upon a medium, at an angle whose complement is equal to the angle of refraction, it becomes incapable of being reflected from a second surface at the same angle, provided the first reflection is operated horizontally, and the last vertically.

I content myself with having thus pointed out these remarkable coincidences; hereafter I shall consider the action which an interposed sheet of mica, or other crystallized body, has upon light thus electrified. With respect to the phenomena of diffraction, it equally supplies us with interesting results, and furnishes an explanation of the remarkable fact, that when diffraction is performed in homogeneous light, the fringes produced are alternately coloured and dark; coloured, when the differences of the length of their paths are as the numbers 2, 3, 4, 5—dark, when they are as the numbers $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$, &c.; a key to the seeming paradox, that two rays of light may be so arranged, that, instead of producing an increased intensity, they shall co-operate, and yet produce darkness.

These experiments likewise lead us to several facts, which, in very delicate researches, may be noticed, though they do not commonly fall under the range of our observation. We may deduce from them that, when a closed vessel, containing air, or other gaseous matter, is exposed to sunshine, the pneumatic equilibrium of it is deranged; it is condensed towards the side which is exposed to the light; if it be a compound, whose parts will not bear compression, without experiencing chemical change, it may so happen that the mere condensation produced by the incident light, is sufficient to produce that change. Hence, a mixture of chlorine and hydrogen gas, exposed to the light, unites with an explosion; and hence it is, that the chloride of hydro-carbon, mixed with chlorine gas, is converted into per chloride of carbon, and muriatic acid is evolved; for the contraction of the glass condenses the mixture, until it gives out sufficient heat to cause combination, 148° Fahr. being sufficient to produce that effect.

NOTES OF AN OBSERVER.

Remarks on Professor Olmsted's Theory of the Meteoric Phenomenon of November 12th, 1833, denominated Shooting Stars, with some Queries towards forming a just Theory. By JAMES P. ESPY.

(Continued from p. 92.)

Having thus undermined the theory of the learned Professor, it would be temerity in me, as but few new facts have been brought to light, to propose one of my own; indeed I have none to offer. I have, it is true, incidentally shown some reasons for supposing that these meteors and the aurora borealis have one common origin—but what theory of the aurora, which has ever yet been advanced, will bear the test of a close examination, any better than the Professor's, concerning the meteors? The magnetic influence has been strongly advocated as connected with the aurora, because many auroral arches

have appeared in Great Britain nearly perpendicular to the magnetic meridian, and the streamers parallel to the magnetic dip; but this notion seems to be refuted by the fact, that *some* arches, even in Great Britain, and nearly *all* in this country, have had a very different bearing.

I may, perhaps, without theorizing, be permitted to turn the attention of meteorologists to a few queries on this subject, which I think worthy of further investigation than I have yet been able to give them.

1st. Do auroral arches always move towards some southern point?

2d. Do the streamers always lean in the direction of the motion of the arch?

3d. Is there always a sudden depression or elevation of the dew-point, about the time of a brilliant aurora?

The first two of these queries may be answered in the affirmative, so far as the observations of others have come within my knowledge; and the latter also, so far as my own observations have extended.

4th. Are clouds, or haze, in our latitude, generally formed behind the arch?

If this last query should, by future observations, be answered in the affirmative, it will account for the fact, that the aurora is more frequently seen, here and in England, in the north than in the south; for as the auroral light is always seen above the cloud which is near it, those who are north of the aurora, will be prevented from seeing it by the clouds or haze. Many facts have come within my knowledge which favour this idea.

