ON

WHIRLWIND STORMS:

WITH

REPLIES

TO THE

OBJECTIONS AND STRICTURES

OF

DR. HARE.

BY W. C. REDFIELD.

NEW-YORK:

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CORNER OF BEEKMAN AND NASSAU STS.

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OBSERVATIONS ON THE STORM OF DEC. 15, 1839,*

WITH A MAP AND DIAGRAM.

BY WILLIAM C. REDFIELD, A. M.

In the Table and Map which are annexed to these remarks will be found the observations which have been obtained of the direction of wind in the body of this storm, in the states of Connecticut, Rhode Island, Massachusetts, New-Jersey, and parts of the states of Maine, New-Hampshire, Vermont, and New-York.

The arrows on the map denote, approximately, the direction of wind, at or near the hour of noon, at the several places of observation. The concentric lines, drawn at intervals of thirty miles, were added, not as precisely indicating the true course of the wind, but to afford better means of comparison for the several observations.

It will be seen, that of forty-eight distinct sets of observations which are comprised in the annexed schedule, about thirty are derived from the meteorological journals of scientific and intelligent observers, or from the log-books of vessels exposed to the storm; and I take this occasion to offer my thanks to the gentlemen who have so kindly furnished me with their observations.

The position assumed for the axis of the gale, at noon, should, perhaps, be nearly in line with the position of the ship Morrison and Cape Cod Bay; at which places the wind was then blowing from opposite points of the compass, but, as may be seen by the map, not in actually opposing directions. The Morrison was from China, bound to New-York; and I have reason to believe that her position at noon may be safely relied on. The violence of the gale was here so great that the ship, as I am informed, was lying to without canvas. This ship had encountered

* Read before the American Philosophical Society, Jan. 15, 1841.

the western side of the gale, suddenly, at 7 A. M., and the sun shone chiefly unobscured during the greater part of the day.

The gale was severe over the entire surface comprised in the map, except, perhaps, on its extreme northern and northwestern portions, and excepting, also, the lighter winds which were observed near the apparent axis of the gale, in the region of Buzzards' and Cape Cod bays, &c. in the afternoon and evening. A very heavy fall of snow accompanied the gale in the states of Connecticut, Rhode Island, Massachusetts, New-Hampshire, and Maine; also, in some parts of New-York and southern Vermont. Some snow also fell in the western and northern parts of New-York and Vermont, but attended with more moderate and variable winds, chiefly from the north and west.*

The southwesterly and southerly winds, which connect the westerly with the southeasterly winds in the circuit of rotation, are found at Nantucket in the afternoon, by the farther advance of the storm, and also in the log-books of a number of vessels whose positions were eastward and southward of the ship Morrison, but beyond the limits of the map.

The barometric minimum, as in other storms, appears to have nearly coincided, in its progress, with the apparent axis of the gale.

My main object in collecting the observations contained in the subjoined schedule, has been to establish the course of the wind in the body or heart of the storm at a given time, and apart from all other considerations. I am in possession, however, of more extended observations of this gale. Many of these appear to agree with some of the following characters or modes of action which pertain, more or less, to many of the storms or gales that visit the United States and other regions. These characters have claimed attention from almost the earliest period of my inquiries.

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* Of the absence of this gale in western New-York, there is abundant evidence; I quote only the following:—"The great storm of December 14–15, which produced so much devastation on the coast of Massachusetts, and which extended to Connecticut, Long Island Sound, New-York, &c. was scarcely felt in the middle and western parts of the state. On the coast, the wind was from some of the easterly points. In this place, [Rochester, N. Y.] the wind was not strong, and was chiefly from the north and west, attended by a snow of two or three inches."—*Annual Report of Regents of the University, March*, 1840, p. 201.

1. The body of the gale usually comprises an area of rain or foul weather, together with another, and perhaps equal, or greater, area of fair or bright weather.

2. The fall of rain or snow often extends, in some direction, greatly beyond the observed limits of the gale.

3. The gale itself not unfrequently exhibits an apparently unequal extent of action, or degree of violence, on different sides of its apparent axis of rotation.

This peculiarity, as well as the second, is most common in winter storms, and in those which sweep over an extensive continental surface; and, like other irregularities, is less noticeable in the storms which are traced solely on the ocean.

4. The barometric indications of a gale commonly extend much beyond the observed limits of its action.

5. The body of the gale constitutes a determinate sheet or stratum of moving air; and of this sheet or stratum a large portion sometimes overlies another and more quiescent stratum of air, the latter having, perhaps, a different motion; as may be often observed in the common winds of the temperate and higher latitudes: in which case the gale is either not felt at the surface of the earth, or the observed changes of wind are found, in part, unconformable to the conditions of a moving whirlwind.

6. Owing to the convergent and somewhat variable courses of storms in the extra-tropical latitudes, as well as to their unequal rates of progress, two storms will sometimes cover, in part, the same field, one of which will overlie the other, and perhaps thin out at its margin, in the same manner as common winds. This, also, may occasion in part a different order of change in the observed winds and weather from that which is commonly noticed in a regular whirlwind storm.

Owing to these and other causes, the oscillations of the barometer are often irregular or unsymmetrical on opposite sides of a gale; and this is particularly noticeable in the higher latitudes.

7. In most gales of wind there is, probably, a subordinate motion, inclining gradually downward and inward in the circumjacent air, and in the lower portions of the gale; and a like degree of motion, spirally upward and outward, in the central and higher portions of the storm. This slight vorticular move-

ment is believed to contribute largely to the clouds and rain which usually accompany a storm or gale; and is probably due, in part, to the excess of external atmospheric pressure on the outward portions of the revolving storm.

8. In storms which are greatly expanded there is sometimes found an extensive area of winds, of little force and variable direction, lying within the circuit of the true gale, and attended throughout with a depressed state of the barometer. This more quiescent portion of air in the centre of the gale has been found to extend, in some cases, to a diameter of several hundred miles.

In the case now before us, the direction of the arrows representing the course of the wind at noon, as carefully drawn on a larger map, shows an average convergence, or inward inclination, of about six degrees. But it is not deemed safe to rely upon this result in a single case, which is liable to be affected by the errors of observation and the deflecting influences of the great valleys and lines of elevation, as well as by the errors of approximation which often arise from referring all winds to eight, or, at most, to sixteen points of the compass.

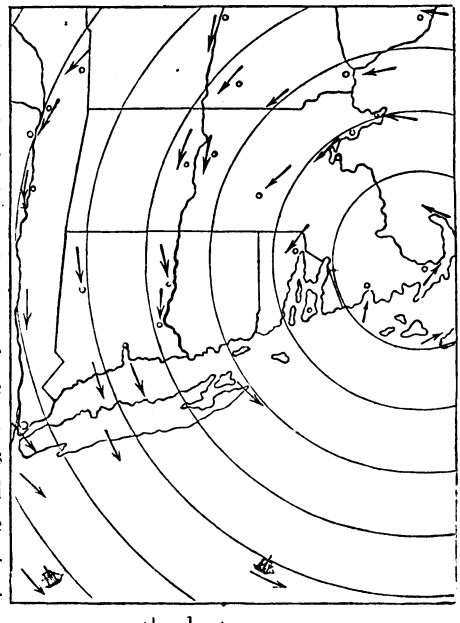
It is not intended, on this occasion, to support the foregoing characteristics by such extended details of evidence as their discussion would necessarily demand; and they are mentioned here only because the true character of the rotation in these gales, as well as the necessary or incidental connection of this rotation with other phenomena which attend them, has seemed to be often misapprehended.

As relates to the whirling or rotary action in the case before us, it may be remarked, that had we obtained no observations from the northwestern side of the axis of this gale, it would have been easy, in the absence of more strictly consecutive observations than are usually attainable, to have viewed the initial southeasterly wind of the gale,* and the strong northwesterly wind which soon followed, as two distinct sheets or currents of wind, blowing in strictly opposing directions; and if we could so far lose sight of the conservation of spaces and areas, the laws of momentum and gravitation, together with a continued depression of the barometer within the storm, we might then have sup-

* Observed between the coast of Massachusetts and latitude 25° N.

posed one of these great winds, if not both, to have been turned upward by an unseen deflection, and to have doubled back upon itself in the higher atmosphere. But the case neither calls for nor admits these speculations. If, however, the axis of this gale had chanced to pass westward and northward of our limits of correct observation, in pursuing its northeasterly course, as did, perhaps, that of the storm of December 21st, 1836, which has been ably examined and discussed by Professor Loomis,* it is, in such case, more than probable that its whirlwind character would not have been established.

[*Note.*—It having been claimed that this and other storms had been found to blow *inward*, towards some central point or line, I was induced to prepare and make public, shortly after the occurrence of this storm, a statement of observations on the direction of the wind *at or near sunset*, from such evidence as was then in my possession, and illustrated by a small geographical sketch or diagram.



To that sketch, which is here subjoined, I have now added the latest observations on the 15th, at the following places, viz: Culloden Point, Worcester, position of ship Morrison, Stratford, Fire Island, Keene, West Point, Salem, (N.Y.) and the position of the barque Ann Louisa. It will be seen that the assumed axis of the storm on this sketch is more advanced in its northeasterly course than appears in the larger diagram of the observations made at noon, as seen on the last page.

I have seen no satisfactory evidence that the revolving character has been wanting in any active American storm.]

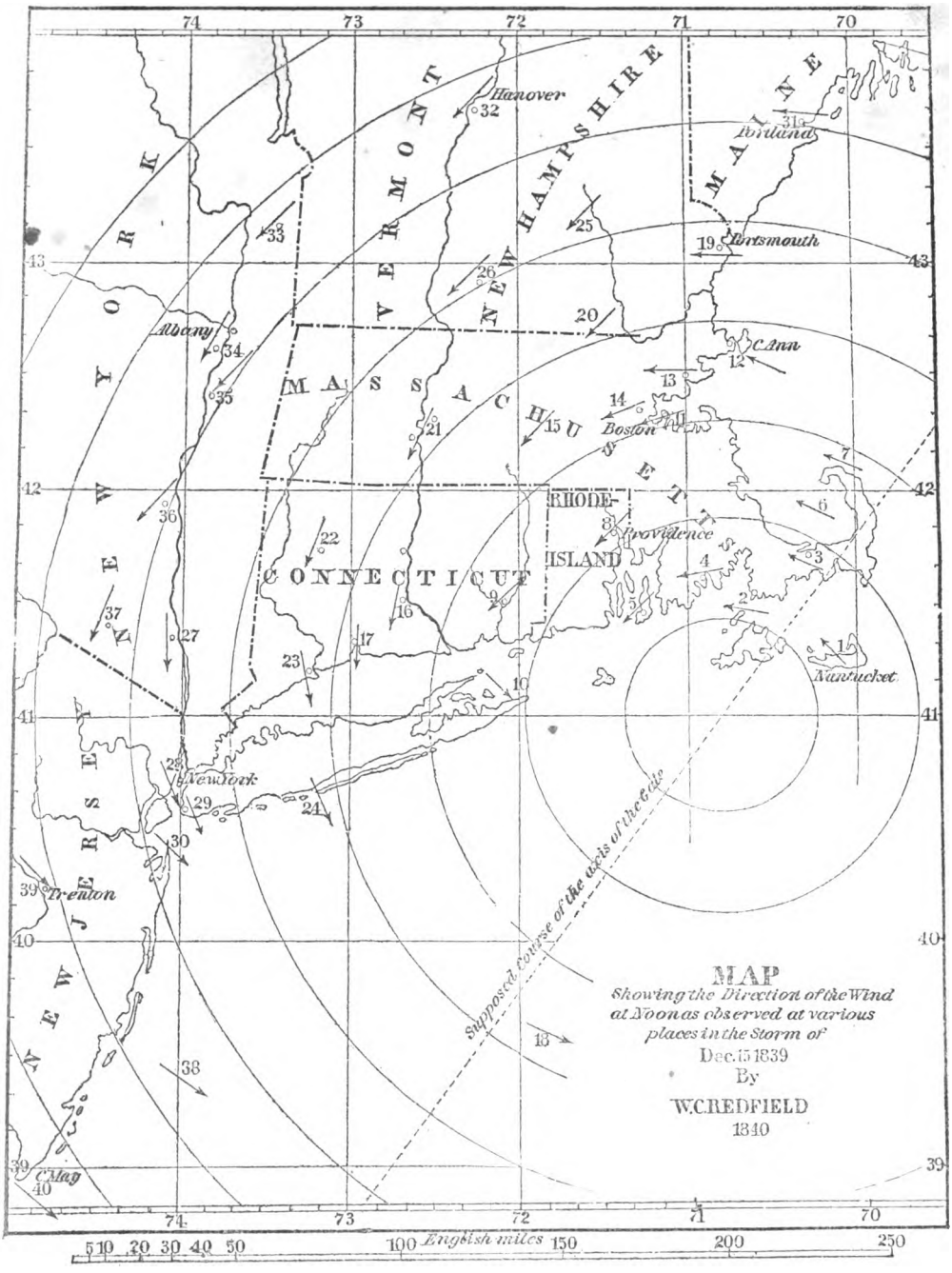
* Trans. Am. Phil. Soc. Vol. VII, p. 125—163.

