Observations of the Solar Eclipse of May 15th, 1836. Communicated by direction of the American Philosophical Society.

TO THE COMMITTEE ON PUBLICATIONS.

GENTLEMEN:—By direction of the Amer. Philos. Society, I send you an abstract of the observations of the late solar eclipse, which have been communicated to the Society. It is not complete, some particulars being required which will probably be obtained from the observers, by the committee to whom the observations have been referred, to prepare them for publication in the Society's transactions.

The times of beginning and end are in mean time of the places of observation.

Very respectfully yours,
A. D. BACHE,
One of the Secretaries, Amer. Philos. Soc.

Table of observations of the beginning and end of the solar eclipse of May 15th, 1836, as observed at Philadelphia, &c.

Places of Observation.	Time of beginning of Eclipse.	Time of end of Eclipse.	Observers' Name.	REMARKS.
Philadelphia.	h. m. s.	h. m. s.		
1. Hall of Am. Phil.Soc	7 3 45.8	9 32 38.3	Dr. R.M. Patterson,	
2. Dr. McEuen's house,	7 3 38.0	9 32 38.1	Dr. T. McEuen,	2770 feet west of Philos. Hall.
3. "	7 3 50.0	9 32 26.5	Mr. W.H. C. Riggs,	
4. No. 100 S. 8th st.	7 3 40.9	9 32 44.1	Mr. S. C. Walker,	1340 feet west of
5. No. 231 Market st.	7 3 41.0	†9 32 34.0	Mr. Sellers,	Philos. Hall. Teles. 42 inches achrom.
Haverford school,*	7 3 24.5	9 31 47.0	Mr. Jno. Gummere,	Telescope 42 inch. achrom.
Germantown, Pa.	7 3 54.5	9 32 44.5	Mr. Isa. Lukens,	
	7 3 55.5	9 32 49.5	Mr. C. Wistar,	Telescope 3 feet achrom.
Phoenixville, Chest.co.‡	7 3 12.0		Mr. H. Wilson,	
West Hills, L. I.§	7 12 48.5	9 43 40.0	Mr. J. Ferguson,	Large repeating circle of coast survey, used for observations.
Washington City,	6 53 58.0	9 20 08.0	Mr. F. R. Hassler,	

* Assumed lat. 40° 01′ 12″. Long. 5h. 01m. 25s. † Doubtful.
‡ Assumed lat. 40° 08′ 07.″ Lon. deduced by H. Wilson, from eclipse, 5h. 01m. 57s.
§ One of the station points of the coast survey. Lat. 40° 48′ 49.2″. Long. 73° 26′ 12″.

Essays on Meteorology. By JAMES P. ESPY, Mem. Am. Philos. Soc., &c.

No. III.

Examination of Hutton's, Redfield's and Olmsted's Theories.

In the preceding essays many facts independent of theory, have been adduced to prove an upward motion of the air in the region of a cloud, and many more will be adduced hereafter.

At present I propose to examine one of the many phenomena which I hink can be explained by this upward motion only; I mean the great quantities of rain and hail which sometimes fall in a very short time. It was demonstrated in the first part of this essay, that the velocity of the upward vortex, in very favorable circumstances, is 4.5 miles a minute. In rising through that height it will precipitate a little more than one half its vapour, which will be about five inches of rain, so that in this case forty inches would be precipitated in eight minutes, provided it were all to fall in one place, which from the nature of the vortex, can seldom occur. That which is condensed above the point of perpetual congelation, should not be taken into the account; because at the moment of its condensation it becomes snow, and being so light will remain in the atmosphere long after the hail and the rain caused by the melting of the finer masses of hail, in passing down through the lower parts of the atmosphere, have reached the earth. Still there will be enough left to account for the most violent rains or hails, of which we have any account.