I have frequently seen clouds accompanied with auroral light, pass from the north to south, and from west to east, with the light always above and in advance of the cloud, so that when the cloud had passed the zenith twenty or thirty degrees, it seemed to hide the light from view. One of the most remarkable instances of this kind occurred on the 17th of May, 1833. About nine o'clock, a brilliant aurora appeared low in the north. A few minutes after nine, beautiful streamers (not in motion) appeared rising into a clear sky, from a dense light below, which seemed to rest on dark clouds near the horizon. I watched it till within a few minutes of ten, and then, while I was taking notes, a bright arch was completely formed; and when I looked out again at ten, it was almost exactly in the zenith, for its southern edge was very near Alpha Lyræ, and it reached entirely across the heavens, in a direction about twenty degrees south of east, and as many north of west. It moved pretty rapidly to the south, and disappeared at twenty minutes past ten, about eleven degrees south of Lyra. *During the whole time of my observation, clouds were constantly forming in the northern part of the heavens, so rapidly as to keep pace with the motion of the arch; all north of the arch being cloudy, and all south of the arch being clear; and when the arch disappeared, it did so as if it was by the intervention of the clouds. The arch was about three degrees broad in the zenith, and certainly not more than a quarter of a degree near the horizon, and in the zenith the light was much less dense—it was a little bent at both ends, and had there something of the appearance of a ribbon, with its edge a little turned*

towards the observer. The wind had been north in the morning, and changed round to the south-west below during the day; but there is reason to believe that an upper (not the uppermost) current was still coming from the north—the cloud certainly moved or *formed* in that direction, and I have frequently known a northern current above to continue to blow after the wind below had changed round even to the south. This arch was not seen at Wilmington, twenty-eight miles south-west of Philadelphia, though the aurora was.

Having mentioned the probability in this case, of an upper current of air from the north, I will propose here another query.

5th. Are there always two currents in different directions, at the time of aurora, and is the upper one generally from the north? Or where the upper and lower currents are in the same direction, from the north, does the upper move fastest?

There are several facts which lead me to suppose that this question may, by further observation, be answered in the affirmative. First, auroras generally commence low, and rise gradually to a higher elevation, which may as naturally be supposed to depend on a horizontal motion towards the south, as on a perpendicular motion upwards;—the same may be said of auroral arches, which most probably travel horizontally. Again, in many cases which have come under my own observation, these currents have been detected.

Besides, many meteorological phenomena can easily be accounted for, on the supposition of an upper current of air from the north, at the time of their occurrence, even when this current is not indicated by clouds.

I have known many instances, some when clouds were visible in the upper current from the north, and some when they were not visible, when the dew-point sunk to thirty-three and thirty-four degrees below the temperature of the air, and in one case to forty-one below. In most of these cases it was discovered by a sudden depression of temperature, (amounting, in one case, to 14° in an hour, with a change of wind to north, which occurred very soon after this great depression of dew-point,) that there had been a north current above, probably not many hundred yards from the surface of the earth, at the time of this depression.

The cause of these great depressions of the dew-point will readily appear, from what will be said hereafter of downward vortices, from the overlapping cold dry current. A small quantity of this cold dry air may, on descending, spread over a considerable surface of land, and thus coming in contact with warm ground, its temperature may soon be much increased, while its dew-point will continue low. It may be added, that the sudden fluctuations of the dew-point about these lines, favour this explanation.

Several phenomena also, which occurred on the thirteenth of November, and the day before, can be accounted for on the supposition that there was a cold north-west current above at that time. The dew point at one o'clock on the twelfth, was 57° Fahr., and at six in the evening, it was 40° . It rained a little during the day, the wind being south-west, but it was perfectly clear in the evening. Having seen many instances of so sudden a change, particularly a

depression of the dew-point, being succeeded by an aurora, I looked out for one that evening, but neither aurora nor shooting stars were visible as late as ten o'clock. There were no clouds in the upper stratum of air visible on the twelfth, but on the eleventh, the lower air was from the east, and the upper from the west, with rain, the dew-point being 37° . These fluctuations being so great and so sudden, I was not a little disappointed at finding not even the slightest appearance of an aurora.