Schedule of Observations on the Direction of Wind in the Storm of December 15th, 1839: With a Map indicating the Direction of the Wind at or near the hour of Noon. By WILLIAM C. REDFIELD.

No.	*Places of Observation.*	*A. M.*	*Noon.*	*P. M.*	*Observers and Authorities.*
1	Nantucket, Ms.	E.	S.E. at 1 *p. m.*	S. W.	Report of James Mitchell, as published by Mr. Espy. [Nantucket.
2	Woodville, Ms.	. . . .	"A little S. of E."	Clouds broke at W.	Observations on board Steamboat Telegraph, by William Mitchell of
3	Barnstable, Ms.	N. E. at 7 *a. m.*	[E. S. E.]	before 2 P. M E. at 2 *p. m.*: S. E.	Report to Editor of Boston Courier. } *I take the mean of E. and S. E*
	Do.	Gale from S. E.		at Sunset. S.W. *p. m.*: Clear	Letter of Wm. H. Brown to W. C. R. } *for true direction at Noon.* W.C.R.
4	New Bedford, Ms.	Sunrise, N.E. mod.	[E. by N.]	at Sunset. 2 *p. m.* E. N. E.: 3½	Joseph Congdon's Meteorological Journal. } *I take E. by N. as the mean*
	Do.	do. E. fresh,		p. m. s. do. E: Sunset S S E	Sam'l Rodman's. do. as publ'd by Mr. Espy. } *for Noon.*
5	Newport, R. I.	N. E.	N. E.	N. E.	Meteorological Journal published at Newport.
6	Cape Cod Bay,	E. S. E.	E. S. E.	E. S. E. at 2 *p. m.*	Report of Capt. Slemmer, Brig Columbus.
7	Provincetown, Ms.	E. S. E.	E. S. E.	E. S. E.	Marine Reports in Boston Newspapers.
8	Providence, R. I.	N. E.	N. E.	N. E.	Professor Caswell's Meteorological Journal.
9	Norwich, Ct.	N. E.	N. E.	N. E.	Norwich Courier.
10	Culloden Point, N. Y.	*"changed to*	*N. W. at Noon."*	. . . .	Capt. Green's Account, as published by Mr. Espy.
11	Boston, Ms.	Sunrise, N. E.	E. N. E. [E. 17° N.]	Sunset, E. S. E.	Wm. Cranch Bond's Meteorl. Journal. } *I take the mean of the observa-*
	Do.	E. by N.	E. by N. }	E. by N.	Robert Treat Paine's Observations. } *tions at Noon.*
12	Gloucester, Ms.	E. S. E.	E. S. E.	E. S. E.	Letter from Gloucester, in the Boston Newspapers.
13	Salem, Ms.	Eastward.	Eastward.	Eastward.	Salem Gazette.
14	Waltham, Ms.	N. E.	[E. N. E.]	E.	Monthly Met. Jour., by C. F., in the Boston Daily Centinel.
15	Worcester, Ms.	N. E.	N. E.	N. E.	Met. Journal at State Lunatic Hospital—in National Ægis.
16	Middletown, Ct.	N. N. E.	[N. by E.]	N.	Reported by Professor Smith. } *I take N. by E. for the mean at Noon.*
	Do.	N.		N. N. E.	Dr. Barratt's Met. Journal. }
17	New Haven, Ct.	N. by W.	[N. 3° E.]	N. N. W.	Report of Capt. Woolsey, Steamboat Providence. } *I take the mean of*
	Do.	N. N. E.		N. N. E. till 1½ *p.m.*	Judge Darling's Meteorological Journal. } *N. 3° E.*
18	Ship Morrison, at sea: Lat. 39° 35 N. Lon. 71° 50 W.	S. E.: W. N. W.	W. N. W.	W. N. W.	Ship's Log Book—also, Statements of Capt. Benson and his Officers.

No.	Places of Observation.	A. M.	Noon.	P. M.	Observers and Authorities.
19	Portsmouth, N. H.	E.	E.	E.	Weekly Meteorological Journal, published at Portsmouth.
20	Nashua, N. H.	N. E.	N. E.	N. E.	Nashua Telegraph.
21	Northampton, Ms	N. E.	[N. N. E.]	N. E.	Observations of W. Atwill and others. *I assume the approximate mean*
	Amherst, Ms.	N. by W.		N. by W.	Professor Snell's Met. Journal. *of N. N. E. for Noon.*
22	Litchfield, Ct.	Night of 14, 15, N. E	[N. N. E.]	N. at Night of 15th.	Litchfield Enquirer. *Assumed mean for noon of 15th, N. N. E.*
23	Stratford, Ct.	N. by W.	N. by W.	N. by W.	Rev. J. R. Linsley's Meteorological Journal.
24	Fire Island Beach, N. Y.	Midnight, N. E. : veered by N.	N. N. W.	N. N. W.	Captains Cartwright and Skiddy, employed at the Beach.
25	Concord, N. H.	Northeasterly.	N. E.	N. E. and more N'ly	Letter from Concord to S. G. Arnold; from Mr. Arnold.
26	Keene, N. H.	N. E.	N. E.	N. E.	Rev. Z. S. Barstow's Meteorological Journal.
27	West Point, N. Y.	N.	N.	N.	Meteorological Journal of the Medical Department.
28	New-York City,	N. by W.: N. N. W.	N. N. W.	N. W. by N.	Meteorological Journal of W. C. Redfield.
	Fort Wood, N. Y. Harbor	N.	[N. N. W.]	N. W.	Met. Journal of Medical Officer. *Mean of N. N. W. taken for Noon.*
29	Flatbush, N. Y.	N.	[N. N. W.]	N. W.	Rev. T. M. Strong's Met. Jour. *Mean of N. N. W. assumed for Noon.*
30	Sandy Hook Bay, N. Y.	N.	N. W.	N. W.	Log Book of Bark Osceola.
31	Portland, Me.	N. E. : at 11 E.	[E. 6° S. mean.]	E. by S.	Met. Report of Keeper of Marine Observatory: Published at Portland.
32	Hanover, N. H.	N. E.	[N. E.]	N.	Professor Young's Meteorological Journal.
33	Salem, N. Y.	N. E.	N. E.	N. E.	William Brand and W. Larkin; Meteorological Journal.
34	Albany, N. Y.	N. E.	[N. 28° E.]	N. E.	T. Romeyn Beck, M. D. Met. Journal. *Mean assumed for Noon,*
	Lansingburgh, N. Y.	N. N. E		N.	E. T. Foote, Meteorological Journal. *N. 28° E.*
35	Kinderhook, N. Y.	N. E.	N. E.	N. E.	Silas Metcalf, Meteorological Journal.
36	Kingston, N. Y.	N. E.	N. E.	N. E.	Isaac Blauvelt; Meteorological Journal. [*noon, N. N. E.*
37	Goshen, N. Y.	N. E.	[N. N. E.]	N.	Nathaniel Webb and John S. Crane; Met Jour. *Mean assumed for*
38	Bark Ann Louisa, off Absecom, N. J.	W. N. W.	N. W.	N. W.	Ship's Log Book, and Statement of Capt. Wilson.
39	Trenton, N. J.	N. W.	N. W.	N. W.	Dr. F. A. Ewing's Meteorological Journal.
40	Cape May, N. J.	N. W.	N. W.	N. W.	Marine Reports, and Letter from Cape May, in Philad. Newspapers.

Abbreviations.—N. H. State of New Hampshire; Me. Maine; Ms. Massachusetts; R. I. Rhode Island; Ct. Connecticut; N. Y. New-York; N. J. New Jersey.——*Note.* My own observations on the 15th P. M. have on a former occasion been erroneously printed N. W. by *W.*; for which read N. W. by *N.*



REMARKS ON THE TORNADO

WHICH VISITED

NEW-BRUNSWICK IN THE STATE OF NEW-JERSEY,

JUNE 19, 1835,

WITH A PLAN AND SCHEDULE OF THE PROSTRATIONS

OBSERVED ON A SECTION OF ITS TRACK.

BY W. C. REDFIELD.

[From the Lond. Ed. and Dub. Phil. Mag. Revised by the Author.]

THE NEW-BRUNSWICK TORNADO.

In a paper printed in the American Journal of Science, I have referred to the support given by Prof. Bache to Mr. Espy's theory of storms, at the meeting of the British Association in 1838, founded upon observations made on the New-Brunswick tornado, and have stated, that in my own examinations I had observed numerous facts which appear to demonstrate the *whirling* character of this tornado, as well as the *inward* tendency of the whirling vortex at the surface of the ground; and further, that the direction of rotation was *towards the left*, as in the North Atlantic hurricanes.* It was due to Professor Bache that my observations should be brought forward; a task which has been too long delayed, partly from a desire that he would revise his former conclusions. The facts which I shall now present, form part of the evidence to which I then alluded.

If the effects which are here presented for consideration be due to "a moving column of rarefied air without any whirling motion at or near the surface of the ground," as maintained by Professor Bache,† we might expect to find a relative uniformity in these effects on the two opposite sides or margins of the track. How far this is from the true state of the case may be seen by inspecting the observations which are found upon the annexed survey and plan of prostrations.‡

* Amer. Journ. of Science, Oct. 1838, vol. xxxv. pp. 206, 207.

† Transactions of Amer. Phil. Society, vol. v, p. 417, New Series.

‡ See the plan and sketches of the prostrations on a section of this tornado, at the close of this article.

The occurrence of these tornadoes appears to have been noticed from the earliest antiquity; and their violence has been considered as the effect of an active whirling motion in the body of the tornado; this peculiarity of action having often been supported by the testimony of eye-witnesses.

The whirling motion, however, has not been recognized by Prof. Bache, Mr. Espy,* or Prof. Walter R. Johnson,† in their several accounts of the New-Brunswick tornado; these writers having been led to adopt or favor a theory of ascending columns of air, without whirling motion, founded on the supposed influence of calorific expansion accompanying the condensation of vapor.

It is remarkable that previous to this period the evidences of the rotation or other characteristic action of tornadoes appear not to have been duly examined and recorded, nor even to have received the distinct consideration of scientific observers. We are therefore left to seek out the peculiarities of their action, by examining the direction of the prostrations and other effects of the wind; and from a careful induction from the effects which are thus registered as by the finger of the tornado, we may hope to arrive at satisfactory conclusions.