Here it may be worth while to turn aside for a moment, to examine the efficiency of the most plausible theory of rain that has ever been given to the world. I mean that of Dr. Hutton. He supposes two currents of air of different temperatures, both nearly saturated with vapour, to be mingled together, and that a precipitation of course takes place, in accordance with the known fact, that at their mean temperature all their vapour cannot be retained, and therefore the surplus will be precipitated. This theory is defective in two respects: First, it does not show how two currents of air could be mingled to any considerable extent; and second, it does not show by calculation, that rain to any considerable amount, would be produced, even if large masses of air at very different temperatures, should be mingled together, which it would be easy to show never can happen, especially in the torrid zone. It may fairly be presumed that no advocate of the Huttonian theory, would suppose that more than 500 feet of a stratum of cold air could be mingled with a stratum of warm air, 500 feet of perpendicular height. Now it will be found by calculation, that if one of these strata is at 60°, and the other at 40° and both saturated previous to their mixture, the whole amount of precipitation, provided they took the mean temperature of 50°, would be less than a grain and one half on each square inch of surface. But as the latent caloric evolved in the condensation of the vapour, would not suffer the mean temperature of the two strata, when mixed to be acquired, but some temperature above 50°, therefore a less quantity than that mentioned, would be precipitated. Such a quantity in most cases, would be entirely evaporated in passing down through the air below, and never reach the earth.

It was mentioned before that 5.1 inches of rain fell in Wilmington, on the 29th of July, 1834, in two and a half hours; let us see whether such a rain could be produced at all, on the Huttonian principles, making the most extravagant allowance for the quantity of air mingled, and also for the difference of temperature of the two strata.

Let us suppose then that one half of the atmosphere at 80° Fah., should be mingled with the other half at zero, over the region round Wilmington, and that 5.1 inches of rain is the result. What will be the temperature of the mingled mass after the rain? The mean temperature is 40°, which would be the temperature after the mixture, if no latent coloric is given out in the condensation of vapour. But from the principles explained before, it will be found, that as five inches of rain, is $\frac{5}{400}$ of the whole atmosphere in weight, the latent caloric given out in the condensation of the vapour forming this rain, will be sufficient to heat the whole compound 59.7°, which being added to the mean temperature 40°, will make the temperature of the air after the rain 99.7°, almost 20° hotter than the hottest half of the atmosphere before the mixture.

This result, however unexpected, ought not to appear surprising. For if gentlemen will frame theories on loose principles, without once putting these principles to the test of calculation, and without even taking the least notice of the latent heat of vapour, or the specific heat of air, they ought not to be surprised that a little plain arithmetic should dissipate their empty visions and "leave not a wreck behind."

Theorists will pardon me for this sweeping denunciation, when I now voluntarily come forward and plead guilty to the same charge; for I too have framed a hypothesis to account for rain, and advanced it under the high sounding name of theory.

Having found that the Huttonian theory would not bear the test of calculation, I imagined there was but one other possible mode of condensing vapour and that was that the vapour by its own elasticity in the lower parts of the atmosphere, thrust itself up into a cold stratum above, when ever such a one overlapped the one below, and was thus condensed into rain.

This hypothesis I thought was altogether reasonable from the great discovery of Dalton aud Gay Lussac, that vapour in the atmosphere rests only on vapour, and thus forms an independent atmosphere, and is not supported in the least degree by the air. I imagined then, that vapour could rush with great velocity from air where the due point was high, to air where the dew point was low. But when I discovered that some rains were so great as to be beyond the power of this theory too, I began to suspect the hypothesis itself, which induced me to put it to the following trial.

I united two glass retorts together by their necks, then having covered one with snow, I put ten drops of water into the other and placed it in a vessel of water at the temperature of 130°, and let it remain in that situation seven hours, the temperature of the room during the experiment being about 70°; not one drop was distilled over in all that time.

I then took the retorts apart, leaving open the neck of the one having the water in it; it has continued in the room, open now for thirty days, with a temperature of 70° night and day, and the dew point in the room never as high as 40°, the ten drops of water being now only slightly diminished.

This refutes the hypothesis of rapid permeation of air by vapour, and indeed, proves that vapour, like heat, when it passes up to the upper regions, must be carried by the air, and not thrust up by its own elasticity. But to return from this digression; if the Huttonian theory is unable to produce such a rain as that at Wilmington, what will it do with the one which occurred at Geneva, on the 25th October, 1822, when it rained thirty inches in twenty-four hours; or the one at Jeyeuse, on the 9th October, 1827, when it rained thirty-one inches in twenty-two hours?*

Or how will it account for a storm of hail† which fell in Orkney on the 24th of July, 1818, in the afternoon, nine inches deep in less than nine minutes? And here it may be remarked that this is the storm mentioned before, in which the barometer was observed to fall two inches, near the end of the storm, when it was not nearly so violent as it was in other places. Or how will it account for the immense quantity of rain which fell at Catskill, New York, on the 26th July, 1819?