Again, on the supposition of a cold current above, it is very easy to account for the sudden and violent gusts which occurred on the twelfth, and the night of the thirteenth of November. On the twelfth there was a gust in the Hudson, which upset a vessel, in which all hands were lost. On the morning of the thirteenth, there were several gusts of wind at New Haven, attended with an increase of meteors; and about eighty miles below Philadelphia, on the morning of the twelfth, about seven o'clock, so violent a wind blew directly from a cloud unusually round and well-defined, quite low, and apparently not more than *two hundred yards long*, that the Captain of the vessel was afraid the ship would drag her anchors. From this cloud, as was mentioned before, darted streaks of light similar to the aurora. Captain Parker also, in the Gulf of Mexico, had the wind blowing directly from the cloud in the north-east, round which the meteors were playing. It was stated before, that the equilibrium of the atmosphere in its ordinary state, is stable, because the upper strata of air certainly contains as much caloric as the lower, pound for pound—and probably a little more; but if we suppose a very cold current from the north-west to overlap a warm current from some southern point, to a great distance, or if we suppose the upper current from the north, to move much faster than the lower to the south, we may easily perceive that in this extreme case, the equilibrium becomes unstable, and many vortices may be formed, (of no great breadth,) of the cold air coming down from above, and of course making a violent current at the surface of the earth, in all directions outwards from the centre of the vortex, and consequently from the cloud, if one should be formed there. That a cloud should be formed there, in many instances, is highly probable; because the vapour surrounding the vortex would be mixed with the cold air of the vortex, and if there should be more than enough to saturate the compound, it would be condensed into a cloud. I acknowledge that in the case of the cloud seen down the Delaware, I have no proof that, to the west of that cloud, the wind blew from the east; but it is very certain that that cloud was very near to the observer, otherwise it would have been seen at Philadelphia; but it was not; neither was there any strong wind at Philadelphia at that time, nor so late as half-past ten o'clock. After that period I cannot speak from my own knowledge.

Now, if we suppose an upper current of very cold air, from the north-west or west, overlapping one of considerable warmth at that time, (and at Philadelphia the air at the surface of the earth, on the twelfth was actually seventeen degrees warmer than on the eleventh, and twenty-one degrees warmer than on the thirteenth,) it will most

satisfactorily account for these several phenomena—the sudden fall of the dew-point seventeen degrees on the afternoon of the twelfth—its fall of sixteen degrees more on the night of the twelfth—the sudden blasts which occurred at that time, caused by the downward vortices—the formation of a cloud in some instances, at the borders of a vortex, where the vapour in the surrounding atmosphere was sufficient in quantity to supersaturate the cold descending air, and the wind blowing from the vortex must necessarily blow from the cloud—the great change of temperature which occurred just before and just after—and, as the meteors have been demonstrated to be of atmospheric origin, and not gravitating bodies, perhaps I may be permitted to add, that the supposition of an upper very cold current, then existing, from the west or north-west, from which, in various places, vortices of small diameter might descend, will not only account for the gusts of wind observed by Mr Palmer, but also throw some faint light on the fact mentioned by him, that immediately after these blasts, the *number of meteors was increased*—for if the fifth query should, by further observation, be answered in the affirmative, it will then be rendered highly probable that the aurora is electrical, and depends on the overlapping of two currents of air of different electrical tensions, and consequently any thing which might bring these two currents into nearer proximity, such as the vortices spoken of above, would probably increase the development of electricity.

Nor is this probability very faint, when it is considered that almost all the clouds we hear of that night, were accompanied with a brilliant display of meteors, or auroral light, and some of them were highly luminous, as if the particles of condensed vapour of which they were composed, formed an interrupted conductor of electricity from one stratum to another, causing the electricity to appear in passing from one particle to another. And even when a cloud was not formed by one of these descending vortices, the vapour in the surrounding air not being enough to supersaturate it, yet the quantity of vapour in it must have increased by the proximity of the surrounding air of a higher dew-point, and thus its power of discharging its electricity would be increased. It need hardly be mentioned, that moist air is a better conductor than dry air.

I am well aware that these hypothetical speculations have but little weight with the true inductive philosopher. He inquires, is it a *fact* that there was an upper current on the night of the twelfth of November, from the north-west, overlapping a warmer current below—and by warmer here I mean containing more caloric to the pound—and has there never been an upper current of cold air overlapping an under one of warm air, since the Bonpland meteors of 1799?

