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1767

A C C O U N T
OF A NEW
H Y G R O M E T E R.

By M. J. A. D E L U C,
Citizen of *Geneva*, F. R. S. and Correspond. Member of the
Academies of *Paris* and *Montpellier*.



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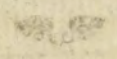
A C C O U N T

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Chosen of Gouvt. F. R. S. and Correspond. Member of the
Academies of Paris and Stockholm.



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M D C C L X I V .

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.

Read June 10, 1773. **I**N 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,

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meter,

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 phenomena 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 appear to
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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 phenomena, 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 phenomenon; 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

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 ofteneft, 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, Another

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 any 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 surpris'd how it could
have

have been so long overlooked. But I afterwards accounted for this, from the difficulties which I met with in the discovery. The notion of an hygrometer being both complex and unsettled, all the obstacles presented themselves at once, and this multitude of ideas exceeded the power of attention. The very first steps were apt to mislead. On the one hand, I looked for an hygrometer with the head full of the matters already used for hygrosopes, which always are more or less altered by water; and, on the other, the name of humidity was applied to that cause, the effects of which I wished to measure; and both points of view turned aside the mind from the idea of water, as being proper to afford the required fixed point in an hygrometer.

17. The first difficulty had not escaped me, but considered in itself it did not appear unfurmountable. I was in hopes that a substance might be found capable of being affected by the soaking power of water, without being altered by it. As the nature of this substance was to determine not only the form of the hygrometer, but also the species of the degrees, which were to indicate the different quantities of humor, I concluded that my second object ought to be the discovery of this substance.

18. In this research, I again divided the objects, by considering separately the three kingdoms, viz. the mineral, the vegetable, and the animal. The two first offered no substance fit for my purpose, viz. none that would obey the impressions of humor, without being altered either by it or by other causes. But in the animal kingdom bones drew my attention; and ivory, in particular, seemed to possess the
required.

required qualities. I had observed that the key of an ivory cock was tighter or flacker, 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 variations

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tions were to be the removal or approach of its fibre 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
C 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 $\frac{1}{4}$ 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

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 transfusion; and the capacity of the vessel, which encreased till the appearance of the dew, seemed afterwards at a stand.

26. This transfusion 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 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 inferior 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, *aab*, open at the end *aa*, 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 *aa* to *c*.

31. Prepare

31. Prepare after this a brass cylinder, about $3\frac{1}{2}$ 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
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in *dd ee* (fig. 1.). Its internal diameter is about $\frac{1}{4}$ 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 *gg* 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 *ff gg* 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 vessel, represented in *bbii*. 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

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 revived 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).

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43. II

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 0 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 basin, 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

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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 deal-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 $\frac{2}{3}$ 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
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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 0 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 0 in the scale of the hygrometer. This is the case expressed in the figure wherein the thermometer is likewise represented at 0 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. 0 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

Fig. II.