* Edinburgh Trans. 1823. † Pouillet Eliesmen de Phisique, II. 758.

About half past 5 P. M. a dense black cloud rose up from the S. W. accompanied with a fresh wind, and about the same time, or a little after, a very thick dark cloud rose up rapidly from the N. E. They met immediately over the town; at this instant a powerful rain commenced.

As soon as the clouds met they seemed to fall down on the river over which they met, and then the cloud rested on the water in such a manner that no space could be seen between them. For half an hour there was no appearance of drops of rain, the water appeared to descend in large streams and sheets. In this half hour the quantity fallen was above twelve inches on a level. Two persons testify that some time after the clouds met, they saw at the same moment a water spout rising up from the river nearly opposite, with a broad bottom ascending to the clouds with a whirling motion, in the form of a pretty regular cone.

The whole quantity which fell was more than fifteen inches, over a space of about eighty square miles; and as far as I can collect from the whole account which is given at large in Silliman's Journal, vol. 4, p. 124, this spout was stationary.

The intelligent author of the account, Benj. W. Dwight, says, it is worthy of remark that eleven days before in the P. M., there fell in a shower of short continuance, more than six inches of rain.

This theory has lately been brought forward and extended by Professor Olmsted of Yale College, with a view of accounting more particularly for hail, than the original author of the theory had done. And though I am aware that the strength of my theory does not depend on the weakness of any other, I think it proper to give the Professor's remarks a passing notice.

"We assign," says Mr. Olmsted, "as the cause of hail storms, the congelation of watery vapour of a body of warm and humid air, by its suddenly mixing with an exceedingly cold wind, in the higher regions of the atmosphere. Let us examine, says he, the effects which would result from the meeting of two opposite winds, at the height of 10,000 feet, during the heat of summer, the one blowing from the latitude of 30°, or from the confines of the torrid zone, and the other from the latitude of 50°, or the northern part of British America. If they had equal velocities, they would meet at the parallel of 40°; and, as in the case of the Gulf stream, a fluid does not readily change its temperature, merely by flowing through a body of the same fluid of a different temperature, and especially air through air, each current would retain nearly its original temperature.

The southerly wind blowing from a point which is still 2,000 feet below the line of perpetual congelation, is comparatively warm, while the northern coming from a point 4,000 feet above the same boundary of the empire of frost, will have a degree of cold, probably surpassing any with which we are acquainted. We infer from our preliminary principles, that immediately on meeting, the watery vapour of the warmer would be frozen with an intensity corresponding to the temperature of the colder current; that the minute hail stones thus formed and endued with such excessive cold, would begin to descend, and accumulate to a size proportionate to the intensity of the cold of the nucleus, and to the space through which they descended, and to the humidity of the lower strata of the atmosphere; that is, the colder they were when they began to fall, the farther they fell, and the more humid the air, the larger they would become."

As Professor Olmsted has not shown how these currents could be generated, the theory is plainly incomplete on this ground. And besides, even if they should be generated, it does not appear how they could be mixed; for either they would meet each other in opposite directions, and so stop each other's motion without mixing to any great extent, or they would slip by one another without much affecting each other's temperature, according to the Professor's own reasoning.

But even if it could be shown that a mixture of two currents could take place suddenly, of even 1,000 feet in perpendicular extent, it has been proved already that under much more favorable circumstances, the dew point being higher, a grain and a half of rain to the square inch would not be precipitated, and that in most cases not a particle of this would reach the ground, for it would be evaporated in its descent, unless the air below should happen to be absolutely saturated with vapour, which seldom occurs.

But, according to Mr. Olmsted, "the minute hail stones being indued with a cold probably surpassing any with which we are acquainted, would begin to descend and accumulate to a size proportioned to the intensity of the cold of the original nucleus."

This remark is erroneous in two respects. First, the cold is certainly not more intense at this great elevation, than one degree for every 100 yards, and is therefore in the northern current only $13\frac{1}{3}°$ below the freezing point; for by supposition it was only 1333 yards above the line of perpetual congelation, when it left latitude 50°.