If the numerous prostrations of trees and other objects, which may be observed in the path of a tornado, be the effects of a violent whirlwind, it appears most reasonable to infer that this whirl had the common properties which may be observed in all narrow and violent vortices, viz. *a spirally descending and involuted motion* of the exterior and lower portions of the vortex, rapidly quickened in its gyrations as it approaches toward the centre or axis of the whirl, and thence continued (in the case of the whirlwind) spirally upward, but gradually expanding in its spiral course by an evolute motion in ascending towards the extreme height of the revolving mass.

If we now contemplate the action of this whirling body, while in a state of rapid progression, on the several objects found in distinct portions of its path, we may expect to witness effects of much complexity, particularly as regards direction; and,

* Trans. Amer. Phil. Society, vol. v, New Series.

† Journ. Academy Nat. Sciences of Philadelphia, vol. vii, part ii.

that amid this apparent complexity, some clue may be obtained that will serve to indicate or establish the true character of its action. Some of the effects which may be expected, or observed, in the path of the tornado will be here considered.

1. We may expect to find, in the path of the whirlwind, strong evidence of the inward or *vorticular* course of the wind at the earth's surface; the violence of which inward motion is clearly indicated by the force with which various objects, often of much weight, are carried spirally upward about the axis of the revolving body.

Now the effects of this inward vorticular motion at the surface of the ground, are clearly manifested in the cases before us; and are also well illustrated by Prof. Bache, in his paper on this tornado, although referred by him to a different action.*

2. As the effects which may be observed at various points in the track were produced at different moments of time, and by forces acting in different directions, as well as of various intensities, we may expect to find great diversities in the several directions of the fallen trees and other prostrated bodies; and further, as all the forces, in addition to their inward tendency, have likewise a common tendency in the direction pursued by the tornado, we may expect to find, also, full evidence of this progressive force in both the specific and mean direction of the fallen bodies.

These effects, I need hardly state, are distinctly observed in the case before us; and appear likewise from the observations of Professor Bache. The results already noticed have been observed also in the tracks of other tornadoes; so that a general inclination, both inward and onward, amid the various and apparently confused directions of the fallen bodies, is distinctly recognized by all parties to this inquiry.

3. It has been often noticed, that where two fallen trees are found lying across each other, the uppermost or last fallen points most nearly to the course pursued by the tornado.

In view of the facts above stated, much pains have been taken to establish, as by induction, a central and non-whirling course in the wind of the tornado; first inward and then upward, like

* Transactions of American Philosophical Society, vol. v.

that resulting from a common fire in the open air. I do not propose to notice the insuperable difficulties which appear to attend this hypothesis. It is important to state, however, that all the above mentioned effects, when theoretically considered, are, at least, equally consistent with the involute whirling action of an advancing vortex. This important consideration I have not seen recognized by the advocates of the non-whirling theory; and it seems proper, therefore, to point out, as we proceed, other and more distinguishing effects of the whirling action.

4. It has been noticed, also, that the directions given to broken limbs and other bodies, by the successive changes in the direction of the wind as the tornado passed over, have been found in opposite courses of change, on the two opposite sides of the track.

This fact, too, has been relied on as disproving a rotary motion in the body of the tornado. But, unfortunately for the alleged objection, this effect accords fully with the rotary action of a progressive body of atmosphere; as is well known to all who clearly understand the *theory* of rotary storms.

In all such whirling masses the successive changes in the direction of the wind at any fixed point on the surface, *are mainly the result of their progressive motion*, and necessarily take place in opposite directions or courses of change on the two opposite sides of the advancing axis. This indication fails, therefore, as a theoretic test; and I now proceed to notice others, which are peculiar to a progressive whirling action.

5. In considering further the effects of such action, we may expect to find that the greatly increased activity of gyration which is always observed near the centre of a vortex, will be indicated by a more violent and irregular action in and near the path pursued by the axis of the whirlwind, than is found under its more outward portions.

This effect is often strikingly exhibited in the path of tornadoes; while, in the supposed ascent of a non-whirling column, it would seem that no part of the surface would be so much exempted from its action, and particularly from its power of prostration, as that lying near its centre.

6. As the effect of rotation must be to produce, on one side

of the advancing axis, a reverse motion which is contrary to the course of the tornado, it is evident that on this side the prostrating power will be be much lessened; that the cases of prostration, therefore, will be here less numerous; and that some of these, at least, will be produced in a backward direction, more or less opposite to the course of the tornado. By this criterion, not only the whirling movement, but the direction of the rotation also, may be clearly ascertained.

This effect is best observed by comparing the directions of the several prostrations, on the two opposite margins of the track, and is strongly exemplified in the case before us. Here we find, that most of the trees prostrated within five chains (110 yards) from the northern or left-hand margin of the track, lie in directions which are more or less backward from the course of the tornado: this course having been nearly east. The prostrations in this part of the track are also, for the most part, less general than on the opposite side of the axis,* a greater portion of the trees being left standing.

It sometimes happens, owing perhaps to the inward or involute motion having exceeded the progressive motion at a particular point, that some inclination backward will be found in the prostrations on the progressive side of the whirl, as seen on the plan or sketch, Nos. 77 to 80. But these unfrequent cases by no means compare in number with the numerous backward and sometimes *outward* prostrations, found on the reverse side of the whirl, as illustrated by Nos. 1, 3, 4, 7, 9, 10, 12, 13, &c. on the left side of the track. Thus we find here a satisfactory indication that this tornado was a whirlwind; and that the course of its rotation was to the *left*, or contrary to the hands of a watch.

7. It is also apparent, that the prostrating power of a whirlwind on the side of its reversed motion as just considered, will be limited to a shorter distance from the axis of rotation than on the opposite side, where the rotary force coincides with the onward motion.

* There was a vacant space in the belt of wood, immediately to the right of the line *c c* or axis of the tornado, owing to which the effect mentioned does not appear so obvious in the figure.

This is seen in the more narrow limit or extent of the prostrations on the north or left margin of the track, as compared with the wider extent of those prostrations which incline inward on the right side of the apparent axis. There were many trees standing beyond the northern border of the track as shown on the plan, but none had fallen.

8. It follows, in like manner, that on that side of a whirlwind in which the rotary motion coincides with the progressive movement, the prostrating power will not only be increased in its intensity, but will also be effective over a wider space; and that few, if any, of the prostrated bodies will be found to have been thrown backward.

In the case before us, as may be seen in the sketch, the prostrations are found to extend on the southern or right side of the apparent axis to a distance nearly twice as great as on the left side. The same general result has also been noticed in the tracks of other tornadoes which I have examined.

The facts here considered are too important to be overlooked, and seem fully to establish both the whirling action and its specific direction.

9. But further: if a rotative action be exhibited, the mean directions of all the prostrations, on each of the two opposite sides, will differ greatly in their respective inclinations to the line of progress, and the mean direction of those on the reverse side of the axis will be found more backward than on the opposite side, where the rotative course coincides with the progressive action.

In the case before us, the mean direction of all the prostrations on the right side of the track is found to incline 52 degrees inward from the line of progress. The course of the tornado is here taken to be east; although for the last half mile its course had been a little north of east. On the left side, the mean direction is found to be S. 3° W., or 93 degrees inward and backward; showing a difference in the mean inclination from the course, on the two opposite sides, of 41 degrees.*

If we now take the indications afforded by the two exterior

* The inclinations of the fallen trees from the course, on both sides the axis, are reckoned inward and backward.

portions of the track, to the width of five chains on each side, where the effects are more distinctive in their character, we find, on the right side, a mean inward inclination of 46 degrees, the mean direction being N. 44° E.; while, on the left side of the track, the mean inclination is not only inward but 48 degrees backward, the mean direction on this side being S. 48° W. We have thus a mean difference in the inclination of the fallen trees, on the two exterior portions of the track, of *no less than ninety-two degrees.*

These indications seem conclusive, also, in favor of the whirling action in the direction from right to left.

10. Although of less importance, it should be mentioned that the diminished action of the tornado which is commonly observed on the hillsides and summits over which it passes, and the greatly increased action in the bottoms of the valleys, and even in deep ravines, afford a strong argument against ascribing the effects to the ascent of a non-whirling rarefied column; as the latter, it would seem, must act with greater force on the hillsides and summits than in the bottoms of valleys. The general correctness of the fact here alleged cannot justly be questioned.

11. The sudden and extraordinary diminution of the atmospheric pressure which is said to take place at the points successively passed over by a tornado, causing the doors and windows of buildings to burst outwards, seems to afford strong confirmation of a violent whirling motion; for an effect of this kind is necessarily due to the centrifugal and upward force of the vorticular action in the interior portion of the whirlwind. There are no other means known by which such an abstraction of pressure can be effected in the open air. An increase of calorific elasticity, if such were produced, either generally or locally, would not greatly disturb the equilibrium of pressure, being resisted by the surrounding and incumbent weight of the entire atmosphere. Besides, the immediate effect of such increased elasticity might rather be to burst *inward* the windows and doors of buildings exposed to its action.

Some of the more important indications mentioned above appear also from an examination of Prof. Bache's observations; although the latter are not all definitely located, as regards the

extreme borders and the axis of the track. Thus, in Fig. 7 of Professor Bache's paper, assuming the course of the tornado to be east, and rejecting a few observations near the centre, to avoid error, we find in twenty observations on the right side of the track, a mean inward inclination of 64 degrees, and for nine observations on the left or reverse side of the whirlwind, a mean inclination, reckoned inward and backward from the course, of 104 degrees; being 14 degrees backward.

It is stated by Prof. Bache, "that the trees lying perpendicular to the track of the storm, are not those furthest from the centre of that track." This generalization accords with my own observations; but not with the statements of Mr. Espy, and can hardly be reconciled with an inward non-whirling motion in the tornado.

It may appear to some, that in the case of a whirlwind the greater portion of the prostrations on the reverse side of the axis should be found in a backward direction; and so they would undoubtedly be found, were it not for the inward and the progressive action. But the force is here so far lessened by the reverse action above noticed, that, in most cases, only a small portion of the trees exposed will be thus prostrated; while the greatest force of the whirlwind, on this side, is felt in its later or closing portion, near the apparent axis, where the inward, together with the rotative and progressive forces, seem to combine their influence in the closing rush towards the heart of the onward moving vortex. This appears to account for the nearly opposite directions of prostration found on this side of the track: and it is apparently by this more violent and closing action, that many trees which were first overthrown in a direction nearly across the centre of the path, were again moved from their position, or swept onward nearly in the course of the tornado. It is proper to remark here, that an attentive examination of these effects has served to convince me that on the right and more central portions of the track the prostrations, for the most part, take place either at the outset or under the middle portions of the whirlwind; while on the left or reverse side, up to the line of the apparent axis, and even across the latter, they occur chiefly under the closing action of the whirl, as above

described. The violent effects of the more central and closing action of the vortex, are more clearly seen as we advance from the left-hand margin towards the centre or apparent axis of the path.

From the causes to which I have just alluded, the effects are usually more violent on and near the line passed over by the axis of the vortex, than in other portions of the track. This *line of greatest violence* is found to coincide nearly with the line which separates the inwardly inclined prostrations of the two opposite sides of the track.* The latter line, or apparent axis of the track, is sometimes called the *line of convergence*, and is indicated on the plan, by the line and arrow *c c*. Along this line, from the causes just mentioned, aided also by the elevating forces about the axis, many of the trees are swept onward, and left with their tops in a direction nearly parallel to the course of the tornado; forming an apparent, but not a just exception, to the more lateral direction which pertains to most of the trees prostrated by the onset of the whirlwind, near the central portions of the track. Indeed, the central or closing violence of the advancing whirl is here so great, that the trees are not unfrequently torn out of the ground and carried onward to considerable distances.

It is proper to state here, that in the tracks of all the tornadoes which I have had opportunity to examine, and in some, at least, of those examined by others, the course of rotation has been found the same as in the case before us.†

In order to make a just and satisfactory examination of the effects of a tornado, it appears necessary to select portions of the track where the extension of wood or single trees, on each side, is found sufficient to mark clearly the exterior limits of the prostrating power, and where the effects on both sides of the axis are also clearly developed. Our next care should be to ascertain, as near as may be practicable, the line which separates the opposite convergence of the two sides, noticed above

* The line of greatest violence, for the most part, is found somewhat *to the right* of the line of convergence.

