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