I answer, I do not expect him to place more confidence in these speculations than I do myself, indeed not so much; for I have seen so many instances of the dew-point falling in consequence of an upper current from some northern point, of which many were accompanied with an aurora here, and many others with an aurora elsewhere, that

I confess I incline to the opinion that these two things are connected together as cause and effect.

But as to others, all I hope or expect by these speculations is to excite interest enough to induce them to observe *whether these things are so*. For this purpose, I will mention a few of the many cases in which I have observed the dew-point to fall immediately before and after the occurrence of an aurora. On the 8th of March, 1834, at half-past five o'clock in the afternoon, the dew-point was $59\frac{1}{2}^{\circ}$ Fahr., and at six o'clock, a shower having intervened, it had sunk fourteen degrees, and was then clear.

This was one of the most sudden falls I had ever seen, and I looked out with considerable anxiety for an aurora, and was a good deal disappointed at not being able, during the whole evening, to detect even the least appearance of one. Next morning, at sunrise, the dew-point was 15° above zero, having sunk $44\frac{3}{4}^{\circ}$ in $18\frac{1}{2}$ hours. The circumstance of no aurora being seen here, when there had been so remarkable a fall of the dew-point, shook my faith in the hypothesis; but it was strengthened again by discovering that the tail of Mr. Olmsted's comet had been seen on that night with uncommon brilliancy, by the Professor himself, at New Haven. He says: "This evening, the sky having been thoroughly cleared by a copious shower of rain, the luminous cone, or lens, reached nearly to the pleiades, through about 60° of long., and was visible till near nine o'clock." Again, from the afternoon of the 29th of March, 1834, to the morning of the 30th, the dew-point sunk from $47\frac{1}{2}$ to $9\frac{1}{2}$, and though it was clear in Philadelphia on both these nights, and the aurora anxiously expected, none occurred.

But here again the comet, or zodiacal light, of Mr. Olmsted, comes to our aid, and will not allow us yet to abandon our hypothesis. For he says: "On the 30th, it reached in a direct line from the sun between the pleiades on the west, and Aldebaran on the east, having its apex a little west of the latter, and making an angle with the ecliptic of seven and a half degrees, which is the inclination of the sun's equator, as has been observed heretofore respecting the zodiacal light."

Now, it is not a little remarkable, that immediately after this repeated appearance at New Haven, it begins to appear at Philadelphia, and as the Professor will be gratified to learn that the tail of his comet was seen beyond the neighbourhood of New Haven, I will for his gratification, as well as to show the existence of various currents, and rapid fluctuations of the dew-point, about that time, quote, verbatim, the words of my journal written at the time.

"On the 31st of March, during the whole day, there was a strong wind from the south-west. Notwithstanding the temperature did not rise above forty degrees, and the dew-point at one o'clock, was thirteen, having fallen from sixteen during the morning, the upper clouds were coming from the south-west as usual, the middle from the west all day, and in the afternoon at five o'clock, a lower stratum of clouds came up from the northern horizon, hiding from view in the north, the upper clouds of both strata, while, at the same time, the wind at the surface of the earth increased in strength from the

south-west. This lower current continued to blow till some time between eight and twelve on the second of April, when it changed to the north. In the mean time, the dew-point had gradually risen until the morning of the second, at eight o'clock, it was $56\frac{1}{2}^{\circ}$, some rain having fallen both on the first and second. All this time the middle current of clouds came from the west. On the afternoon of the second it began to fall, and in the evening, as soon as it was dark, an aurora was visible in the north-west, reaching from Orion's belt nearly to the northern horizon, at first not rising so high as the pleiades; but about nine o'clock, the pleiades were in the brightest part. At the same time there was a very faint aurora nearly in the east, its central part reaching as high as *Spica Virginis*."

The next morning, third of April, the dew-point had fallen to 24° , that is, 32° in twenty-four hours. During all this day, the air below came from the north, the middle from the west, and the upper from the south. As soon as dark, this evening also, the aurora appeared in the north-west, occupying the same place as on the preceding evening. It, however, rose higher in the latter part of the evening, and a few minutes before nine o'clock, it assumed exactly the appearance of the aurora described by Professor Bache, in the *Journal of the Franklin Institute*, vol. xiii., p. 1, with this only exception, that the pillars were not so straight and smooth, and they rested on a pretty dark cloud, which sloped downwards towards the horizon, which it reached exactly under Orion's belt. This cloud was much narrower at its northern end, where it was about 15° high, and under it was a dull auroral light, down to the very horizon. These pillars continued to rise nearly at right angles to a pretty strong north wind.

