, before the ſun ſhone in the garden. They both of them fell conſiderably, but one of the two 7 degrees and a half leſs than the other. One of the cauſes of this diſparity is probably in the inſtruments themſelves, and is owing to their being differently affected by the action of the humor. There is a difference of the ſame kind obſervable in the thermometers, which are likewiſe more or leſs ſenſible to the impreſſions of the heat even when the bulk of their liquid is the ſame; that is to ſay, they are acted upon more or leſs quickly by the degree of heat which ſurrounds them, according to the thickneſs, or even according to the nature of the glaſs of which the ball is made. Conſequently it is poſſible that the different thickneſs or poroſity of the ivory may have had ſome influence on the going of the hygrometer in this obſervation (66 and 69).

93. But theſe differences in the ivory pipes muſt produce a much greater difference in the ſenſibility of the hygrometers, than thoſe of the glaſs balls can produce in the thermometers; becauſe it is much more difficult for the humor to penetrate the ivory, than for the heat to get through the glaſs. So that any encreaſe of the obſtacles retards the introduction of the humor, much more than that of the heat; and conſequently the difference of ſenſibility muſt be more difficult to be prevented in the hygrometers, than it is in the thermometers.

This ſlowneſs of the humor in pervading the bodies into which it inſinuates itſelf, makes it a de-

ſirable circumſtance, that the ivory pipe of the hygrometer ſhould be the thinneſt poſſible; in order that it might be more readily affected. This I had foreſeen, before I had learnt it from experience; but I was afraid of its being attended with ſtill greater inconveniences than that it was intended to remedy; from the action of the mercury againſt pipes whoſe ſides would be thinner. However, this might be tried. In the mean time, I fancy that, for obſervations in which it is abſolutely neceſſary that the inſtrument ſhould eaſily be affected, leſſer hygrometers might be made, whoſe tubes containing a leſs quantity of mercury, would reſiſt the action of it, though with a leſs degree of thickneſs. (Perhaps it would not be impoſſible to uſe tubes made of ſome very thin quills.) I cannot yet aſcertain whether theſe little hygrometers could be graduated by themſelves, or whether they muſt be compared with thoſe of which I have given the dimenſions; this we ſhall learn from experience.

94. The difference there is between the heat and the diſcrete humor in the power of diffuſing itſelf, occaſions in another reſpect a conſiderable difference in the goings of the thermometer and hygrometer. The heat is brought into a ſtate of equilibrium much ſooner and with much greater certainty than the humor. Two thermometers accurately conſtructed and fixed near each other, in a place where the heat does not change very ſuddenly, always agree together. This is not the caſe with two hygrometers: they ſeldom agree, that is, they ſeldom preſerve the ſame conformity to each other, when there is the leaſt variation in the humor: at ſome times their difference increaſes, at others it diminiſhes; this can only

only arife from a difference in the caufe that acts upon them.

95. We may form our ideas of the manner in which the invifible humor diftributes itfelf; from that in which all kinds of vifible vapours are diffufed. We fee them feparate, re-unite, fly off from certain places, rufh into others, and in fhort yield to every impreffion of the air. The motion peculiar to their own particles, which I look upon as the caufe of their elafticity *, is not fufficiently rapid, and the vapours themfelves are too thick to overcome always the contrary motion of the air. This, I believe, is what conftitutes the chief difference between vapours, and the igneous fluid, as far as relates to the power of putting themfelves into a ftate of equilibrium in the air, which is moving. The current of air carried towards a chimney which has fire in it, frees the room from fmoke, and is but a very flight impediment to the diffufion of the heat through it.

96. Though the invifible vapours by reafon of their exceffive thinnefs are more capable of being put in equilibrium in the air than the vifible ones, they are very far from having this property in as great a degree as the heat. Which leads me to think, that part of the difference obferved between my hygrometers, even before funrife, may have been owing to the unequal diftribution of the humor, though the two inftruments were only at the diftance of a foot from each other, without the interpofition of any folid body.

* The fyftem I adopted on that point may be found in my work upon the *Modifications of the atmofphere*.

