SCIENCE:

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THE TORNADO: ESPY'S EXPERIMENTS.

THESE investigations of Espy upon what actions may be considered as taking place in the upper air, as has been already said, are of the highest importance, and demand a special consideration. A proper interpretation of his results will help us in all our studies and reasonings. One of the most serious difficulties that we shall encounter, however, is in the fact that in the open air we are not dealing with a limited confined space, but we have to do with unlimited space and a well-nigh frictionless medium. The apparatus which he used has been sufficiently described already. The earlier investigation with the nephelescope was made without the use of a condensing syringe; and in this we are able to comprehend clearly just the action which took place, while in the later researches the results were quite complex. Mr. Espy first carried his jar into air the temperature of which was quite low, at freezing or below; and, after it had attained the temperature of its surroundings, the stop-cock was closed, and the jar was taken to a room with high temperature, 70° to 80°. The air inside was expanded by the heat, and the amount of this expansion was measured on the gauge. He then opened the stop cock, and closed it at the moment the mercury reached a level in the gauge. The rising of the mercury in the gauge after explosion, he thought was due to the gradual heating of the air which had been cooled by the sudden expansion. In the same way the jar was left in a high temperature for a time; then the stop-cock was closed, and it was carried to a low temperature. In this case, of course, the mercury in the gauge had the opposite motion to that it had before. The same experiments were tried with both moist and dry air.

One of the more important results from these researches, Espy does not seem to have thought of, though it dimly foreshadowed the epoch-making experiments of Mayer and Joule, in England, on the mechanical equivalent of heat. It is plain that in this confined space used by Espy there must be a relation between the amount of rise or fall in temperature and the corresponding change in air-pressure under a constant volume, and this will enable us to determine the expansion of air per degree of heat applied. In moist air the average difference of temperature between the cold and warm room was 45°, and the rise of the gauge 2.97 inches; in moist air, going from a warm to a cold room, the fall in temperature was 45°, and the fall in the gauge 2.60 inches: while with dry air these quantities were 57° and 3.34 inches and 63° and 3.30 inches respectively. The amount of change per degree in the four cases was .066, .057, .059, and .052 of an inch, or a mean of .059 of an inch in all the cases. The reading of the barometer is not given; but, if we assume it to be 29.80 inches, we find that the increase of temperature required to double the pressure in the jar was not far from 505°,—a result which is remarkably close to the 490° found by the more careful and extended researches of others. We now see the important bearing of this first work on our studies. If we compress air 5 inches by the gauge, we would heat it 83°; and if 10 inches, 167°; and so on, provided no heat was lost or dissipated in the operation. We ought also to be able to calculate from the reading of the gauge, after any operation of either condensation or expansion, exactly how much the air was heated or cooled, provided always that no heat was lost or gained.

In the light of this interpretation, let us examine some of Espy's experiments. In one instance he compressed the air 10 inches. We may suppose that he waited long enough to allow the jar to attain the air temperature before explosion. After the explosion the gauge reading was 2.15 inches, which would indicate that the cooling was not far from 36°, provided the only influence on the gauge was through the rise of pressure consequent upon the heating of the air by surroundings. It would appear that the theoretical cooling from such an expansion should be much greater than this; and, in fact, we cannot reason or theorize upon this result in any way, without first learning the probable loss of heat during compression, and its gain during expansion, to and from the environment. Espy's nephelescope was duplicated in its different parts, with the addition of a most delicate thermometer loaned by Professor Russell. This instrument has a bulb .06 of an inch in diameter and 1 inch in length, and would change one degree in from two to three seconds. It is plain, that, in observations with any degree of speed in *change* of temperature, any instrument will be liable to lag behind the existing temperature. This difficulty, however, may be partially avoided by conducting operations at variable rates of speed, because, if we obtain the same result with different rates, we can conclude that the lagging is inappreciable. Again: the amount of this lagging may be approximately computed by using two thermometers with different rates of lagging. Whatever may be this lagging, however, it is plain that it would be exactly the same in moist as in dry air, so that our comparative results will be entirely free from any error due to the instrument. In saturated air, moisture collects on the bulb; but this can make no difference, since in saturated air the dry and wet thermometers read alike.

Experiments.