On the next day, the fourth, the wind was north-east, no change having taken place in the upper currents, and on the fifth, the wind continued very strong from the north-east, the upper current being from the south-west, and the middle veering from the west to the south. This night also, the aurora was seen between ten and eleven in the north-west; and the dew-point, although it had risen the preceding evening with the upper south wind, was down again on the morning of the sixth, to 15° .

I have not yet been informed whether these auroras were seen at New Haven, but Dr. Gibbons, of Wilmington, Delaware, who has watched the aurora very minutely for several years, says, in his *Meteorological Journal*, published in the *Advocate of Science*, that the aurora was not seen on any of these nights, nor indeed on any night of March, April, May or June. The inference here again appears to me inevitable, that these auroras could not be three or even two miles high;—how much lower we know not. Indeed, I find, by comparing Dr. Gibbons' *Journal* with mine, that of fifteen auroras which were seen at Wilmington, from the 10th of July, 1831, till the 15th of December, 1833, only two were seen at Philadelphia. In all these cases there is marked in my journal a sudden and unusually great change in the dew-point: twelve of them falls, and three rises. Of the three auroras corresponding to the three rises, one was the 17th of May, 1833, which was seen at Philadelphia, as a beautiful arch passing the zenith, at ten o'clock.

The dates of all these auroras should be mentioned here, but this paper has already been extended beyond the limits originally intended; and I must content myself with mentioning a few dates when there was a remarkable change of the dew-point here, but no aurora, in the hope that some persons who keep a meteorological journal to the north of this, may have the curiosity to examine whether an aurora occurred at any of those times or not, and publish the result of their examination, whether it shall be in the negative or affirmative. At all events, the comparison of these accounts, if published, will be highly interesting, and cannot fail to aid in the formation of a just theory in explanation of this meteor.

In 1832. January 26th; February 21st and 24th; March 14th, 18th and 27th; April 20th or 21st; May 21st and 27th; August 25th; September 5th and 23d; October 16th, 24th and 26th; November 14th and 20th; December 19th or 20th.

In 1833. January 11th or 12th, 16th and 26th; February 7th and 25th; March 2d; July 31st; August 8th; October 3d and 11th.

In 1834. January 3d or 4th, 13th and 21st; February 12th and 26th; March 3d, 8th, 21st or 22d, and 30th; April 11th and 27th; June 2d or 3d, 11th or 12th, and 27th; July 11th; November 16th.

At all these times there was a remarkable depression of the dew-point; but I have not had it in my power to ascertain whether the aurora was seen or not at distant places on these nights;—it was not seen at Philadelphia. It may be mentioned too, that in case the aurora was seen about these times, it was probably seen a little before them, in places north of Philadelphia; for I observe that when the aurora was seen about the same time at New Haven and at Philadelphia, it occurred a day or two sooner at New Haven than at Philadelphia, and that in like manner, it occurred sooner in one instance at Philadelphia than at Wilmington, Del.

This probability is increased from the fact, that the aurora, and especially auroral arches, are known generally to travel towards the south.

As there are no facts in opposition to the suggestions here made, and some, though not enough to found a theory upon, in favour of them, I am sure that even the rigid inductive philosopher will excuse me for throwing out these hints as worthy of further investigation.

(*To be continued.*)

Remarks upon the Employment of Pressure Engines, as a substitute for Water Wheels. By DANIEL LIVERMORE, Civil Engineer.

FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

The committee of the Institute have, by their accurate and valuable experiments, afforded the most satisfactory evidence that, under peculiarly favourable circumstances, water wheels may be so constructed and used as to ensure an *effective* application of three-fourths of the power applied for their propulsion; and from this, some me-