Second, the original nucleus would not accumulate in the manner described; but on the contrary it would be entirely melted by the time it had descended far enough into the air below the line of perpetual congelation, to have condensed vapour less than one-seventh of its weight. This will easily be perceived by comparing the relative latent heats of vapour and of water, and this too even if it received no heat from the warm air into which it fell. But even if the original nucleus were of the temperature of the interplanetary spaces, 57° or 58° below zero, it would not increase one-fifth in size by condensing on itself the vapour, before it would be entirely melted by the disengaged latent caloric.

Professor Olmsted concludes his essay by saying that the momentum of a hail stone would be one hundred times greater if it did not at every stage of its progress down to the very ground, receive new accessions of watery vapour, which being matter at rest, is to be put in motion by the falling body, and consequently its speed is continually retarded.* But he must now perceive, from what has already been said, that the velocity of descent will not be diminished one-fifth, even when the stone has received an addition of vapour great enough to melt it.

Before I take leave of this extension of Hutton's theory, I must take notice of another remark made by Professor Olmsted, which if correct, would of itself prove fatal to the theory which I have advanced. He says, "we have certain evidence from the concourse of opposite winds, and from the density and consequent blackness of the clouds, that a great condensation takes place in the region of the storm."

Now it appears to me that it would be much easier to account for the concourse of winds, by supposing a rarefaction in the region of the storm, just as the rarefaction in a chimney is the cause of the air in the room moving towards the fire place. It shall be shown hereafter what effect would be produced by a condensation in the region of a storm.

I come now to a most important part of this investigation, the northeast

* Vide Silliman's Journal, vol. 18, p. 1.

storms of the Atlantic states. It is well known since the days of Franklin, that these storms commence in the south west and travel towards the north east with a velocity which varies at different times and places, and that the wind always blows from some eastern point at the commencement of the storm.

Mr. Redfield of New York has collected a great many highly interesting facts connected with these storms, of which some of the most important shall now be detailed.

When a storm commences within the torrid zone it travels west of north until it reaches lat. 30°, when it has become nearly north, it then gradually deflects more and more east of north, until about lat. 40°, it is moving about N. E. That these storms are probably nearly round, varying in diameter, and more slow in their advance along the coast, in proportion to their size, and also slower in low latitudes than in high. That on their north western side, the wind sets in more northerly and changes round during the storm by north, and on the south east side of the storm the wind sets in at the commencement more easterly and south easterly, and changes round by the south.

Mr. Redfield thinks that these facts can only be accounted for on the supposition, that these storms are exhibited in the form of great whirlwinds.

As a more particular proof of this position, he details the facts which occurred in Connecticut, as one of these storms passed there in 1821. He says, "that the mass of atmosphere upon the earth's surface was moving for several hours, apparently towards the N. W. over Middletown, with a probable velocity of seventy-five or one hundred miles per hour, while in the northern parts of Litchfield county, at a distance say of forty miles, the wind, at about the same period, was blowing with nearly equal violence in the opposite direction towards Middletown." Now it will appear by a little reflection, that all these facts agree with the idea of an upward vortex, more consistently than with a horizontal whirlwind.

Indeed I do not hesitate to say, that the last fact is inconsistent with a horizontal whirlwind, and proves with irresistible evidence, the existence of an upward vortex, at least in this storm. For two winds cannot blow towards each other for several hours as here described, without either rising upwards when they meet, or blowing outwards at the sides. But we have proof positive, that they did not blow outwards at the sides, for at N. York, S. W. of the point between Middletown and Litchfield, to which the winds from those places were blowing, the wind changed round by the N. to the N. W., or W. about the time these winds began to blow violently. And we have strong reason to believe that it did not blow outwards to the N. E.; for at the commencement of the storm, through its whole course, the wind always blew from some eastern point.

There is one conclusion which Mr. Redfield draws, which I do not find to be justified by the facts detailed in this storm. "That along the central portion of the track, the storm was violent from the south eastern quarter, *changing suddenly to an opposite direction.*" Now I find, that of fifteen points on the south east side of the storm, at which the wind set in S. of E. only two, Bridgeport, Conn. and one at sea, forty miles north of Cape Henry, are given, as having the wind to change round, even as far as the west. These two, I *suspected* as being contrary to my theory; and upon examination of the newspapers of the day, I find that they report the wind at both these places to have changed round only to the S. W., just as far as it *should* change to satisfy my theory.