97. I ſhall not attribute intirely to the ſame cauſe, the great difference obſerved between my hygrometers, when one was expoſed to the ſun, while the other ſtood in the ſhade. The immediate action of the ſolar rays, or of the luminous heat, produces a variety of effects, which, as I have ſaid before, do not appear to follow the ſame laws as thoſe of dark heat. And if I may be allowed to propoſe a conjecture upon this particular point, before fuller experiments have been made, it ſhould ſeem, that the immediate action of the ſolar rays muſt occaſion a greater evaporation than what is produced by dark heat, even when they hold the thermometer at the ſame height. But let the cauſe be as it will, we ſee by this experiment, that in a ſection of air about a foot wide, through which the ſolar rays did not immediately paſs, the action of the humor upon the hygrometer was 23 degrees greater than in the place round about; though that of the heat upon the thermometers was only a degree and a quarter leſs; which leads us to conceive how many apparently ſmall cauſes may contribute to produce ſenſible differences in the diſtribution of the diſcrete humor.

98. Another uſe to be made of theſe obſervations is, to compare them with thoſe that I have made in the mountains of Sixt; in order to form a better judgment of the proportion between the different degrees of humidity, in the ſuperior and inferior parts of the atmoſphere. My hygrometer, held in the ſhade upon the ſummit of Buet, roſe to 132½, and was not yet ſtationary. This is pretty nearly the greateſt degree of dryneſs obſerved in the hygrometer expoſed to the ſun in the garden; while the

the one that remained in the ſhade, the ſame upon which the obſervation at the mountain had been made, was not in fact higher than 103, though marked in the table of obſervations at 107 (91).

99. But the difference between the obſervations made upon the mountain of Sixt, and thoſe I am ſpeaking of, was ſtill greater by much after ſun-ſet. The 30th of Auguſt, at a quarter after ten at night, I obſerved the hygrometer without ſide the cottage upon the mountain, and found it at 123 (79); and on the 13th of September following, in the plain, it was not higher than 31 at 9, and 24 at 10 o'clock. The wind was ſouth, and the height of the barometer upon the plain, pretty much the ſame during both the obſervations.

100. It is true that, notwithſtanding the ſimilarity of theſe circumſtances, theſe obſervations cannot be directly compared, on account of the diſagreement in ſome other circumſtances. In the firſt place, the difference of fourteen days at this ſeaſon of the year may have produced a ſenſible change in the ſtate of the air. There was already, for inſtance, a conſiderable difference in the degrees of the thermometer; it was at 13 and ¾ when the obſervation was made on the mountain, and no higher than at 10 on the plain. Beſides, at this time of night, there would always be an eſſential difference between the upper and lower parts of the atmoſphere, even though in the day time they ſhould have the ſame degree of humidity: for the vapours being condenſed after ſun-ſet, and thus producing a kind of dew, they muſt neceſſarily deſcend, and from this very cauſe be more abundant in the low grounds than

on the higher ones. I ſhall add, that though my hygrometer was expoſed to the open air on the mountain, as it was in the plain, yet it was not ſo much inſulated there, being tied to the box of my portable barometer. The difference obſerved, however, is ſo conſiderable, that, notwithſtanding the concurrence of all theſe particular cauſes, I cannot but aſcribe it in ſome meaſure to that general one which I have ſuſpected, namely, that there is comparatively a leſs degree of humidity in the upper than in the lower parts of the atmoſphere.

101. The obſervation of the 13th of September ſeems likewiſe to throw ſome light upon the phænomena of dew. We know that when the ſky is cloudy, there is little or no dew, and it has likewiſe been obſerved from this very circumſtance, that the air is not ſo much cooled aſter ſun-ſet. The cauſe of theſe differences appears to me to be, that when there are no clouds in the air at ſun-ſet, or when they are diſperſed, the heat of the inferior air, and that which riſes from the earth, diſſipates itſelf into the ſuperior regions, and then the vapours which are diſperſed throughout the air condenſe and fall down again in dew; but when the clouds are continued, and thus ſeparate the inferior from the ſuperior air, they prevent this diſſipation of the heat, and the vapours remain ſuſpended. And if the ſky grows cloudy ſome hours after the ſetting of the ſun, and after the heat has ſenſibly diminiſhed in the inferior air, it encreaſes again in it; becauſe the heat, which continues to riſe out of the earth, is accumulated in the inferior air. This appears in the obſervation I am ſpeaking of. The clouds having been ſepa-

rated for a while, at 10 o'clock there was ſome dew, and the hygrometer fell ſenſibly till eleven: but afterwards the clouds cloſing again, the heat encreaſed, and the humidity evidently diminiſhed.