† As in the tornado which passed through Allegany county, N. Y. July 25th, 1838; described by Mr. Gaylord in the American Journal of Science, vol. xxxvii, p. 92.

as the axis or line of convergence. We should then determine the general direction of this line and of the track at the place examined; which being done, we may proceed to measure the distance to which the prostrations are extended on each side, and then carefully to take the position and direction of prostration of each and of all of the fallen bodies, noting with care, also, any other phenomena which may serve to aid our inquiries. We may thus obtain valuable materials for future analysis; and this course of investigation, if faithfully pursued, will, it is believed, remove all reasonable doubt of the rotative action of these tornadoes. An examination of their probable origin, or of the causes of their enduring activity and violence, belongs not to the present occasion.

New-York, 5th February, 1841.

SCHEDULE AND PLAN OF PROSTRATIONS.

SCHEDULE OF THE PROSTRATIONS

Observed on a Section of the Track of the New-Brunswick Tornado, of June 19th, 1835

No.	Direction of Prostration.	Inclination.
	TABLE I. Left side of the track to the line *b b*—5 chains.	Inward and backward.
a	Tree lies S. 20° W. . .	110°
b	.. .. S. 80 W. . .	170
1	.. .. N. 67 W. . .	203
2	.. .. S.	90
3	.. .. W.	180
4	.. .. S. 80 W. . .	170
5	.. .. S. 40 W. . .	130
6	.. .. S. 40 W. . .	130
7	.. .. S. 80 W. . .	170
8	.. .. S. 10 E. . .	80
9	.. .. S. 50 W. . .	140
10	.. .. S. 50 W. .. .	140
11	.. .. S. 26 W. . .	116
12	.. .. S. 50 W. . .	140
13	.. .. S. 65 W. . .	155
14	.. .. South . . .	90
	Mean direction, 16 cases, S.48°W. Mean inclination from course, inward and backward, 138 degrees.	
	TABLE II. Left side of the axis, from the line *b b* to *c c*—1¼ chains.	
15	Tree lies S. 2° E. . .	88
16	.. .. S. 12 W. . .	102
17	.. .. S. 35 E. . .	55
18	.. .. S. 62 E. . .	28
19	.. .. S. 25 E. . .	65
20	.. .. N. 80 W. . .	190
23	.. .. S. 20 E. . .	70
24	.. .. S. 80 E. . .	10
25	.. .. S. 45 E. . .	45
26	.. .. S. 10 E. . .	80
27	.. .. S. 45 E. . .	45
28	.. .. S. 20 E. . .	70
29	.. .. S. 60 E. . .	30
30	.. .. S 60 E. . .	30
31	.. .. East. . . .	00
32	.. .. S. 75 E. . .	15
33	.. .. S. 56 E. [Included as belonging by its inclination to this table.]	34
35	.. .. East . . .	00
	Mean direction, 18 cases, S. 37° E. Mean inclination inward, 53 deg.	
	TABLE III. Right side of apparent axis from line *c c* to W E—3 chains.	
21	Tree lies N. 56° E. . .	34
22	.. .. N. 60 E. . .	30
34	.. .. N. 80 E. [Included as belonging by its inclination to this table.]	10
36	.. .. N. 85 E. . .	5
37	.. .. East (two) . .	0
..	.. .. East . . .	0
38	.. .. East . . .	0
39	.. .. N. 30 E. . .	60
40	.. .. N. 70 E. . .	20
41	.. .. N. 55 E. . .	35
42	.. .. N. 50 E. . .	40
43	.. .. N. 78 E. . .	12
44	.. .. N. 45 E . .	45
45	.. .. N. 45 E. . .	45
46	.. .. N. 25 E. . .	65
47	.. .. N. 35 E. . .	55
48	.. .. N. 40 E. . .	50
	Mean direction, 17 cases, N.60° E. Mean inclination inward, 30 deg.	

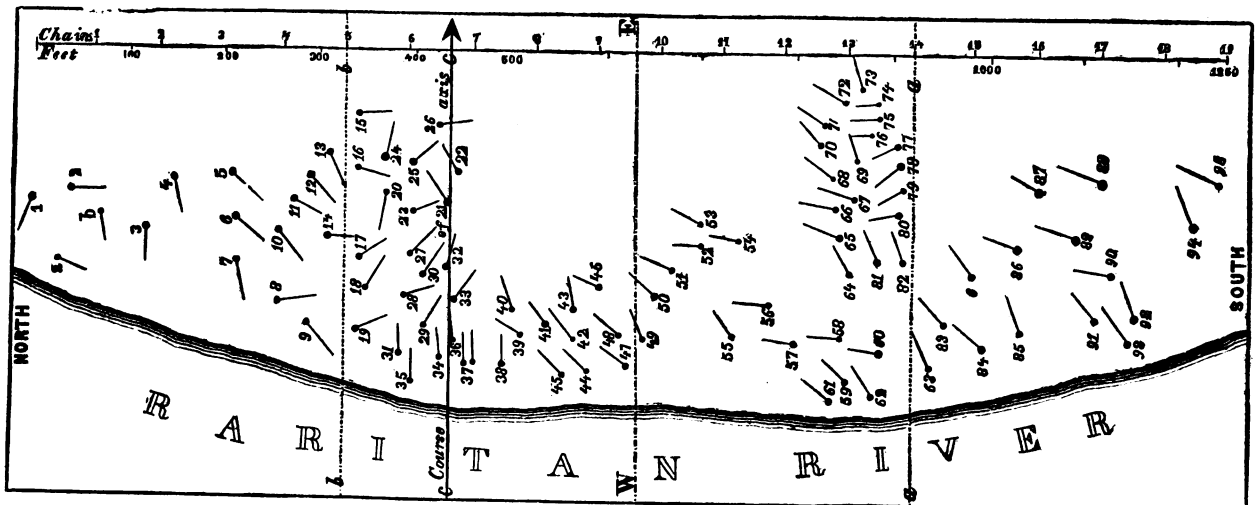
No.	Direction of Prostration.	Inclination
	TABLE IV. Right side of axis, from line W E to line *a a*—4½ chains.	Inward and backward.
49	Tree lies N. 67° E. . .	23°
50	.. .. N. 45 E. . .	45
51	.. .. N. 22 E. . .	68
52	.. .. N. 3 E. . .	87
53	.. .. N. 30 E. . .	60
54	.. .. N. 10 E. . .	80
55	.. .. N. 35 E. . .	55
56	.. .. North . . .	90
57	.. .. N. 10 E. . .	80
58	.. .. N. 3 E. . .	87
59	.. .. N. 45 E. . .	45
60	.. .. N. 10 E. . .	80
61	.. .. N. 35 E. . .	55
62	.. .. N. 60 E. . .	30
63	.. .. N. 40 E. . .	50
64	.. .. N. 20 E. . .	70
65	.. .. N. 10 E. . .	80
66	.. .. N. 20 E. . .	70
67	.. .. N. 40 E. . .	50
68	.. .. N. 70 E. . .	20
69	.. .. N. 50 E. . .	40
70	.. .. N. 35 E. . .	55
71	.. .. N. 30 E. . .	60
72	.. .. N. 50 E. . .	40
73	.. .. North (two) .	90
74	.. .. North " .	90
75	.. .. North " .	90
76	.. .. N. 20 W. (clump of 3)	110
77	.. .. N. 35 W. . .	125
78	.. .. N. 30 W. . .	120
79	.. .. N. 10 W. . .	100
80	.. .. N. 65 E. . .	25
81	.. .. N. 70 E. . .	20
82	Mean direction, 33 cases, N. 24° E. Mean inclination, 66 degrees.	
	TABLE V. Right side of track, from line *a a* to outward limit of prostration—5 ch.	
63	Tree lies N. 65° E. . .	25
83	.. .. N. 45 E. . .	45
84	.. .. N. 40 E. . .	50
e	.. .. N. 55 E. . .	35
85	.. .. N. 70 E. . .	20
86	.. .. N. 23 E. . .	67
87	.. .. N. 31 E. . .	59
88	.. .. N. 20 E. . .	70
89	.. .. N. 22 E. . .	68
90	.. .. N. 10 E. . .	80
91	.. .. N. 55 E. . .	35
92	.. .. N. 70 E. . .	20
93	.. .. N. 55 E. . .	35
94	.. .. N. 68 E. . .	22
95	.. .. N. 25 E. . .	65
	Mean direction, 15 cases, N. 44° E. Mean inclination inward from course of tornado, 46°.	

Mean direction of all the prostrations on *left* of axis, 34 cases, S. 3° W.—being 3° backward, or 93° inward and backward.

Mean direction on *right* of axis, 65 cases, N. 38° E.—being 52° inward from course.

Difference of mean inclination on the two sides, 41 degrees.

Difference of opposite marginal sections (Tables I and V) 92 degrees.



Sketch of the Prostrations found on a section of the Track of the Tornado of June 19, 1835, *on the bank of the Raritan, opposite the City of New Brunswick, in the State of New-Jersey.*

EXPLANATIONS.—The east bank of the river is here covered with a belt of wood; the latter having a very irregular outline on the east, where it is bounded by a clear field. The line *c c* represents the apparent course of the axis of the tornado: W. west, E. east. The large dots on the several figures show the root ends of the trees, which were chiefly a species of cedar. In all these cases of prostration, part of the roots were still fast in the ground. Course of the tornado east. The approximate positions of the several trees are in many cases slightly changed in the sketch, for the purpose of a distinct exhibition of each.

Note.—This bank of the river is intersected by small ravines with wooded margins, one of which is nearly opposite to chain 5, and another is near chain 13, and which cause most of the irregularity in the wooded outline.

[From the American Journal of Science and Arts, Vol. XLII, No. 2.]

REPLY TO DR. HARE'S OBJECTIONS

TO THE

WHIRLWIND THEORY OF STORMS.

BY W. C. REDFIELD.

An article, entitled "*Objections to Mr. Redfield's Theory of Storms, with some Strictures on his Reasoning;* by Robert Hare, M. D., Prof. of Chem. in the Univ. of Pennsylvania," which appears in the last number of this Journal, and is also found in a modified form in the London, Edinburgh and Dublin Philosophical Magazine for December, 1841, has given occasion for the notes and remarks which follow.

The several series of facts and observations, showing both the rotary and progressive movement of great storms, which I have published, together with those which have also been adduced by Reid, Milne, Dové, and Piddington,* are deemed sufficient to establish the whirlwind character of these storms. In the absence, therefore, of contravening facts of a reliable character, it seems incumbent on an objector to set aside these facts and observations as unfounded and inaccurate, or to show that the results which they appear to establish have been deduced erroneously. This task Dr. Hare has not attempted; and I might therefore have been excused from replying to his

* See this Journal, 20 : 20–40; 25 : 114–121; 31 : 115–130; 35 : 201–223; also, a paper read before the Amer. Phil. Society, 1841, (Trans. N. S. Vol. VII,) and copied into the present volume of this Journal, p. 112–119.

Reid on the Law of Storms, Weale, London, 1838.

Transactions of the Royal Society of Edinburgh, Vol. XIV, p. 467–487.

Poggendorff's Annalen, Jan. 1841, &c.

Piddington's three Memoirs on the Law of Storms in India. Calcutta.

objections and strictures; as these cannot affect the results which it has been my chief aim to establish.

But the observations which I have published extend also to the so-called tornado or water-spout, and with similar results :* while Mr. Espy and Dr. Hare have each in turn advanced his theory of tornadoes and storms, founded on *à priori* reasoning or speculation, and on alleged deductions from phenomena observed. Hence, perhaps, originates this fourth attempt, from one or other of these sources, to discredit the results of my principal inquiries; being, however, the first from Dr. Hare.

