Experiments and observations on the heat and cold produced by the mechnical condensation and rarefaction of air

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Experiments and Observations on the HEAT *and* COLD *produced by the* MECHANICAL CONDENSATION *and* RAREFACTION *of* AIR.

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If a thermometer be inclosed in a receiver and the air suddenly condensed, the thermometer rises a few degrees above the temperature of the atmosphere; and if the air be exhausted from a receiver inclosing a thermometer, the mercury sinks a few degrees immediately; but in both cases after some time it resumes its former station. These facts are well known to philosophers of the present age, but they do not all agree in the explanation of them. Thinking the subject worthy of elucidation, I was induced to institute a series of experiments for the purpose, which I apprehend have led to a clear demonstration of the cause of the phenomena, and moreover make the facts themselves appear in a somewhat different point of view from what they are seen in at the first moment.

One circumstance is very remarkable, that whether the mercury rises or falls in these instances, it is done *very rapidly;* whereas in the open air, if a thermometer be only two or three degrees above or below the temperature, it moves very slowly. This seems to have suggested to every one the idea that the elasticity of the glass bulb of the thermometer has a principal share in producing the effect, by causing the bulb to yield a little to the pressure of the air. It has however been found upon trial that the same effects take place whether the thermometer is sealed or not. My experiments accord with this, having made a thermometer and left it unsealed for the express purpose; in all the experiments with condensed and rarefied air, there was no sensible difference observed to arise from the inequality of pressure on the external and internal surfaces of the bulbs, the sealed and open thermometers varying the same in kind and also in degree, except from circumstances to be noticed hereafter.

It being certain then that a real change of temperature takes place, it remained to determine the quantity and manner of that change. Having chosen a small and consequently sensible thermometer, with a scale of degrees sufficiently large to admit of distinguishing one tenth of a degree, I proceeded to ascertain several facts experimentally.

EXPERIMENT 1.

Took a receiver, the capacity of which was about 120 cubic inches, and suspended the thermometer with its clear bulb in the central part of it; then letting the whole acquire the temperature of the room, which was without a fire, I exhausted the air and afterwards restored it, marking the effects upon the thermometer. The medium of several trials nearly agreeing with each other was as under:

The Thermometer

———in the air of the room stood at....36°.8
———sunk upon exhaustion to ....34 .7
———rose when the air was restored to..38 .9

The *suddenness* of the fall and rise puzzled me most: after reflecting upon it for some time, I conjectured that the real change of temperature of the air or medium was much greater than the thermometer indicated, but that the inequality existed only for a few seconds of time, because the receiver, &c. immediately impart heat to or abstract it from so small a quantity of air as 120 cubic inches, which are only equal to 40 grains in weight.—The phenomena of the thermometer seemed very well to accord with the supposition of *great heat or cold* acting upon it for a few seconds only.

EXPERIMENT 2.

Pursuing this idea I imagined that if two thermometers whose bulbs were very unequal in magnitude were inclosed together, the smaller bulb ought to give the greater variation: accordingly I inclosed two, the diameters of their bulbs being .35 and .65 of an inch respectively; and having exhausted the air and restored it again repeatedly in succession, and found a mean of the variations, that of the small bulb was 2°. 8, and that of the large, 2°. 2.

EXPERIMENT 3.

Repeated the exhaustion with the small thermometer inclosed in three different circumstances successively; 1st with the bulb in the centre of the receiver; 2d with the bulb resting on the wet leather of the plate; and 3d with the bulb resting against the side of the receiver.

1st Case—Reduced by exhaustion	2°.45
2d Case—	2 .15
3d Case—	1 .2
1st Case—Raised by restoring the air	4 .05
2d Case—	2 .25
3d Case—	2 .8

EXPERIMENT 4.

Inclosed a wine glass with about a cubic inch of water in it, containing the bulb of a thermometer, in a receiver; and, exhausting the air, the thermometer sunk half a degree suddenly, and then continued stationary; upon restoring the air it suddenly rose half a degree.

All these experiments confirmed my conjecture of a much greater degree of heat and cold being produced in these cases than the thermometer points out, but that its continuance is so short as not to effect a material change in the temperature of the mercury. The following experiments were made to ascertain what may be the *real* degree of heat and cold generated in those operations.

EXPERIMENT 5.

The same receiver & small thermometer as above being used, I found the exhaustion was effected by working the pump *one minute*. The thermometer sunk nearly 2° in the first half minute, and the remainder, a few tenths of a degree, in the latter half minute. The operation being stopped, and things remaining in the same state, it required some minutes of time before the thermometer recovered *one* degree of the heat lost. Upon opening the cock, the receiver filled with air in five seconds, and the greatest volocity of the rising mercury was about the end of that time. The rising continued for 30 or 40 seconds from its commencement, but $\frac{3}{4}$ of the effect were produced in the first 10 seconds. The greatest velocity of the rising mercury is 1° in $3\frac{1}{2}$ seconds. After the thermometer had attained its utmost height, it began to fall again at the rate of $\frac{1}{10}$ of a degree in a minute.

EXPERIMENT 6.

Took the same thermometer and heated it to 50° above the temperature of the air, then let it be cooled by the medium of air, and it began to fall at the rate of 1° in $3\frac{1}{2}$ seconds.

The two last experiments seem to prove that when air is let in to the receiver in the ordinary way, *an increase of temperature of* 50° *is produced in the medium within the receiver for* $3\frac{1}{2}$ *seconds.* This high temperature is reduced *in a few seconds* by the receiver and surrounding bodies, to their own temperature.

EXPERIMENT 7.

On condensed Air.

