A verbal communication from Dr. Hare, respecting some experiments made by him to ascertain the comparative heating or cooling influence of changes of density resulting from changes in pressure in dry air, and air replete with aqueous vapour : also, some experiments favourable to the idea that a vacuum has a capacity for caloric.

Hare (R)

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A Verbal Communication from Dr. Hare, respecting some Experiments made by him to ascertain the comparative heating or cooling influence of changes of Density resulting from changes in Pressure in dry Air, and Air replete with aqueous Vapour: Also, some Experiments favourable to the idea that a Vacuum has a capacity for Caloric.

Dr. Hare adverted to the fact, that in an essay, published in Silliman's Journal in 1822, he had, agreeably to the authority of Dalton and Davy, stated, that the cold consequent on the rarefaction of air in its ascent towards the upper strata of the atmosphere, was one of the causes of the formation of clouds; and in his text books he had soon after published an engraving of an apparatus, by means of which he was accustomed to illustrate, before his pupils, the transient cloud which arises from a diminution of pressure in air containing aqueous vapour.

In the essay above mentioned, Dr. Hare had alleged, that as much caloric was given out by aqueous vapour, during its conversion into snow, as would be yielded by twice the weight of red-hot powdered glass. But Mr. Espy, he considered, had the merit of being the first to suggest, that the heat, thus evolved, might be an important instrument in causing a buoyancy tending to accelerate any upward current of warm moist air.

Dr. Hare had been willing to admit, that this transfer of heat might co-operate with other causes in the production of storms, but could not concur with Mr. Espy in considering it competent to give rise to thunder gusts, tornadoes, or hurricanes. These he had considered, and still considers, to be mainly owing to electrical discharges between the earth and the sky; or between one mass of clouds and another.

With a view to a more accurate estimate of the comparative influence of rarefaction and condensation, in causing evolution of heat in dry air, and in air replete with aqueous vapour, Dr. Hare had performed a number of experiments, of which he proceeded to give a description.

Large globes, each containing about a cubic foot of space, furnished with thermometers and hygrometers, were made to communicate, respectively, with reservoirs of perfectly dry air, and of air replete with aqueous vapour.* The cold, ultimately acquired by any degree of rarefaction, appeared to be the same, whether the air was in the one state or the other; provided that the air, replete with aqueous vapour, was not in contact with liquid water in the vessel subjected to exhaustion. When water was present, in consequence of the formation of additional vapour, and a consequent absorption of caloric, the cold produced was nearly twice as great as when the air was not in contact with liquid water; being nearly as 9 to 5.

Under the circumstances last mentioned, the hygrometer was motionless; whereas, when no liquid water was accessible, the space, although previously saturated with vapour, by the removal of a portion of it together with the air which is withdrawn by the exhaustion, acquires a capacity for more vapour; and hence the hygrometer, by an abstraction of one-third of the air, revolved more than sixty degrees towards dryness. But when a smaller receiver (after being subjected to a diminution of pressure of about ten inches of mercury, so as to cause the index of the hygrometer to move about thirty-five degrees towards dryness) was surrounded by a freezing mixture, until a thermometer in the axis of the receiver stood at three degrees below freezing, the hygrometer revolved towards dampness, until it went about ten degrees beyond the point at which it rested when the process commenced.

It appears, therefore, that the dryness produced by the degree of

* The hygrometers were constructed by means of the beard of the avena sensitiva or wild oat, also called animated oat.

rarefaction employed is more than counterbalanced by a freezing temperature.

As respects the heat imparted to the air above mentioned, the fact, that the ultimate refrigeration in the case of air replete with vapour, and in that of anhydrous air, was equally great, and that when water was present the cold was greater in the damp vessel, led to the idea, that the heat arising under such circumstances could not have much efficacy in augmenting the buoyancy of an ascending column of air: but when, by an appropriate mechanism, the refrigeration was measured by the difference of pressure at the moment when the exhaustion was arrested, and when the thermometer had become stationary, it was found, cæteris paribus, that the reduction of pressure arising from cold was at least one-half greater in the anhydrous air, than in the air replete with vapour. This difference seems to be owing to a loan of latent heat made by the contained moisture, or transferred from the apparatus by its intervention, which checks the refrigeration; yet, ultimately, the whole of the moisture being converted into vapour, the aggregate refrigeration does not differ in the two cases.

Agreeably to Dalton's tables, at 70° the quantity of moisture in 31 grains or 100 cubic inches of air, is $\frac{551}{1000}$ of a grain. The space allotted to this weight of vapour being doubled, it would remain uncondensed at 45° F., being associated with the same weight, but double the volume, of air; but at 32°, notwithstanding the doubling of the space, only $\frac{356}{1000}$ of a grain would remain in the aëriform state; of course $551 - 356 = \frac{195}{1000}$, or nearly $\frac{2}{10}$ of a grain, would be precipitated.

The latent heat given out by the condensation of this vapour, would heat, as is well known, 1000 times its weight of water, or 195 grains, one degree; or 31 grains $\frac{195}{31} = 6.29$ degrees; and as the capacity of air for heat is only one-fourth of that of water, it would heat 31 grains of air $6.29 \times 4 = 25.16$, or nearly 25° F. As air, at 32° F., expands $\frac{1}{480}$ for each additional degree, the difference of bulk, arising from the heat received, as above calculated, would be $\frac{25}{480}$, or $\frac{1}{19}$ nearly.