Moreover, I have sometimes ventured to offer summary sketches of other meteorological facts or results which seemed to follow from the above mentioned and other developments, which came under notice in pursuing my meteorological inquiries.† These results were thus given, partly as *notifications* and partly because I was not willing it should appear in after years that they had been overlooked in conducting my inquiries. These inceptive statements seem to have occasioned many of the "strictures" and criticisms which I am now to notice.

Dr. Hare says, that my "idea that tornadoes and hurricanes are all whirlwinds, involves some improbabilities," and that it requires that "during every hurricane, there should be blasts of a like degree of strength coinciding with every tangent which can be applied to a circle," and that "thirty-two ships equidistant from the axis of gyration, and from each other, should each have the wind from a different point of the compass with nearly equal force." The only modification he admits "is that resulting from the progressive motion which tends to accelerate the wind" on one side, "and to retard it upon the other."

I could never have imagined that any "idea" of mine necessarily involved the conditions here specified; and if the fact be such, Dr. Hare would have rendered some service by making it manifest. The modification admitted by him, vitally important as it is, shows only *one* of the conditions which would doubtless prevent any such perfect symmetry of results as he demands; to say nothing of the practical error of supposing

* See this Journal, Vol. XLI. (July, 1841,) p. 69–77. Do. Jour. Frank. Institute, Vol. III, third series, p. 40–49.

† See this Journal, 33 : 50–65; also, various incidental remarks and statements in other papers.

that the course of the wind in a whirlwind must coincide with the tangents of a circle. He alleges also, "that as respects any one station, the chances would be extremely unfavorable that the same hurricane should twice proceed from the same quarter." If by this is meant that the changes of wind at any one station in the same gale, are not likely to come back to the same point of the compass from which it had before blown, except by an extraneous force or influence, we shall in this be able to agree. He states further, that "in the course of time it would be felt, at any station, to proceed from many different directions, if not from every point of the compass." The first of these conditions is verified by observation, except as I have shown that the changes in a regular whirlwind storm will not, in the true wind of the gale, be likely to exceed sixteen points of the compass at any one station. It will be difficult, however, for Dr. Hare to show, that the regular changes in a progressive whirlwind storm, as truly exhibited at any fixed station, should run through "*every* point of the compass;" although this may sometimes happen to a ship moving in the storm.

Dr. Hare does not appear to perceive, that the several conditions above referred to are, for the most part, no more predicable of the whirlwind storm, than of the affluent theory of storms which he advocates.

Dr. Hare states, that "the fact that during the same storm, different vessels, variously situated, are found to have the wind in as many different directions, may be explained by the afflux of winds from all quarters to a common focal area, as well as by supposing them involved in a great whirlwind." This might be true, as I have virtually stated elsewhere, provided that the direction of the wind at such vessels was found, at a given time, to be towards such a "focal area;" *which does not happen:* the observed differences of the winds from these centripetal directions being nearly equal to 90°, (or a right angle,) as has been repeatedly shown.*

I have formerly stated that "I have observed in the effects of the New-Brunswick tornado, numerous facts which appear to

* See this Journal, 25 : 116; 31 : 117, 118; 35 : 210–215. Jour. Frank. Institute, 1839, p. 323–336, and p. 363–378. Dové in Poggendorff's Annalen, Jan. 1841, pp. 10, 11, seq.

demonstrate the *whirling* character of this tornado, as well as the *inward* tendency of the vortex at the surface of the ground."* But Dr. Hare thinks, "that the survey of Bache and Espy shows that it would be inconsistent with the facts to suppose such motion, unless as a *contingent* result." Now, without inquiring whether the constant whirling action to which I alluded be a contingent or a necessary result, it is proper to notice, that the great question between us is and has been, *have storms a gyratory character?* To me, the facts established by all the strict observations which have been made and properly stated, proclaim the affirmative. We shall probably find, on a strict examination, that even the surveys of Prof. Bache, though not comprising all the particulars which I deem essential to a right view of the case, may yet be best explained by admitting a general and continued whirlwind action.

Dr. Hare next adduces an imperfect quotation on the law of atmospheric circulation, as depending on the earth's rotation, centrifugal action, &c.; and presumes me to mean, "that the centrifugal force communicated to the air at the equator, causes it to rise and give place to those portions of the atmosphere," from adjacent latitudes, which "have less rotary motion;" and proceeds to comment on this presumption. I beg leave to assure Dr. Hare that he has greatly misapprehended my meaning; and furthermore, that I have never found any evidence of the supposed general ascent of the air from the lower to the upper atmosphere in the equatorial regions.

In my first essay, the prevalence of westerly winds in the higher regions of the atmosphere, was incidentally and partially ascribed to the deflection of the trade winds by mountains. Dr. H. alleges that this explanation harmonizes with the theory of Halley. He adds: "In fact, as the water accumulated by these winds in the Gulf of Mexico, is productive of the Gulf Stream, is it not reasonable that there should be an aërial accumulation and current, corresponding with that of the aqueous current above mentioned?" This comes nearer to my views of the *course* of circulation in the atmosphere, but does not so well accord with the common theory of the trade winds. That the alleged accumulation of water in the Gulf of Mexico by the

* See this Journal, 35 : 207.

trade winds, is the main cause of the Gulf Stream, Dr. Hare may perhaps show hereafter. The contrary would appear to have been settled by the levelings which have already been obtained.

Dr. Hare intimates that the trade winds "cannot be explained without the agency of temperature;" he alleges also that I "reject the influence of heat;" and proceeds to quote a paragraph from which, as well as others, he infers that I "consider gravitation, uninfluenced by heat or electricity, mainly the cause of atmospheric currents;" and he inquires, "what other effect could gravitation have, in the absence of calorific and electrical reäction, unless that of producing a state of inert quiescence?" He also speaks of my treating momentum as "the antagonist of gravitation."—[p. 141–142, ¶ 5–8.]

Now to all this, I answer:—1. That, to my apprehension, the essential features of the trade winds can be best explained without assigning the agency of temperature as the chief moving power.—2. It is an error to say, that I reject the influence of heat.—3. I consider the influences of momentum, centrifugal force, and centripetal action, as being comprised in the laws of gravitation.—4. It is true that I do not consider "electricity" as a general *cause* of atmospheric currents; for the reason that, so far as I know, this has never been shown.—5. That the only effect of gravitation, without calorific or electrical reäction, would be to produce "a state of inert quiescence" in the atmosphere of a moving and rotative planet like our own, is to me inconceivable.—6. I have never considered nor asserted "momentum" to be "the antagonist of gravitation."—In the paragraph which is quoted by Dr. Hare, I had suggested *the courses of great storms as indicating the law of circulation in our atmosphere, and which I deemed to be founded mainly on the laws gravitation.* By some mistake, he has given the phrase "*causes* of great storms," instead of *courses;* and proceeding on this error, he calls it a summing up of the "causes" of atmospheric currents: although he alleges at the same time, that I here admit but *one* cause.

It is next asked, "If the minuteness of the altitude of the atmosphere, in comparison with its horizontal extent, be an objection to any available currents being induced by calorific

rarefaction," as he states I have alleged, "wherefore should not momentum or any other cause *diminishing or counteracting the influence of gravity*, be on the same account equally inefficient?" To this I answer:—1. Momentum, and the other modifications of the gravitating power, are of far greater magnitude and force than the influence of the mere difference of temperature in the several geographical or climatorial zones.—2. The main tendency or result of this greater force is to produce *horizontal*, not vertical motion.—3. The words which I have italicized above, show only the misapprehension already corrected, and which appears to run through the strictures which I am noticing. By "available currents," as above quoted, I here understand the great currents of the atmosphere, constituting the trade winds, &c.

In succeeding paragraphs [¶ 10–12] Dr. H. criticises the terms by which I have endeavored to point out, that a whirling or rotative movement *is the only known cause of a violent and destructive force in winds or tempests;* as the last clause of the paragraph quoted by him should read. There is little probability that my meaning has been misunderstood by general readers; and it appears afterwards to have been divined by Dr. Hare himself.

After a short comment on the functions of gravitation, Dr. H. inquires—"But if neither gravity, nor calorific expansion, nor electricity, be the cause of winds, by what are they produced?" I answer:—1. According to my apprehension, the gravity which induces a nearly equal "distribution of the atmosphere over the surface of the globe," may and does, in its modified influences, constitute the main basis of winds and storms.—2. That calorific expansion is *a* "cause of winds" is universally admitted; but that it is the *chief* cause, I cannot perceive.—3. If "elecricity" be the cause of winds, it seems incumbent on Dr. H. to show it.

For my own part, having never attempted to write out or establish a *theory* of the winds, in the common acceptation of the term, nor yet, of the origin or first cause of storms, I have no occasion to go into these inquiries any further than relates to my present purpose. It is true that I entertain some definite views on these points, which have resulted from observation

and inquiry; but the choice of time and occasion for their more full development, and also of the evidence on which they rest, belongs to myself rather than to another. I do not intend being diverted from my ordinary business, or from the results of direct observations in storms, by engaging in a controversial discussion of those general views of the alleged cause of winds, and of the physico-mechanics of the atmosphere, which now prevail; and which are held by men of the highest attainments in physical science. And in relation to storms, I have long held the proper inquiry to be, *What are storms?* and not, *How are storms produced?* as has been well expressed by another. It is only when the former of these inquiries is solved, that we can enter advantageously upon the latter.

I have stated, incidentally, that all fluid matter has a tendency to run into whirls or circuits, when subject to the influence of unequal or opposing forces, &c. Dr. Hare says, that "if this were true, evidently whirlpools or vortices of some kind, ought to be as frequent in the ocean, as agreeably to my observation, they are found to be in the atmosphere." That "the aqueous Gulf Stream, resulting from the impetus of the trade winds, ought to produce as many vortices in its course as the aërial currents derived from the same source;" and he adds, "there are few vortices or whirlpools in the ocean," for reasons which he has chosen to assign.—[¶ 14–16.]

Now the alleging that aqueous currents have an equal tendency, with aërial ones, to run into "vortices," belongs to Dr. H., not to me. In the ocean, we can but partially observe the upper surface of superficial currents, moving apparently unobstructed on the more quiescent waters beneath, and with the relative equality of motion in the parts generally maintained. I see not how the unimpeded movements of this denser and nearly non-elastic fluid are to produce vortices equal in number or magnitude to those which occur in the inferior layers of an elastic aërial current, moving on or near the surface of the earth, over obstructions and inequalities, and with other disturbing conditions almost innumerable. Of Dr. Hare's views of aqueous vortices it is unnecessary to speak; but there are mariners, if I remember their statements aright, who can give him an account of the frequency of ocean or Gulf Stream vortices, somewhat

different from that which he advances. Whenever a stream or current of water of moderate depth moves over an unequal bottom, there is found no lack of vortices, of various forms and dimensions, some of which exhibit both upward and downward movements, often of considerable velocity.

Dr. H. doubts if a whirlpool ever takes place without a centripetal force resulting from a *vacuity*. I see not how this doubt can militate against my views of vortical action; but I have myself seen many hundreds of such whirlpools or vortices, and have occasionally watched their developments with much interest.

After commenting on certain arbitrary conditions "of opposing or unequal forces," Dr. Hare desires to be informed how "unequal and opposing forces" are generated in the atmosphere; producing sometimes whirlwinds of unmeasured violence.—[¶ 17–18.] It may be readily seen, that aërial currents of unequal density, temperature, and velocity, superimposed one upon another, and all moving over a surface of unequal character and with frequent elevations, and subject also to the influence of adjacent currents, must often move *unequally*, and in *unconformable directions;* thus unavoidably running, to some extent, into vortices, eddies, or circuits, of various magnitudes and activity; some of which may occasionally become extended, and spin on an upright and moving axis with that violent and continued action which characterizes the tornado or water-spout. Indeed, it must be obvious that uniformly direct lines of motion belong not to our atmosphere or system. But, as before observed, I have here no special concern with the origin of these or other vortices; the simple fact of their existence being all that is necessary for me to maintain.