Took a large spherical glass receiver, the capacity of which was something more than twice that of the former (above one gallon), and suspended a thermometer in the centre of it, of a larger bulb than that before used; the receiver had a brass cap and stop-cock adapted to it: Then doubled the density of the air within it by a condenser. The thermometer rose 2° or more. Let out the air suddenly and the thermometer immediately sunk each time from 3° to 3°. 5; at the same time an exceedingly dense mist was produced in the receiver, which soon subsided.

Suspecting that *aqueous vapour*, which always exists in the atmosphere, and is liable to assume the liquid or aërial form according to circumstances, might be the principal agent in the production of heat and cold by condensation and rarefaction, I thought that an increase of it might produce a greater effect, and that cold air, which contains less vapour, might have a less effect. The reverse however was the fact, as appears by the following,

EXPERIMENTS 8 & 9.

In a cold morning last winter when the air was clear and the thermometer without stood at 20°, I took the receiver and condenser into the open air, and let them stand for 15 minutes to acquire its temperature; then repeatedly condensed the air to a double density, and suddenly liberated it again. On a medium of 5 trials the mercury fell 3°.3 on opening the cock.—The vapour precipitated was whiter than usual and not nearly so dense.

Again, took the receiver and condenser into a dyer's stove where the temperature was about 100°, and the air abounded with vapour in a transparent state: after some time, condensed the air and liberated it as before, when on a medium of 5 trials the mercury sunk only 3°, and a very copious mist was precipitated, so dense that one could but just distinguish the degree of the thermometer through it.

These experiments shew that the greater the quantity of vapour condensed the less is the change of temperature; and that consequently, if air was entirely free from vapour, the change of temperature would be a *maximum*. Indeed this is clearly consistent with the known law, that when vapour is condensed, heat is given out. Any process to cool the air must be retarded by the condensation of part of the vapour it contains. Suppose for instance that a portion of the atmosphere contained $\frac{1}{60}$ of its weight of aqueous vapour, and that $\frac{3}{5}$ of this vapour were condensed by 50°of cold; that is, $\frac{1}{100}$ of the whole elastic mass was converted into water; then the heat given out would be sufficient to raise the temperature of the remaining mass of air and vapour 6 or 8°, which sufficiently accounts for the small difference observed in the results upon warm vapoury air and cold dry air. Hence vapour, far from producing the change of temperature in question, tends to diminish the effect.

If any doubt remained with me respecting the *real* change of temperature that takes place in the operations related above, it was completely removed by the results of the two following experiments.

EXPERIMENT 10.

Inclosed a small graduated glass tube of $\frac{1}{15}$ of an inch internal diameter, & 10 inches long, with a short column of mercury in it, in the large receiver; the tube was sealed at one end and open at the other, so that a portion of air of given capacity was confined by the mercurial column, which was near the open end of the tube, and subject to rise or fall by any variation of elasticity of the air on either side, being a proper manometer: then doubled the density of the air in the receiver, and opening the stop-cock, the mercurial column soon ran up to its former station, but instantly turning the cock again, the mercurial column returned or fell down gradually for 5 or 10 seconds, to the amount of nearly $\frac{1}{10}$ of the whole aërial column, and then became stationary. Again opening the cock, a quantity of air rushed out, and the mercury resumed its original station. These effects were always the same, on a repetition of the experiment.

EXPERIMENT 11.

Let the mercurial column of the manometer down by a wire to $\frac{1}{4}$ of the length of the tube from the sealed end; then exhausted $\frac{3}{4}$ of the air from the receiver, which was seen by the mercury rising to the top of the tube; and upon opening the cock the mercury fell to its former station, but then suddenly turning the cock, the mercury gradually rose for the space of 5 or 10 seconds to *more* than $\frac{1}{10}$ of its original height above the stationary point, and remained there till the cock was opened; after which it resumed its proper station.

The phenomena in the two last experiments can be explained only on the following principle:—The air in the receiver and in the manometer is subject to a like degree of rarefaction and condensation in those experiments, or very nearly so. When the equilibrium of heat in the air is disturbed by the operations of condensation and rarefaction, it is restored in the manometer *instantly* by reason of the contiguity of the glass to the air; but in the large receiver it requires a sensible time of 10 seconds or more to restore the equilibrium throughout the whole internal capacity. It is this restoration that increases or diminishes the elasticity of the air confined in the receiver, and thereby causes the retrogradation of the mercurial column. Now I have found by former experiments that a change of 50° in temperature effects a change of $\frac{1}{10}$ nearly, in the capacity or bulk of air. It follows therefore that in the case of restoring the equilibrium in condensed air, about 50° of cold is produced; and in letting in air to an exhausted receiver something more than 50° of heat is produced. The small difference seems to arise from this, that the condensation of vapour in the former case *diminishes* the effect, and in the latter, if any there be, *increases* the effect, that would arise from operating upon purely dry air.

The experiments and observations hitherto related go principally to ascertain facts without any reference to the theory of them: This however may be given in a few words, and is the same that is ascribed to Mr. Lambert by Messrs. Saussure and Pictet and by them adopted. He conceives that a vacuum has its proper capacity for heat, the same as air, or any other substance; and that the capacity of a vacuum for heat is *less* than that of an equal volume of atmospherical air; also that the *denser* air is, the *less* is its capacity for heat: upon these principles the phenomena are easily referable to that class of chemical facts where heat and cold are generated by the mixture of two different bodies.—If this theory be right, and I think there is little doubt of it, we may hence be led into a train of experiments, by which the absolute capacity of a vacuum for heat may be determined; and likewise the capacities of the different gases for heat, by a method wholly new:—but this must be left to future investigation.