All these facts lead to the conclusion, that in this storm, at least the wind in the neighborhood of the storm, blew directly towards its centre, and if so, it follows beyond all doubt, that there was an upward vortex in the middle of the storm. Now as it is impossible to conceive of an upward vortex being formed in the region of the storm, if there is a condensation of air there; so it can only continue on the supposition that the air, as fast as it arrives in the vortex from all sides, becomes rarefied, whatever may be the cause of that rarefaction.

As it has been said that a condensation in the region of the storm would cause an afflux of air there, let us for a moment examine the assertion. Suppose that no latent caloric is given out in the condensation of vapour, and that in a circular space of one hundred miles in diameter, five inches of rain have fallen, the whole condensation which would take place by the change of vapour to water, would be less than a fiftieth of the whole atmosphere, and the air on all sides of the storm, would not have to move one mile towards the centre, before the equlibrium would be restored. Besides it is manifest that this motion could not take place at the surface of the earth, but rather in the region of the cloud and above it. And even if the velocity at the surface of the earth is supposed to be as great as in the region of the cloud, it could not be a mile an hour, for it never has been known to rain five inches an hour in a storm of this magnitude, and the condensation of the air is supposed to take place during the whole rain.

I have myself had the pleasure of seeing and pointing out to many of my friends at various times, particularly to Professor Bache, the clouds moving outwards above, and inwards below, during a summer's thunder gust, which could not be, if there was a condensation of air in the region of the cloud, and I may add without the fear of contradiction, that it proves the reverse. Besides, I have known many instances of long continued and violent rains in the south, during the prevalence of a strong and long continued north wind, and of long continued and violent rains in the north, during the strong and long continued south wind.

An instance of the latter occurred on the 11th, 12th, 13th, 14th, and 15th of May, 1833. In my journal it is stated that a strong south wind prevailed during this whole period night and day. And by consulting the papers of the period, I find the following facts:

Harrisburg, May 16, 1833. When our paper went to press the Susquehanna was sixteen feet above low water mark, and rising—a greater freshet than has taken place for sixteen years—the rain must have been much greater up the river than in the vicinity.

Albany, 15*th*. The most painful accounts begin to be received of the destructive effects of the freshet. The river continued to rise until 10 o'clock this morning, when it was a foot higher than it was in the great freshet occasioned by the ice in the spring. On the 17th, it had fallen only a few inches.

The *Amsterdam* (Mohawk Herald) of the 16th, says, "every bridge and mill dam on the creek near Fort Johnson has been swept away."

Hartford, 18*th*. The water in the Connecticut last evening, was 19½ feet above low water mark.

Montreal, May 15*th*. A larger quantity of rain has fallen here since midnight of last Friday, (five days) than we have had for a considerable period past, and the rain is now falling in torrents, the atmosphere cool and very unpleasant.

The *Goshen* Patriot, says the Delaware rose twelve feet above an ordinary freshet—not a raft above Milford was preserved entire.

These facts afford conclusive evidence that in this case at least, the wind at Philadelphia blew hard for five days, exactly towards one of the greatest rains which our country has ever witnessed. And the statement, that the atmosphere at *Montreal* was cool and very unpleasant, would lead us to suppose that the wind there was coming from some northern quarter; for during this whole period the temperature was very high in Philadelphia, the mean minimum being 65°, and the mean maximum 76°, and if a southern wind prevailed there, it is not at all likely that the air would have been cool and unpleasant.

Again, from the 3d of June, 1835, to the 12th of the same month, the wind was constantly from the north, with one exception from north east, pretty strong for a considerable portion of time.

I find by the *Charleston* Courier, that a dreadful storm of rain set in there on the 3d, and another very violent one on the 8th, which was increasing when the paper went to press on the 9th at 10 P. M., and that on that day there had been no mail from Fayetteville, and that there were six letter mails due from N. Y. and Boston, and five from Washington, Baltimore, and Philadelphia.

All these facts seem utterly at variance with a horizontal whirlwind; and entirely consistent with an upward vortex, if they do not absolutely prove one.