Dr. Hare then proceeds to state, that in former papers on the causes of tornadoes, he has adduced facts and arguments "tending to prove that the proximate cause of the phenomena of a tornado is an ascending current of air, and the afflux of wind from all points of the compass to supply the deficiency thus created." He also states, that "in this mode of viewing the phenomena, no difference of opinion exists between Espy and himself, however they may differ respecting the cause of the diminution of atmospheric pressure," &c.—[¶ 19–20.]

I have no desire to offer strictures upon the views of a respected professor of science; but it seems proper here to inquire how an ascending current of air can thus be obtained, and whether this *effect*, which perhaps may be due only to an excess of lateral and subjacent pressure, on the exterior of the tornado, be not here adduced as the cause of the effect?

Dr. Hare has been "led to consider gyration as a casual and not an *essential feature*" in tornadoes, and he adduces the dislocation and partial turning of a chimney-top on its base, in the New-Brunswick tornado, as being due to a *local* whirl within the body of the tornado, and proving that in tornadoes and hurricanes there are local whirls.—[p. 144, ¶ 21.]

I have long since ascertained that local whirlwinds are not of very rare occurrence in great whirlwind storms; the New Brunswick tornado itself having been one of several violent local whirlwinds which occurred within the limits of a somewhat remarkable storm of the above character. This tornado also sent off a duplicate vortex or whirl not long after its passing the Raritan; the path and violent effects of both whirlwinds having been distinctly traced on a field of unripe grain; the smaller one branching off to the right of the main track, where, after causing some prostrations, it passed into the Raritan marshes, crossed a bend of the river, and was no more seen. But the whirling motion, so far from being only of "casual" and limited occurrence, appears to be a constant attribute of the tornado; although not always exhibited with uniform intensity and effect in its path, owing, apparently, to the frequent rising or narrowing of the vortex, and perhaps other causes.

In his paper, as found in the English Journal, Dr. Hare says —"A fact which is admitted by Mr. Redfield, was considered by Espy and Bache, as well as myself, to be irreconcilable with the idea that a general whirling motion is essential to tornadoes. I allude to the circumstance, that when several trees were prostrated one upon the other, the uppermost was found to have fallen with the top directed towards the point towards which the meteor was moving; while the direction in which the lowermost trees were found to have fallen indicated that they were overthrown by a force in a direction precisely the opposite of

that which had operated upon those above mentioned."—[*Phil. Mag.* ¶ 24.]

It is an error to allege that I have "admitted" a fact such as is here stated. On the contrary, in careful explorations made on foot, through an aggregate extent of more than fifty miles of the tracks of various tornadoes, I have never met with such "a fact," or combination of facts, as Dr. Hare describes. In all the cases that I have met with in which trees have fallen one upon another, if their tops pointed in opposite or nearly opposite directions, these directions *have never been parallel to the course pursued by the tornado;* but always in directions more or less transverse to the same: and I consider the opposing allegation as one of the chief errors of my opponents.

The trees which have fallen in directions which are more or less *backward* from the course pursued by the tornado, are almost invariably found *on the left side of the track, exterior to the line of its axis:* But few of these point directly backward, and still fewer can be found near the axis, as the hypothesis of my opponents requires. Of the trees found with their tops pointing *directly forward*, or nearly so, a small number have been seen on or near *the right margin of the track*, with appearances which showed them to lie as they first fell; a fact which seems equally fatal to their hypothesis. Some trees, along and near the line of the axis, are, however, found pointing in this onward direction, and much stress has been laid on *this* fact by one of my opponents: But it appears, on examination, that in all these cases the trees *have been torn or twisted from the transverse position in which they first fell;* owing, as I infer, to the more violent force exhibited at and immediately behind the centre of the whirl, or at the point which may not inaptly be termed the heel of the vortex.*

It is true, however, as I *have* "admitted," that when trees are found to have fallen one upon another, the top of the uppermost tree points in a direction *more onward* than the one beneath; as is seen by the diagrams and schedules of Prof. Bache, and as may be inferred, perhaps, from the sketches given by Professors Olmsted and Loomis:† And it is *equally* true, that this fact no

* See this Journal, July, 1841, pp. 69–79.

† See this Journal, Vol. XXXIII, p. 369; Vol. XXXVII, p. 343.

more favors the hypothesis of a directly inward motion, than that of a whirlwind; but, as an abstract deduction, is "reconcilable" with either. The proper generalization of this class of facts I attempted to give in my paper on the New-Brunswick tornado; which is, 'that the uppermost, or last fallen of these trees, points *most* [or more] nearly to the course pursued by the tornado;' i. e. more nearly than the underlying tree which first fell; divergence from the course of the tornado being still a marked feature of these overlying prostrations.

I have never found a directly backward prostration on the line of the centre or axis of the tornado. This, as well as the above mentioned facts, will be found sufficiently "irreconcilable" with a direct "afflux of the wind from all points of the compass," 'in a central and non-whirling course,' "towards a common focal area."

In the same Journal, Dr. Hare says he "cannot understand how the opposite forces belonging respectively to the different sides of the whirlwind, can be made to bear successively *upon one spot*, so as to cause trees to fall in diametrically opposite directions."—[*Phil. Mag.* ¶ 25.] Neither can I understand this, if each of these "opposite directions" *be parallel to the course of the tornado*, as is alleged by Dr. Hare, in the passage last noticed.

Dr. Hare next tells us—"A fact, irreconcilable with a general whirling motion, has been recorded by Messrs. Espy and Bache. A frame building was so situated as to be protected by another edifice in one direction from the suction of the tornado, and yet was exposed to its influence as it advanced, and as it moved away. Hence, two of the four posts on which the frame rested were so impelled by the wind as to make furrows in the ground, of which one was nearly at right angles to the other. Evidently such furrows could not rise from the transient tangential impulse of a whirlwind."—[pp. 144, 145, ¶ 22.]

In the English Journal, Dr. Hare alleges that one of the four posts on which the building was supported, "*was first moved towards the tornado*, as it advanced:" while Prof. Bache shows us that the tornado advanced from south 80° west, to north 80° east; and that the posts were first moved "*to the west of north.*"

But on what grounds this "fact" is pronounced "irreconcilable with a general whirling motion," I am wholly unable to perceive. For, had he closely examined the whole case, he would hardly have failed to see that the movements of this building, as described by Prof. Bache, are fully "reconcilable" to an involute "whirling motion," such as I allege to be characteristic of these tornadoes; and that there was no necessity for resorting to the gratuitous hypothesis of its being "protected by another edifice in one direction," or even that of "the suction of the tornado."

If a whirlwind figure having a diameter of three or four hundred yards by the scale of Prof. Bache's figure, [Plate III, fig. 3,]* be drawn on tracing paper, with involute whirling lines representing, horizontally, the course of the wind from the exterior to the interior of the tornado, and if the centre or axis of this figure be passed from west to east along the line pursued by the axis of the tornado as indicated on the plate, revolving at the same time to the left with a velocity greatly exceeding its advancing motion, it may be seen that the wind of the whirl will be indicated as beginning at this building from nearly south, i. e. moving "to the west of north" nearly, or in the general direction of the first furrows in the ground. It will also be seen, that the wind of the whirl, changing by southwest, and having its gyration quickened near the centre, would, immediately after the passing of its axis, exhibit its greatest force from the western quarter, corresponding to the second movement of the posts in the ground; the wind veering from thence towards the northwest as the tornado passed away: thus showing two directions of wind which sufficiently coincide with the first movements of the posts of the building "to the west of north," and subsequently "to the eastward," or "nearly at right angles" to its first course; according to the descriptions and plan of Prof. Bache, who gives the course of the axis as "east 10° N.," the building being to the southward or on the right of this line.

I say nothing here of the alleged protection by "an edifice" which after the first moment, according to the hypothesis of motion adopted by Messrs. Espy and Hare, was constantly

* See Jour. of Franklin Institute, Vol. III, third series, 1841, pp. 273 and 276.

more or less to *leeward* of the building so protected. By applying to Prof. B.'s plan, as before, a compass card, moved from west to east without revolving, we shall find their wind to commence nearly at east, passing thence through south to southwest, and possibly to west-southwest, near which it would terminate. Thus, the first effects of the wind, when, even upon the hypothesis of "suction," the building was unprotected, could not produce the first motion in the direction "to the west of north," which may perhaps be fairly taken at 5° or 10° west of north; and the wind, on their hypothesis, would hardly appear to have reached a point which could produce the second movement "to the east."

I have been thus particular in this examination, because the case thus alleged by Dr. Hare is a further specimen of the erroneous inductions which have been made and relied on by my opponents. In examining the plans referred to, it should be observed, that the sketch of prostrations in the orchard, which is included in fig. 3, is evidently on a more reduced scale than that given in the plan of the building; otherwise, the buildings must be of size sufficient nearly to have covered the orchard. This change of scale may cause some confusion unless particularly noticed.

That the velocity and consequent force of the whirling movement of the tornado is maintained by the direct *pressure* of the surrounding atmosphere, rather than by the "suction" alleged by Dr. H., I can readily conceive; but that the "impulse of a whirlwind" of this character is generally found to be "tangential" to its axis, which he seems to consider a necessary condition, I do not admit.

Dr. Hare appears to concede, that my survey of this tornado shows effects which accord with whirlwind action; but he seems desirous of limiting this admission to the prostration of "certain trees," and alleges that this survey "does not demonstrate gyration to be an essential feature of tornadoes," and that "it is sufficiently accounted for by considering it as a fortuitous consequence of the conflux of currents rushing into a space partially exhausted."—[¶ 23.]

Now I cannot but think, that readers who have no theory to support, will view the results of my survey in a very different

light. Dr. Hare omits to mention, that the survey comprised the entire breadth of the visible track, at perhaps its broadest place; that it was intended to include every tree prostrated within its limits; that it essentially agrees with the main features of the more partial surveys of Prof. Bache; that I have shown by clear inductions from all the prostrations in the survey that the whirling motion was one general effect, comprising the entire width of the track; that the tornado must have arrived at this ground in nearly its most perfect action, having just left the surface of the Raritan river; that the axis of prostration was not found in the centre of the track, but nearest its left margin; that the main rotation was wholly to the left or in one constant direction; and, that the leading features of the prostration found in this survey have also been observed as constantly occurring in the tracks of many other tornadoes.*

I may add, that in a careful exploration of the track of this tornado for several miles, I found nothing to contravene the results presented in my published survey; the general features of the prostration being greatly analogous to those which I have given.

Dr. Hare thinks it singular, that I should have declined noticing the "insuperable difficulties" of the hypothesis of 'a central and non-whirling course in the wind of the tornado,' to which I have alluded in bringing forward facts and inductions which seem to contravene this hypothesis. He states, also, that "the advocates of the disputed hypothesis are not aware of any such difficulties," and intimates the impropriety of the allusion "without naming the facts and arguments" which justify it.—[¶ 24.]

I considered it more proper, however, to rely solely on the survey and inductions which I then presented; as these appear sufficient to set aside, not only the hypothesis itself, but also some of the chief deductions from the phenomena of this tornado which have been put forth and relied on by Mr. Espy and Dr. Hare.† Besides, I had no wish to assume a controversial atti-

* See this Journal, 41 : 69–77. Do. Jour. Franklin Institute, Vol. II, third series, p. 40–49.

† See Journal of the Franklin Institute, Vol. XX, new series, 1837, p. 56–61; also. Vol. II, third series, 1841, p. 356–359.

tude, in assailing by argument an hypothesis which virtually discards the observations of mankind in all past ages down to the year 1835. The testimonials of these observations appear in the names and terms applied by all people in all languages to this small but violent class of storms. "The facts" demanded, I had supposed, were furnished on that occasion in sufficient numbers.