102. I take it for granted here, that the moſt common and moſt plentiful dew proceeds from the air, and not from the earth, as ſome philoſophers have imagined. I ſhould produce the proofs I have collected of this fact from a multitude of experiments, if it had not been done in an excellent paper, written by Profeſſor le Roi, *On the elevation and ſuſpenſion of water in the air* *. Theſe phænomena of the dew become very intereſting examined with the help of the hygrometer, and joined to obſervations of the degrees of ſaturation of the air with reſpect to water, which have been ſo ingeniouſly imagined, and begun by the author of this memoir. If this part of natural philoſophy is ever cleared up, as I hope it will be, we ſhall be much indebted for it to the ſagacity of this true philoſopher.

103. I ſhall only mention one more obſervation I have endeavoured to make with my hygrometer, which ought not to be omitted, as it is connected with the principles upon which the inſtrument is conſtructed. It has likewiſe a reference to medicine, in as much as one of the objects of that ſcience, in its inquiries to preſerve our health, is to determine the effects of water at different degrees of heat upon our organs. Ivory being an animal ſubſtance, the effects produced upon it by water at different degrees

* Mem. de l'Ac. des Sc. de Paris, for the year 1751.

of

of heat, may aſſiſt us in diſcovering thoſe which are produced upon our bodies from the ſame cauſe.

104. The point o of my hygrometer, as I have before obſerved (44), is that of the extreme humidity produced by melting ice. It was therefore of ſome importance to know what difference there would be in this point, when the hygrometer ſhould be plunged into warmer water. This I endeavoured to find out; and the following is the reſult of my firſt inquiries.

105. The moment I took one of my hygrometers out of melting ice, I plunged it into water at the heat of 45 degrees of the thermometer that I have called common. It fell ſuddenly four of its degrees below the thread which marked its height in the melting ice, but immediately roſe again, and in four minutes reached 8 degrees and a half above the ſame thread. Deducting $22\frac{1}{2}$ from the height for the dilatation of the mercury (48), there will remain 14. Conſequently the water warmed at 45 degrees of the common thermometer, really made the hygrometer ſink 14 degrees below o.

106. Half an hour after this, the water being at 38 degrees, I found the hygrometer no higher than $6\frac{1}{2}$, that is to ſay, $6\frac{1}{2} - \frac{38}{2} = -12\frac{1}{2}$. Conſequently the true point of humidity indicated by the hygrometer was $12\frac{1}{2}$ below o. Laſtly, the heat of the water being reduced to 28 degrees the hygrometer was at $3 - \frac{28}{2} = -11$. I was then obliged to put an end to the experiment, which I have not been able to take up again ſince, for want of leiſure. But

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what has already been obferved is fufficient to fhew us that the warmer the water is, the more it dilates the ivory (though we faw that the mercury rofe in the hygrometer after having funk for a moment). From hence, I fancy, may be drawn this general confequence, already indeed forefeen, namely, that in *an equal acting quantity*, the warmer the humor is, the more it feparates the particles of thofe bodies which it pervades.

107. I fay, in an *equal acting quantity*; and this is one of the objects which will probably furnifh us with a variety of moft ufeful knowledge, at the fame time that it is moft likely to give the greateft exercife to the genius and attention of natural philofophers. The forementioned experiment proves, that the warmer the water is, the more it dilatates the ivory pipe of the hygrometer, and the fame thing I make no doubt happens with the difcrete humor.

On the other hand, the evaporation being certainly greater in fummer than in winter, there muft of courfe be more vapours in the air in the firft of thefe feafons than in the latter. Thefe then, as it appears, are the two circumftances moft likely to make the hygrometer fall in fummer; a greater degree of humor in the air, and an encreafe of heat. And yet I have already experienced that the mean height of the hygrometer is greater in fummer than in the other feafons. I found my firft hygrometer, which was made in winter, too fhort in the fummer; but it would be of a fufficient length now that we are in autumn. The mean height of the four new ones is already (the beginning of November)

ber) 17 degrees leſs than it was in the months of Auguſt and September.

108. I hope this paradox will be explained, and that the principles which may clear it up will draw uſeful conſequences along with them. Thoſe philoſophers who look upon evaporation as a diſſolution of water by air in the manner of menſtrua, that is, by affinity, will eaſily apply their principle to the ſolution of part of theſe phænomena. The diſſolution is greater when the menſtruum is warmer, and conſequently the air muſt keep a greater quantity of water in diſſolution, and ſuffer a leſs part of it to be precipitated, in ſummer than in winter. I cannot but allow that this ſyſtem is extremely ſpecious, and that many phænomena are very happily explained by means of it. This is what Mr. le Roy has ſhewn us in the memoir I have already quoted; in which, without contending that air really acts as a menſtruum with reſpect to water, he demonſtrates, by a parallel very well kept up, that all the chemical expreſſions concerning diſſolutions may with propriety be applied to deſcribe the ſeveral phænomena he examines, relative to the elevation and ſuſpenſion of water in air, as well as to its precipitation under different forms.