If Mr. Redfield should perceive that all the interesting facts which he has with such laudable industry collected, are fully explained by a theory which accounts also for the rain, I am sure he will not be very tenacious of his horizontal whirlwind; especially when he does not pretend to show that either the whirlwind is the cause of the rain, or the rain the cause of the whirlwind. Let us, however, examine for a moment (for I should be proud to enlist Mr. Redfield under the banners of a true theory) what would be the phenomena, on the supposition that there is a horizontal whirlwind, say of one hundred miles in diameter, moving with a velocity of seventy-five miles an hour, or 110 feet per second. It is demonstrated in mechanics that if a body moves in a circle, with a radius of sixteen feet, and a velocity of sixteen feet per second, its centrifugal force will be equal to its gravity. And as centrifugal force is directly as the square of the velocity, and inversely as the radius, the centrifugal force of the air in this whirlwind is ascertained by the following proportion:

$$\frac{16^2}{16} : 1 \text{ (gravity)} :: \frac{110^2}{25\times5280} : \frac{1}{74} \text{ or } \tfrac{1}{74}\text{th part of the gravity.}$$

And as a wedge of air fifty miles long is about eight times as heavy as a column of atmosphere equal to its base, its whole centrifugal force will be $8\times\frac{1}{74}$ of fifteen pounds to the square inch, which would cause the barometer to rise about $1\frac{4}{10}$ of an inch in the borders of the storm, both at its commencement and termination; and cause a motion of the air outwards due to this pressure, which would be about 280 feet per second, according to the principles established in a previous part of this essay. Now these two phenomena are entirely wanting in all N. E. storms; for the air does not blow outwards from the storm, nor does the barometer rise at the termination above the mean, though it sometimes does at the commencement, for a reason which shall hereafter be explained. Besides, if such a whirlwind could be generated, it is manifest that it would soon be destroyed by its outward motion, unless some mighty cause exists, of which we have no knowledge, to generate new motion in the air, which would descend from the upper regions of the atmosphere in the middle of the whirlwind, to take the place of that which had thrust itself out by its centrifugal force. It may be added, that the readiness and ease with which the air would descend in this whirlwind, would be so great that the rarefaction of the air in the inside, caused by the centrifugal force of the air would be a quantity very minute, unless we suppose the whirlwind to reach to a great height, which cannot be the case, if it is produced by friction on the West India Islands, and on our coast, as is alleged.

Therefore, it will not account for the great fall which is known to take place in the barometer, during these violent storms, a fact which is fully explained by the theory here proposed. Besides, Mr. Redfield need not be told that this downward motion of the air in the centre of the whirlwind, would increase its capacity for vapour, and effectually prevent deposition.

Bibliographical Notices.

Concise decimal tables for facilitating Arithmetical calculations, &c., designed for practical men. By TIMOTHY CLAXTON, Boston. *Published by the author.**

A sheet containing one of the most concise series of tables which we have ever seen, for facilitating arithmetical operations, has been published by the author, Mr. T. Claxton of Boston. It is accompanied by a pamphlet explanatory of the tables, and containing also an exposition of the system of decimal fractions, a list of data from which the tables are compiled, and an index to them.

The tables may be classed as mathematical, mechanical and miscellaneous. The former contain tables for finding the circumferences and areas of circles from their diameters, the diameters from the circumferences, and square roots of the areas, the side of a square equal in area to a circle from the diameter or circumference given, &c., the solidity of a cone from the square of the diameter of its base, and its height, and a sphere from its diameter, &c. &c. Among the mechanical tables are a series for the reduction of weights and measures, for calculating the weights of solid and hollow cylinders of cast-iron, the weight of square and round bars of iron, of spheres of cast iron, lead, &c. all from convenient data. Among the miscellaneous tables, are those for reducing sterling money to dollars, or vice-versa, the amount of rent or salary for any number of days, having the annual amount &c. &c. There are in all, forty-eight tables conveniently arranged upon a sheet of 10 by 13 inches which may be hung up in the counting-house, or folded for the pocket, for reference.

These tables recommend themselves highly for convenience, and as far as we have examined the calculations we have found them correct. B.

* The Franklin Institute owe to the liberality of the author a number of copies of these tables, which he has requested may be distributed among the members. His object in the publication, is the dissemination of what he justly conceives to be useful matter. COM. PUB.