Dr. Hare next adduces "the statement of a most respectable witness, that while the tornado at Providence was crossing the river, the water which had risen up, as if boiling, within a circle of about three hundred feet, subsided as often as a flash of lightning took place;" which he alleges to be a "fact which is utterly irreconcilable with Mr. Redfield's rotary theory." He adds: "Now, supposing the water to have risen by a deficit of pressure resulting from the centrifugal force of a whirl, how could an electric discharge cause it to subside?"—[¶ 25.]

For the supposition here made, as well as for "the water which had risen up," Dr. H. seems alone accountable; as his witness, Mr. Allen, speaks only of "the effervescence produced by the tornado in the water" having "perceptibly abated." The water he states to have been "in commotion like that in a huge boiling caldron;" but *that which rose up* from the surface, he describes as "misty vapors resembling steam," which "after the flash, seemed sensibly to diminish for a moment."* I cannot perceive that the fact thus alleged has the least unfavorable bearing upon my views of rotative action. Therefore, without considering the optical effect which may result from a flash of lightning, or the immediate conversion of clouded vapor into rain, which oftentimes suddenly follows, I will only state, that another competent observer, who was very near this whirlwind when it left the western shore and who watched its progress across the river, has described to me the appearance of the cloudy sprays or mists blown from the surface of the water, and which filled the lower extremity of the tornado, but he has mentioned no sudden disappearances of the same. He did, however, observe the *whirling action* of the tornado with great distinctness, both when it first entered upon the river, and in its

* See this Journal, Vol. XXXVIII, p. 76.

effects upon the sails and position of a schooner with which it came in contact; and likewise, as exhibited by the circling or whirling directions of the various objects carried into the air, as it came off the high grounds on its approach to the river. The highly intelligent eye-witness of my opponent also describes "the misty vapors" as "*entering the* WHIRLING VORTEX;" thus showing from his own observation a fact which fully supports my views, and is fatal to the objections and hypothesis of motion set forth by Dr. Hare. Moreover, there were decisive memorials of a general whirling action found along the path of this tornado.

Dr. Hare chooses also to say, "that the explanation which Mr. Redfield dignifies with the title of his 'theory of rotary storms,' amounts to nothing more than this, that certain imaginary nondescript unequal and opposing forces produce atmospheric gyrations; that these gyrations, by their consequent centrifugal force, create about the axis of motion a deficit of pressure, and hence the awful and destructive violence displayed by tornadoes and hurricanes."—"I cannot give to this alleged theory the smallest importance, while the unequal and opposing forces, on which it is built, exist only in the imagination of an author who disclaims the agency either of heat or electricity." —[p. 145, ¶ 26–27.]

The recital of this passage appears necessary on account of the gross error into which Dr. H. has here fallen. I have never attempted to dignify any "explanation," induction, sketch, or essay, "with the title" of my "theory of rotary storms." It must, at least, have been a mistake of person. I have little fondness for theory-making; and as little respect for hypotheses of winds or storms, other than those which result directly from sufficient and reliable observations. Neither have I disclaimed "the agency of heat," as already stated; but it may have been my offense to have disclaimed "electricity" as a known cause of storms. My cursory explanations of the action of a whirlwind or tornado, even as shown up by Dr. Hare, are, in my view, better suited to the observed facts of the case than any which he or Mr. Espy has offered.

I do not solicit for my views even that "smallest importance" which is denied them in the mind of my critic; but the attention

with which he has treated them, both here and abroad, does not appear to agree well with the disavowment. With the facts before him which are shown in my survey of the tornado, and also with the numerous observations made in great storms, which I have published, it is both vain and absurd to pretend that my views of their rotation are founded only in imagination. I am not conscious of having "built" or indicated any "theory," views, suggestions, or explanations of storms or whirlwinds which have not been based on observations of my own and facts otherwise ascertained, sufficient, in my view, to warrant them; the 'unequal and opposing forces' even included: although, I have not always urged these facts upon the attention of my readers; having, not unfrequently, reserved them for more appropriate occasions. Hence, my alleged proofs have been chiefly confined to the progressive course and rotative action developed in storms; which last, strangely enough, has been so pertinaciously denied by Mr. Espy, and now by Dr. Hare.

My opponent next attempts to show, "that any deficit of pressure about the axis" of a whirlwind, "consequent to the resulting centrifugal force, could only cause in the atmosphere a descending current, while it could not tend in the slightest degree to carry solids or liquids aloft."—[p. 146.] I was also surprised to find this hypothetical downward current in the midst of a whirlwind alleged as a necessary condition, on former occasions, by Mr. Espy. If the allegation be true, it must be easy to show that the ascending currents in chimneys should become inverted; for, so far as simple gravitation is concerned, it can make little difference whether the rarefaction be mechanical or calorific.

But the ascending effects in the interior of a whirlwind have been too often witnessed by myself and others to require discussion. Indeed, it would almost seem that the objectors had been precluded from all opportunities for correct observation. There are numerous cases, however, in which the movements of the objects elevated cannot be seen in the central and lower parts of the whirlwind; owing, as I have had good occasion to know, to the great angular velocity of the central gyrations.

Dr. Hare appears to suppose, that gyration in a revolving fluid mass will not quicken as it approaches the centre, unless

as resulting from a centripetal force "caused by suction at the axis."

A constant centripetal *force* I have already recognized on this as well as former occasions. But this by no means requires or produces a direct centripetal *course* in the moving air which yields to its influence. But in the cause assigned for this force, as well as in the specific directions of the movements produced, we differ essentially. So far from ascribing this quickened gyration to the "suction" alleged by Dr. Hare, I know of no such power in the uninclosed atmosphere; conceiving, that neither rarefaction nor any other known cause can here occasion "suction," according to the common use of this term. Air, whether rarefied or not, can never ascend but in obedience to a *pressure* or *force* sufficient to exceed both its own weight and that of all the atmosphere which lies immediately above it, or in the immediate direction or locality of its motion. This erroneous hypothesis of "suction," in some form or other, appears to lie at the bottom of the various speculations and inductions of my opponents.

In noticing the spirally involute and quickening motion which I allege as observable in 'all narrow and violent vortices,' Dr. H. gives an erroneous reference for his quotation; and the latter seems also to be somewhat inaccurate. I do not see that his speculations on this quickened motion 'towards the centre or axis of the whirl,' can affect either my views or the disputed fact of gyration; and they are sufficiently answered by observations published in my first paper,* as well as by the remarks made above on centripetal force.

Dr. Hare thinks that, so far as my observations show the quickening of the whirling motion towards the centre of the tornado, they tend to confirm the views of my opponents and to refute those which I uphold. To me it appears that this is an entire abandonment of his ground. It is the general fact of gyration which I am chiefly concerned to uphold, and which has been combated by him and his predecessor in this controversy. I dispute with no one as to how it may be produced. Should better explanations of this fact than mine be offered, they will

* See this Journal, Vol. XX, p. 45–46.

be cheerfully adopted. In the mean time, I shall adhere to my observations and opinions, rather than to the hypotheses and speculations of my opponents.

Dr. Hare thinks, "that any theory of storms which overlooks the part performed by electricity must be extremely defective." I do not perceive that the part performed by electricity in a gale of wind, squall, tornado, or other storm, ever constitutes an essential feature of the same: but the part so performed appears to me to be only incidental and subordinate to the action and main effects of the storm. Electricity is not wind, nor water, nor vapor; but an imponderable matter or effect, which is not known to exert any constant mechanical force or action upon the effective currents of the atmosphere. "Thunder and lightning, and convective discharge," are but momentary or transient exhibitions of electricity, producing no visible effects upon these currents; whatever may be their agency in restoring the disturbed equilibrium of the different atmospheric elements. The electricity developed by a steam boiler is not considered as producing the steam or its jet, or the condensation of the latter; but is itself produced by these. Even were it shown that a stream of electricity was constantly developed between the rarefied column of a moving tornado and the surface beneath, I cannot see how this could be assumed as the *cause* rather than the *effect* of the local rarefaction. If the part which electricity performs in a storm be essential, or controlling, its functions ought to be distinctly pointed out.

I would humbly suggest that the old practice of forming or inventing theories or schemes of action for the powers of nature ought to be mainly abandoned. The Wernerian and Huttonian theories are well remembered; and how small would have been the progress of the science to which they relate, had its cultivators continued to exhibit only the spirit and philosophy of the early advocates of these theories; and how much less, if guided by a philosophy so speculative and untenable as that of the affluent and up-moving hypothesis of winds and storms! More strict and extended observations and inquiry, with greater caution in the adoption of hypotheses, whether old or new, would, in my opinion, tend greatly to the advance of meteorological science.

Observation, rather than "lucubration," has been my employment when exempted from other duties: and if the results of observation do not accord with the "lucubrations" of Mr. Espy and Dr. Hare, I conceive that I am in no degree responsible for the difficulties of their position.

New-York, January 13, 1842.

From the American Journal of Science and Arts, Vol. XLIII, *No.* 2.

REPLY

TO

DR. HARE'S FURTHER OBJECTIONS

RELATING TO

WHIRLWIND STORMS;

WITH SOME EVIDENCE OF THE WHIRLING ACTION OF THE PROVIDENCE TORNADO OF AUGUST, 1838.

BY

W. C. REDFIELD.

In my Reply to the objections and strictures of Dr. Hare,* I attempted to show that these could have no weight or efficacy in disproving the whirlwind character of violent storms and tornadoes, and that good evidence of whirlwind action in the tornado of New Brunswick was afforded by those very facts which he had set forth as disproving its rotation.

Having corrected the errors into which my opponent had fallen, I also referred to additional proofs of rotation which had been afforded by this tornado. This was deemed sufficient in replying to Dr. Hare, who had chosen to "enter the lists" as a disputant, in support of his own and Mr. Espy's notion of the centripetal course of the wind in storms; particularly as this New Brunswick case had from the first been greatly relied on by these two writers, as supporting their peculiar theories.

* This Journal, Vol. XLII, April, 1842.

At the same time, however, I possessed in my field notes abundant evidence of the constant rotative action of other tornadoes; and diagrams illustrating some of the traces of these storms had long been prepared and cut in wood; but I saw no defect in the evidence of rotation already exhibited, that could render the publication of these necessary.

Among the tornadoes the traces of which I had thus prepared to illustrate, was that which passed near Providence in August, 1838, of which some account has been given by Dr. Hare;* and as the desire to obtain favor for his own electrical hypothesis may have induced him to appear as my opponent, I propose, on this occasion, to exhibit what I deem to be conclusive evidence of the whirling character of his Providence tornado.

But before proceeding with this evidence, it may be proper to take some notice of his rejoinder, which, under the title of "additional objections," appears in the last number of this Journal. [This Vol. p. 122.] The friends of strict scientific inquiry have probably been disappointed in this paper; for he seems here to have abandoned the main question at issue, even as staked upon his own allegations, and to have undertaken a *petite guerre* of criticisms, which have little if any relation to the evidence on which the issue depends.

Dr. Hare says he had "endeavored to point out various errors and inconsistencies in the theory of storms proposed by me, or in the reasoning and assumed scientific principles on which that theory had been advanced." Now it has never been my purpose to "propose" or "advance" a "theory of storms" founded on "reasoning and assumed scientific principles." This has, indeed, been attempted by others; with what success, is best known to attentive inquirers. Whereas, I have mainly endeavored to exhibit a matter-of-fact view of the actual phenomena of storms, so far as relates to their progress, the violent rotative winds which they exhibit, and their immediate effects on the barometer. That I should assume, therefore, the correction and refutation of all the several allegations and errors contained in his second attack, will hardly be expected. Perhaps the following comments may suffice.

Humble as are the claims on which my "meteorological reputation" rests, I do not perceive that it depends so much on the

* This Journal, 1840, Vol. xxxviii, p. 73–77.

particular "course" which my opponent has taken, as he seems to imagine, (par. 41); nor that it is likely to be materially affected by his writings on meteorology. But should the fact prove otherwise, I will endeavor to bear it with becoming philosophy.