On rapidly compressing the air and suddenly releasing it, the amount of rise and fall in temperature was the same, about 7° for 10 inches of compression. We shall be entirely within the limits of error if we assume the lagging to be 1°. Since the air at the side of the jar was at very nearly the air temperature, we can consider that the average heating and cooling of the whole air was not far from 4°. This shows what an enormous loss of heat was sustained by the air in compression to one-third its bulk. The theoretical heating should have been 163°, while the actual heating was one-fortieth of that. But the most important fact in this connection is that Espy, under the same conditions, found the cooling after expansion to be nine times that found above. Espy emphasizes the great necessity that exists in closing the stop-cock at the moment the mercury reaches a level, or at the exact moment when we may suppose the cooling by expansion is greatest. On repeating these experiments, it was a matter of great astonishment to find that the delay of a few seconds only in stopping the expansion, after the columns in the gauge were at the same level, almost entirely obliterated the subsequent rise. This would seem to show that heat from the environment had little or nothing to do with this rise, and this is an exact corroboration of the indication of the thermometer; for a rise of 4°, which was found, would represent less than .25 of an inch on the gauge. Does the mercury in the gauge reach a level before the air inside the jar is in equilibrium with the outside air? Professor Marvin has suggested that the momentum of the mercury in the latter part of the expansion would cause it to reach its level sooner than the air its equilibrium. On performing the experiment at different speeds of expansion, it was found that a definite relation existed between the rapidity of fall of the mercury and the subsequent rise after arresting the expansion. For example: the air each time was compressed to 400 millimetres, and expanded in 5, 10, and 20 seconds. The amount of rise in these cases was approximately 41, 21, and 12 millimetres respectively. It might be thought that this was due to the greater accession of heat to the air during the slower expansion (that is, the cooling would not become as great), but in all these cases the thermometer indicated the *same* cooling at the end. It must also be plain that this effectually disposes of the question of lagging, as suggested above. The evidence is cumulative and conclusive, that the rise noted by Espy was not due to a heating, from outside, of the air cooled by expansion; and his whole theory regarding the difference in cooling, of dry and moist air, falls to the ground.

As regards the fact that there is only an exceedingly slight rise after waiting a few seconds, Professor Seaman has suggested that the air may be heated in these few seconds, and therefore there is no rise. But it takes time to heat the air under these conditions, and in these few seconds the amount of heating is exceedingly slight: in fact, this one thing is a strong argument against the view that the rise noted after expansion is due to heat; for the rise is very rapid, and is accomplished in a few seconds. Still another point is to be noted: by making the compression to 400 millimetres very rapid (that is, in 10 seconds), and then expanding in 5, 10, or 20 seconds, we have heated and cooled our air by very nearly the same amount, and it has come back to just a degree or two below the outside air temperature; so that the subsequent rise cannot be due to the accession of heat from outside. There is an exceedingly interesting matter right here that I leave for physicists to consider. We are told that the only way in which air can lose its heat is by performing work. Now, is it possible for us to consider that the amount of work done in compressing the air to 400 millimetres is exactly counterbalanced by the work done by the air in forcing aside the outside air as it rushes out of the jar? It would seem as though the former must be a thousand or more times greater than the latter, if we take account of all the circumstances. One more experiment was tried to determine the cause of the rise in the gauge after explosion. If this rise were due to the behavior of the gauge, rather than to outside heat, we ought to be able to obtain it at any moment after expansion, and long before any marked cooling had taken place. When the expansion was arrested after one or two seconds, there was a marked rise in the gauge. This arrest must have been long before the cooling could possibly have brought the air to the outside temperature, for the just previous compression had heated it 4° above the air, and the expansion could not cool it down before 5 seconds had elapsed. Whatever may have been the cause of this rise, there is one point about which there is not the slightest doubt, and this is the principal point that we are to consider. Under all conditions of slow or rapid compression and expansion, the final cooling after explosion was almost identically the same, whether moist or dry air were used. This was determined by the thermometer; and in this experiment it must be admitted that the lagging of the thermometer had no influence, for it would be precisely the same in both moist and dry air. The rise of the gauge after explosion with dry air was slightly greater than with moist air, but this may have been due to a difference in the whirls which the explosion always produced inside the jar. It seems almost incredible that this fatal slip should have occurred at such an extremely critical point in Espy's work; and I am impressed with the conclusion thus reached, not so much by my interpretation of Espy's doubtful results, but far more because the two sets of experiments dovetail into each other so perfectly, and the one serves as a check on the other.

It is not a little remarkable that we have obtained somewhat the same result as this by another method of reasoning. The condensation of the moisture or the appearance of the cloud, even in thoroughly saturated air, is exceedingly evanescent in a jar of this kind. It is next to impossible, except with very high compression (15 to 20 inches), to get a particle of mist to settle to the bottom. Under the compressions employed by Espy, the cloud or mist entirely disappears in a few seconds, and not an atom of moisture reaches the bottom of the jar. Suppose, now, that, at the instant of expansion, latent heat were liberated by the formation of the cloud, which would prevent a further cooling. It is very plain that it would be used up immediately in the evaporation of the cloud; and the disappearance of the mist proves that the sensible heat has again become latent, and can have absolutely no effect in expanding the air or in causing a final higher gauge reading, as Espy thought.