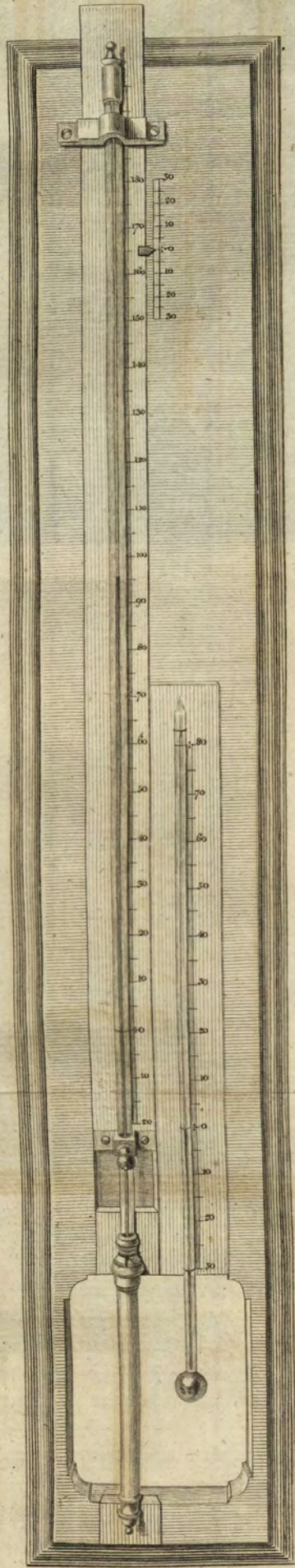
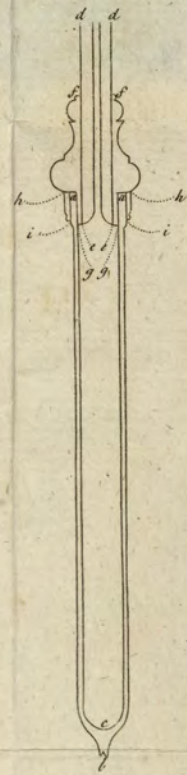
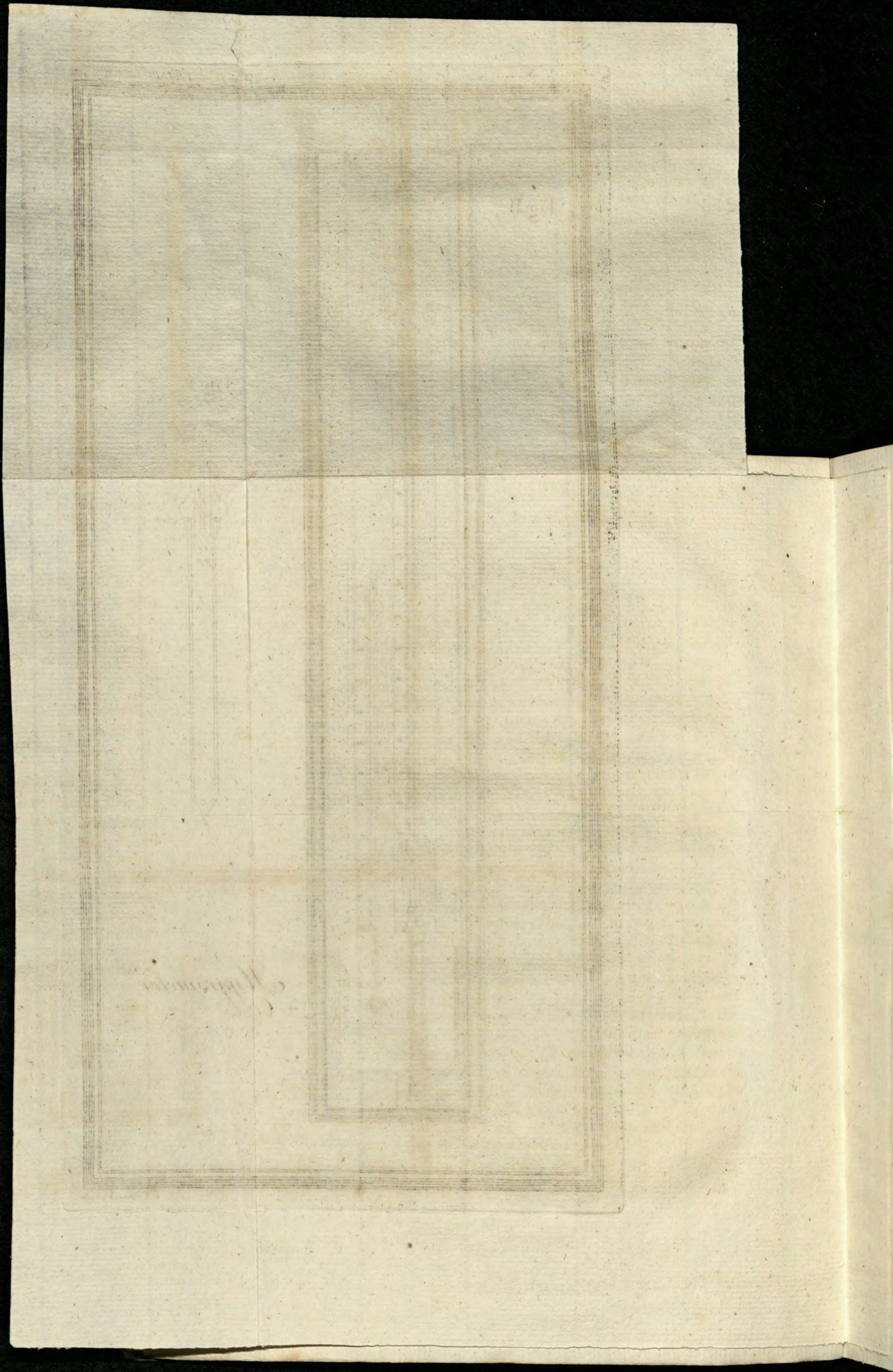


Fig. I.