109. If it was not too common a practice, to conclude things from words, I ſhould in fact think theſe chemical expreſſions very conveniently adapted to explain a number of theſe phænomena. But I have rejected them here, on account of this conſideration; that when I took in a greater number of phænomena, I found them no longer accurate, any more than the general idea of the diſſolu-

tion

tion of water by air. I have given the reasons for this in my work, upon the modification of the atmosphere; and shall only repeat here, that these modifications of the humor appear to me almost intirely to be produced by the igneous fluid; and that if the air has any share in them, it is only as being an elastic fluid. The particles of these fluids, each according to its degree of power, strike, separate, and draw along with them those of the humor, and communicate to them the elasticity they possess; in the same manner as they do to the particles of all volatile, and likewise of all fixt substances which they corrode and decompose.

110. This system will not only furnish a solution of the paradox which engages our attention, but will, I believe, carry us much farther. The heat of the summer keeps the humor in very great agitation; and though there is more of the humor at this season than in winter, yet this heat will not allow it to continue either as long a time, or in as great a quantity, upon the bodies or in their pores. That is the reason why the hygrometer falls less. But we see at the same time, that the portion of the humor which does sojourn, and which I call the active part, has more power to dilatate the bodies, from the greater degree of motion impressed upon it by a greater heat. Consequently the dilatation of the bodies, from this cause, will be in a compound ratio of the quantity of humor, and of its active force, or of the heat. And if, for instance, we compare any summer's day, in which the hygrometer in open air is at the same degree as on any winter's day, the air on the summer's day will contain more humor than on the

winter's day; but there will be leſs of it will act upon the hygrometer; and yet as the active part will have more ſtrength, the effect upon the whole will be the ſame. This is what appears to me, but I can not now enlarge any farther on this ſyſtem. I have ſaid enough to ſhew that the ſubject is very expenſive, and deſerves an attentive examination.

Fig. II.

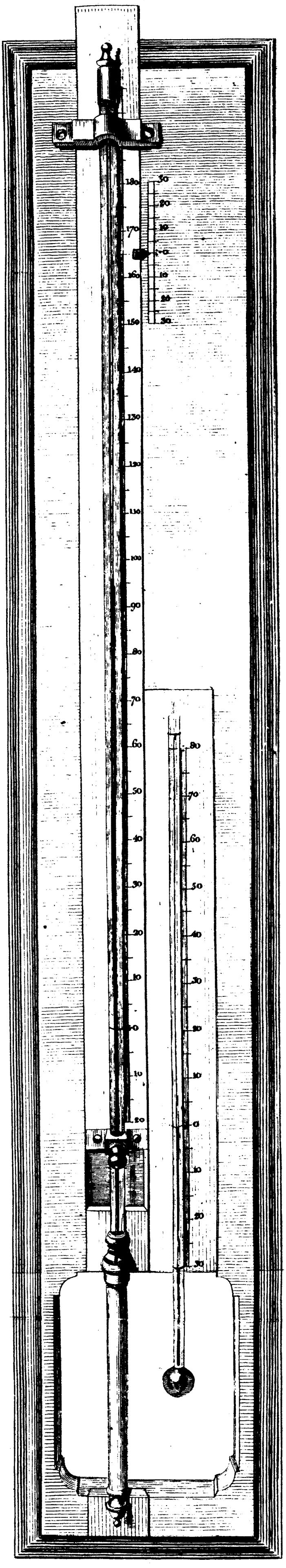
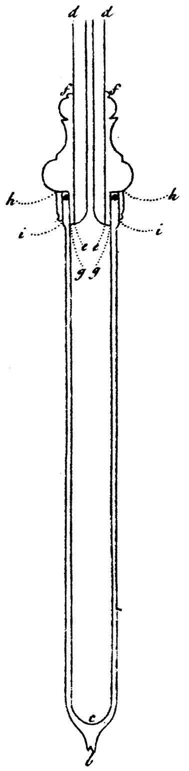


Fig. I.



Hygrometer.

J. Basire Sculp.