A note should be made of the condition of Espy's moist air. The presence of a haze or cloud is no evidence of saturated air, for such cloud has been produced in air having only two per cent of moisture. When air is pumped in from the room, it has an enormous number of dust-particles in it, and these give the semblance of fog on sudden expansion Espy tried to saturate his air by putting a little water into his jar, but it is certain that this expedient would be of little or no effect. Bubbling air through three inches of water will not saturate it, and it was found that nearly all expedients failed to do so. The only satisfactory saturation could be effected by passing the air through a bottle full of small pieces of sponge saturated with water. While we cannot think that Espy's air was saturated, yet it is certain that the experiments in 1889 were with saturated air, and hence must have shown a difference between dry and moist air, if any could have done so. The delay of several days in some of Espy's experiments after compression before explosion should have served, and probably did serve, to increase the moisture in the air, and not to diminish it, as he thought, and as his researches seemed to indicate. It might be thought essential, in order that this question may be settled beyond all doubt, that there be some explanation of Espy's results showing a slightly greater rise in the gauge after expanding dry air than when moist or partly moist air was used. Undoubtedly, if all the conditions were known, the difference could be easily explained. It is absolutely certain that it was not due to any latent caloric of elasticity that was given out by the moist air.

I think this discussion will enable us to reason more or less effectively as to what are the probable heating and cooling effects in the free atmosphere from descending or ascending currents, and the resultant liberation or production of energy. It is well known that the most perfect locomotive makes use of only five to ten per cent of the total energy developed. We have just seen, that in condensing air to 10 inches, instead of obtaining an increase of temperature of 163°, as theory seems to indicate, we have barely reached 4°, or one-fortieth of the theoretical amount. It is plain that this is due to the loss of heat into the environment of the air. Suppose, now, we take away this confining jar; suppose we make steam in the open air instead of our locomotive boiler; or suppose, instead of trying to compress air in our jar, we had the total horse and steam power of the whole earth engaged in compressing the free air by forcing it through syringes or force-pumps into the atmosphere. What would be the result? The utter absurdity of all this is most plainly manifest, but is it any more so than the attempt at developing effective energy in the free air, as has been theoretically accomplished by some? If there is this enormous dissipation of heat under conditions which we can control, must there not be a very much greater dissipation of heat in all out-doors? Is it not highly probable that many of the theoretical deductions find their only shadow of support in the fact that the assumptions call for a perfect engine without loss of a particle of energy? Has theoretical meteorology ever produced even a single essential effective element or part of this perfect engine? If the considerations herein set forth are borne out by subsequent researches, we must most certainly come to the conclusion that thus far theoretical meteorology has not had a single well-supported fact on which to base its profoundest theories of tornado generation and movement. Professor Wild of St. Petersburg has well said, "Without exact and satisfactory data, meteorology cannot develop as a science, but will be, as heretofore, mainly a tumbling-ground for vague speculations and dilettanti investigations." H. A. HAZEN.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

The editor will be glad to publish any queries consonant with the character of the journal.

On request, twenty copies of the number containing his communication will be furnished free to any correspondent.

Ohio Meteorites.

UNDER date of June 13, Mr. George F. Kunz, in an article in *Science* upon meteorites, mentions two copper ear-rings found by myself in an Ohio mound as partly composed of meteoric iron. Mr. Kunz is in error as to the locality of the find. It was made at Frankfort, Ross County, O., and not in the neighborhood of Fort Ancient, as stated in the article.

The ear-rings are coated with a heavy plate of the iron, and are splendidly preserved, the iron having resisted atmospheric agencies remarkably well. It is slightly corroded in one place only.

The state of preservation is due to the placing of the objects in a layer of fine, dry gravel by the builders of the mound. The nearest skeleton was distant five feet, and the ear-rings did not accompany any remains. However, there were three copper hatchets placed alongside these ear-rings, and five other spools or ear-rings, too; but these latter were not covered with meteoric iron, or any other substance. The mound was examined in April, 1889. WARREN K. MOOREHEAD.

Xenia, O., June 18.

Vertical Components of Motion in Cyclones and Anticyclones.

IN saying that there is an ascending component of motion in cyclonic areas, and a descending component in anticyclones (*Science*, May 30), I meant that the winds in these areas of low and high pressure do not move horizontally, but obliquely upwards or downwards. The evidence of this has been presented and discussed by Loomis, in his "Contributions to Meteorology," in the *American Journal of Science;* and an abstract of these has been prepared by Clayton for the *American Meteorological Journal.* Hann and others have also discussed the matter. If Mr. Velschow, who makes inquiry on this point in *Science*, June 20, is not already acquainted with the writings of these authors, a reference to them would perhaps satisfy him. W. M. DAVIS.

Cambridge, Mass., June 21.

BOOK-REVIEWS.

Locke. By ALEXANDER CAMPBELL FRASER. Philadelphia, Lippincott. 16°. $1.25.

THIS the latest volume of Blackwood's "Philosophical Classics" is one of the best of the series. It opens with a quite full account of Locke's early life and education, with brief sketches of his family and the various persons with whom he came in contact in those years, and by whom he may be supposed to have been influenced. It then recounts his entrance into political life, and the