Hygrometer.

J. Basse sculpt.



*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 $9\frac{1}{2}$.

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 $23\frac{1}{2}$. 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
being

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

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shut, has hitherto been 94 , $99\frac{1}{2}$, $100\frac{1}{2}$, $105\frac{1}{2}$, 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 hygrosopes, 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,

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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 hy-

* 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,

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, happens at the same time to be the thinnest of them all. What connection 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 irregular

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).

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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 dilatibility 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 which 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

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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 extremed 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
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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 tracts 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 Phenomena 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, 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 $13\frac{1}{2}$. 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 $13\frac{3}{4}$, 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 $132\frac{1}{2}$. 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 concern, to point out to us the path

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 $23\frac{1}{2}$, at the very time that ours, at the top of the mountain and in the sun, had been only at 6.

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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
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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 dif-

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 rising, 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.

piece 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 the ball insulated, in order to observe the heat of the free air.

19. One of the hygrometers I made use of for this observation, was at 93, and the other at 95, 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 2 to the height of that one which kept itself the lower; but there might be no difference between them, but such as should be produced by the difference

A TABLE

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.	The 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	16½
	Noon	15½	96½	120½	17¾
	1	16¾	103	126	18
The vapours condensing in the air weaken the action of the sun. }	2	16¼	103	125	17¾
Barom. 27. inch. A South wind begins to blow. }	3	16¾	102½	123	17½
The clouds rise. }	4	15¾	107	133	16
The clouds meet, and the sunshine withdrawn. }	5	13¾	88½	106	13¾
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 desirable

firable 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
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only arise from a difference in the cause that acts upon them.

95. We may form our ideas of the manner in which the invisible humor distributes itself; from that in which all kinds of visible vapours are diffused. We see them separate, re-unite, fly off from certain places, rush into others, and in short yield to every impression of the air. The motion peculiar to their own particles, which I look upon as the cause of their elasticity*, is not sufficiently rapid, and the vapours themselves are too thick to overcome always the contrary motion of the air. This, I believe, is what constitutes the chief difference between vapours, and the igneous fluid, as far as relates to the power of putting themselves into a state 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 smoke, and is but a very slight impediment to the diffusion of the heat through it.

96. Though the invisible vapours by reason of their excessive thinness are more capable of being put in equilibrium in the air than the visible 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 observed between my hygrometers, even before sunrise, may have been owing to the unequal distribution of the humor, though the two instruments were only at the distance of a foot from each other, without the interposition of any solid body.

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

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 132 $\frac{1}{2}$, 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

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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 $\frac{1}{4}$ 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
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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 phenomena 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 after 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 increases 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 separated

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

104. The point 0 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 0.

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 0. 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 what

what has already been observed is sufficient to shew us that the warmer the water is, the more it dilates the ivory (though we saw that the mercury rose in the hygrometer after having sunk for a moment). From hence, I fancy may be drawn this general consequence, already indeed foreseen, namely, that in *an equal acting quantity*, the warmer the humor is, the more it separates the particles of those bodies which it pervades.

107. I say, in an *equal acting quantity*; and this is one of the objects which will probably furnish us with a variety of most useful knowledge, at the same time that it is most likely to give the greatest exercise to the genius and attention of natural philosophers. The forementioned experiment proves, that the warmer the water is, the more it dilatates the ivory pipe of the hygrometer, and the same thing I make no doubt happens with the discrete humor.

On the other hand, the evaporation being certainly greater in summer than in winter, there must of course be more vapours in the air, in the first of these seasons than in the latter. These then, as it appears, are the two circumstances most likely to make the hygrometer fall in summer; a greater degree of humor in the air, and an encrease of heat. And yet I have already experienced that the mean height of the hygrometer is greater in summer than in the other seasons. I found my first hygrometer, which was made in winter, too short in the summer; but it would be of a sufficient 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 phenomena. 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 phenomena 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 phenomena 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 phenomena. But I have rejected them here, on account of this consideration; that when I took in a greater number of phenomena, I found them no longer accurate, any more than the general idea of the dissolution

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ton 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

winter's day; but there will be less of it will act upon the ygrometer; 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.

THE END.

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