When air, replete with aqueous vapour, was admitted into a receiver partially exhausted, and containing liquid water, a copious precipitation of moisture ensued, and a rise of temperature greater than when perfectly dry air was allowed to enter a vessel containing rarefied air in the same state. In the instance first mentioned, a portion of vapour rises into the place of that which is withdrawn during the partial exhaustion. Hence when the air, containing its full proportion of vapour, enters, there is an excess of vapour which must precipitate, causing a cloud, and an evolution of latent heat from the aqueous particles previously in the aëriform state. Dr. Hare conceives that as the enlargement of the space occupied by a sponge, allows, proportionably, a larger quantity of any liquid to enter its cells, so any rarefaction of the air when in contact with water, consequent on increase of heat or diminution of pressure, permits a proportionably larger volume of vapour to associate itself with a given weight of the air. When, subsequently, by the afflux of wind replete with aqueous vapour, the density of the aggregate is increased, a portion of the vapour equivalent to the condensation must be condensed, giving out latent heat, excepting so far as the heat thus evolved, being retained by the air, raises the dew point.

Hence, whenever a diminution of density of the air inland causes an influx of sea air to restore the equilibrium, there may result a condensation of aqueous vapour, and evolution of heat, tending to promote an ascending current. This process being followed by that which Mr. Espy has pointed out, of the transfer of heat from vapour to air, during its ascent to the region of the clouds, and consequent precipitation of moisture, might, Dr. H. thought, be among the efficient causes of those *non*-electrical rain storms, during which water is transferred to the soil of the United States, from the Gulf of Mexico or the Atlantic.

Dr. Hare proceeded to mention some additional experiments which he had made, respecting the increase of temperature resulting from the admission of dry air into an exhausted receiver. When the receiver was exhausted so as to reduce the interior pressure to one-fourth of that of the atmosphere, and one-fourth was suddenly admitted, so as to reduce a gage from about 22½ inches to 15 inches, heat was produced; and however the ratio of the entering air to the residual portion was varied, still there was a similar result.

When the cavity of the receiver was supplied with the vapour of ether or with that of water, so as to form, according to the Daltonian hypothesis, a vacuum for the admitted air, still heat was produced by the latter, however small might be the quantity, or rapid the re-admission. When the receiver was exhausted, until the tension was less than that of aqueous vapour at the existing temperature, so as to cause the water to boil, as in the Cryophorus, or Leslie's experiment, still the entrance of $\frac{6}{1000}$ of the quantity requisite to fill the receiver caused the thermometer to rise a tenth of a degree. An alternate motion of the key of the cock, through one-fourth of a circle, within one-third of a second of time, was adequate to produce the change last mentioned.

Dr. Hare considered the fact, that heat is produced, when to air, rarefied to one-fourth of the atmospheric density, another fourth is added, irreconcileable with the idea, that this result arises from the compression of the portion of air previously occupying the cavity, since the entering air must be as much expanded as the residual portion is condensed.

As, agreeably to Dalton, a cavity occupied by a vapour acts as a vacuum to any air which may be introduced, Dr. Hare argued, that when a receiver, after being supplied with ether or water, is exhausted so as to remove all the air and leave nothing besides aqueous or ethereal vapour, the heat, acquired by air admitted, cannot be ascribed, consistently, to the condensation of the vapour.

The facts above stated, he added, are not reconcileable with the idea of De la Rive and Marcet, that the first portion of the entering air is productive of cold, although a subsequent condensation is productive of an opposite change. The effect upon the thermometer was too rapid, and the quantity of the entering air too minute, to allow it to be refrigerated by rarefaction in the first place, and yet afterwards to be so much condensed as to become warm by the evolution of caloric.

Notwithstanding the experiments of Gay Lussac and of those of De la Rive and Marcet, there appeared to Dr. Hare to be evidence in favour of the heat being due to the space, rather than to the air which it contained.

With respect to Gay Lussac's celebrated experiment with the Torricellian vacuum, supposing such a vacuum to be a pre-eminently good liberator of heat, as it ought in reason to be, the caloric would be absorbed by the mercury as rapidly as this metal could be made to encroach upon the space occupied by the calorific particles.

Admitting that, for equal weights, the specific heat of air is seven times as great as that of mercury, there could not have been a capacity greater than that of about 200 grains of the metal, whereas a very small stratum of this metal, equal to one-fourth of an inch, would, in the apparatus employed, amount to more than a pound.

The rapidity with which a mercurial thermometer is affected by the changes of temperature, in experiments like those which he had been describing, showed, in Dr. Hare's opinion, that there was something not yet understood respecting the transfer of heat in such cases. It was hardly reconcileable with the process of conduction or circulation, as ordinarily understood.

In the experiments of De la Rive and Marcet, in which the entering air being made to impinge upon the bulb of a thermometer, was productive of a fall in the thermometric column, it might be inferred, he conceived, that the bulb interfered with the access of caloric from the space. It was in fact the bulb upon which the air acted previously to its distribution in the space where it could have encountered the due proportion of caloric.