Referring to the approval of my views by men of science, he says: "It strikes me, however, that a fault now prevails which is the opposite of that which Bacon has been applauded for correcting. Instead of the extreme of entertaining plausible theories having no adequate foundation in observation or experiment, some men of science of the present time are prone to lend a favorable ear to any hypothesis, however in itself absurd, provided it be *associated with observations.*" Now, as before stated, it is "*observations*" and their results which I have mainly endeavored to promulgate: and in relation to storms, if it has been attempted to associate "hypothesis," whether "absurd" or otherwise, "with observations," it would appear to have been by my opponents; and yet the seeming dislike to "observations" may be somewhat unfavorable to this construction.—That my "Reply" was properly "so called," may be inferred from the evasive "course," as well as title of his rejoinder; and it appears likewise from the tone and character of the succeeding paragraphs as well as from the closing sentences of that under notice, where "reliable facts and observations,"—"established character of storms"—and "the whole modern meteorological school" are quoted in a form of words and connexion which I did not use.

It appears to be difficult for Dr. Hare to give accurate quotations, unless in the cases in which he ventures to give a reference. Thus, in par. 43, he succeeds in adducing more correctly than in his previous quotations, my remark that "the grand error into which the whole school of meteorologists appear to have fallen, consists in ascribing to heat and rarefaction the origin and support of the great atmospheric currents," &c. This is a question of atmospheric dynamics which I believe has not been sufficiently examined by any writer. The remark quoted was made incidentally, on the occasion of Mr. Espy's first attempt to discredit certain facts or results which I had stated;* and if the pretended accusation and "denunciation" (!) of the meteorologists, which is now charged upon me, can be made to cover Dr. Hare's seem-

* This Journal, Vol. xxviii, p. 316.

ing discomfiture, perhaps I need not complain. By the ill chosen phrase "whole school," was simply meant, all meteorologists to whose writings I had obtained access. It was an inadvertent form of expression, not particularly noticed by me till after publication, and has probably given more pain to myself than to any one else.

In adducing the quotation which refers to Sir John Herschel, my opponent chooses to omit the preceding sentence, which notices his recognition of *the influence of the earth's rotation on the general winds;* this being the very cause which I then ventured to suggest as the most influential in their production. That Sir John Herschel has not thought himself accused and denounced in any of my remarks, I have good reason to believe.

It may be well to inform my opponent that I am not one who has "forgotten" that the aqueous ocean of the globe, no less than the superincumbent atmosphere, is subject "to the gravitating power" and the influence of "the rotary and orbitual motions of our planet:"* But does he mean to maintain that these influences must produce aqueous movements of equal velocity with those of the same influences in the atmosphere—even as apart from the question of gyration? At a proper time there can be shown him, not "torrents in the ocean," but a system of *currents* in the several oceans, which fully exemplify the great physical truth which he has volunteered to aid me in asserting. [Par. 45.] Nor need he apprehend any conflict on my part with the views of observant geologists: While his "perfect equilibrium" will be found to preclude a "perfect" repose, either of the aerial or aqueous coverings of the planet.

I had noticed Dr. Hare's error in alleging that I reject the influence of heat. In repeating this allegation, he now intimates that "It is very possible that his opinions may have changed since he read my "*objections;*" but that he DID REJECT THE INFLUENCE OF HEAT† when the preceding and following opinions were published must be quite evident." And he then adduces the fragment of a sentence, "*Were it possible to preserve,*"

* See this Journal, Vol. xxv, p. 131, also this volume, p. 152.

† In cases of quotation, where it is proper to notice the bearing of particular words or phrases, I adduce these in small capitals, as above; but have not felt myself at liberty to follow the example of Dr. Hare, in italicising sentences and fragmentary quotations, as if they had been put forth in the same emphatic forms by the writer.

&c. [par. 46.] Now whether this passage be most remarkable for the self-complacency or the pertinacious unfairness which it exhibits, I shall leave unprejudiced readers to determine. That my opinions have not changed since I read Dr. Hare's "*objections*," the following quotation may serve to show; and as it is a portion of the very paragraph from which he has here quoted, and part of the same article in which the alleged "denunciation" of the meteorologists occurs, it could hardly have escaped his eye or memory. I said, (this Journal for 1835, Vol. XXVIII, p. 317,) "I 'freely ADMIT that HEAT IS OFTEN AN EXCITING as well as modify-'ing CAUSE of *local* WINDS, and other phenomena, and that it HAS 'an incidental or subordinate ACTION (though not such as is usu-'ally assigned) in the organization and DEVELOPMENT OF STORMS, 'and that, in certain circumstances, IT INFLUENCES the interposi-'tions of the moving strata of the atmosphere. Its greatest DIRECT 'INFLUENCE is probably EXHIBITED in what are called LAND AND 'SEA BREEZES, or in the DIURNAL MODIFICATIONS which are EXHIB-'ITED by regular and GENERAL WINDS. But, so far from being the 'great prime mover of the atmospheric currents, either in produ-'cing a supposed primary north and south current, or in any other 'manner, I entertain no doubt, that if it were 'possible to pre-'serve [as Dr. H. then inaccurately quotes] the atmosphere at a 'uniform temperature over the whole surface of the globe, the 'general winds could not be less brisk, but would become more 'constant and uniform than ever.'" And with all this before him, he reasserts that I rejected THE INFLUENCE OF HEAT! If greater injustice has been manifested in any scientific discussion of the present century, it has not fallen under my notice.

But as I may have been more frequently misunderstood on the subject of the action of heat and rarefaction than perhaps any other, I will avail myself of this opportunity to say, that in my first paper (this Journal, Vol. XX, p. 18) I had quoted from Dr. Hare a few sentences which, so far as they went, expressed my notions then, and which I have never yet found any reason to change; and I concluded what I said then upon the subject of heat in these words: "To create in the midst of these equable 'winds or elsewhere, by the aid of rarefaction, a fanciful vacuum 'into which the atmosphere from a distance of many miles, and 'even many hundreds of miles, is to rush with all the fury of a 'storm, is to do violence to the established principles of natural

'science. To ascribe such effects to such a cause, is no better 'warranted than to refer all storms to the direct influence of elec-'tricity and magnetism."—Can it be that this summary rejection of "the influence" of "electricity" has occasioned the infliction of the "Objections," "Strictures," and "Additional Objections?"

It seems to dissatisfy Dr. Hare, that I should have stated the proper inquiry to be *What are storms?* and not *How are storms produced?* He asks, "suppose that before ascertaining *how* fire is produced, chemists had waited for an answer to the question, *what is fire*, how much had science been retarded?" [47.] But, waiving the lack of analogy between fire and storms, suppose that in treating of fire, one philosopher should mean by it the heat of combustion; another the heat and smoke, maintaining that the fire depended on the latter; while a third should view it as comprising both these, together with all the effects produced in the surrounding air: would not the proper inquiry then be, *What is fire?*—It appears evident that the laws and phenomena of storms must be first ascertained and established, ere we can successfully investigate their origin or primary causes. And this principle, I trust, has hitherto guided my inquiries.

Dr. Hare appears unwilling to relinquish the grateful task of rendering obnoxious the phrases "grand error" and "school of meteorologists;" which he honors with oft repeated notice. He speaks also of "an endless controversy,"—in which he has chosen to volunteer, and which he prefers to carry on by criticisms instead of abiding the issues of fact, even when these have been presented by himself. He says, To follow me "in detail through all the misunderstandings which have arisen, and which would inevitably arise during a continued controversy, would be an Ixion task." It may be, that grace to acknowledge "the misunderstandings" which the controversy had brought to light, would have tended greatly to shorten its duration.

In paragraphs 49 to 52 Dr. Hare has expended his labors on some superfluous suggestions in my earliest paper, which, more than three years since were virtually withdrawn, and the public notified of their relinquishment;* but which, after all, seem at

* See note prefixed to my article on hurricanes, Vol. xxxv, p. 201 of this Journal. Also in Nautical Magazine for January, 1839.

this time to be held in more favor by Dr. H. than by myself. In par. 51, with characteristic fairness, he has joined a passage from the same paper to another from a subsequent paper, and has adduced it, with a formal reference, as a continuous quotation from the latter. I see little advantage, however, that he can derive from it: The "unresisted rotation" refers to the seeming non-resistance of the air to a mass turning on its own axis: And did he never know the *rotative* velocity of a moving body to "become accelerated" by the oblique "resistances" of other bodies with which it came in contact?

In par. 53–56 my opponent labors to convict me of inconsistencies in various passages which he has culled from my reply to Mr. Espy in the "Franklin Journal;"—as if any inconsistencies of mine could disprove the rotative and progressive character of storms. The alleged inconsistencies result only from his confounding cases which I view as distinct, and from some inaccuracies in the choice of terms.—This labor is also continued on a collection of passages on the barometer. [57–61.] Had our objector given as much attention to the operations of nature in the open air, as he has to the phenomena exhibited in the laboratory, he could not by any possibility have fallen into the error which is exhibited in these paragraphs. It is singular enough that a critic who has detected so much of what he has pleased to fancy inconsistencies and contradictions in my writings, should have failed to perceive that the space "around the exterior border" might, nay indeed must be something very different from the "first portion" or "last portion of the gale." Observation has shown that most of our winter storms are preceded by a high state of the barometer, and that the beginning of the storm is shewn by the falling of the mercury, which rises when the heart of the storm or gale is passed and the wind changes.

Of inaccurate or fictitious quotation, I am sorry to notice an example in "the *reliable facts and observations of our theorist;*" [par. 62]—exhibiting a manner of controversy which can in no wise contribute to the advancement of science. Of the quotation which is here adduced, I believe that not more than three words can be found together in my writings.

In his criticisms on my statements of the changes of wind in storms, [62–68] Dr. Hare fails to notice the distinction between "*suddenly*" and *immediately*, in passages which in their ori-

ginal state and connexion are perhaps sufficiently correct; and he would make the statement of an *exception* which "sometimes happens," to be a contradiction or neutralization of the "evidence," or general result. Had he observed sufficiently he might have found, that his fancied analogy derived from the rotary action of a solid, is entirely inapplicable to the case of natural eddies and whirls, produced in part by a gravitating or centripetal force acting from the exterior. He might thus have learned that his hypothetical statement of the law of rotation in fluids does not, at least in all cases, agree with fact, and can in no way alter or affect the vorticular or other rotative action exhibited in nature. Nor can he disprove or annul the fact, that an immediate or a sudden change does takes place only at the circular *inner margin* of the violent part of a regular and extensive whirlwind storm.

His implied allegation [69] that "there is no evidence" that the wind was more violent on the southeastern* side of the gale of August 17th, 1830, than on its northwestern side, is opposed by the testimony of Capt. Waterman of the Illinois and the log-book of the ship, as compared with observations made at the same time on the opposite or northwestern side of the gale.—It was on or near the central line or axis of this storm, that only southeasterly and northwesterly winds were exhibited.

Dr. Hare has inferred that "in no case would the inner portion of the southeastern and more violent limb" of a gale or hurricane "be beyond the cognizance of our merchants and insurers;" and then says, that "experience shews, that every northeaster brings in a crowd of vessels having only to complain of the violence not the direction of the wind." [70.] But, do the alleged "crowd of vessels" come from far in the southeastern offing? The storm of August 17th, 1830, was at New York a strong "*northeaster*," and would the Illinois, in the Gulf Stream off Nantucket, have found no cause to complain of the "direction of the wind" if bound to New York or Philadelphia?—this ship having had the wind set in at "south," and veering "first to southwest, then to west and northwest," a "perfect hurricane!" "*Experience*" has shown,

* This I believe to be Dr. Hare's meaning; for the word "southwestern," I deem to be a misprint: else Dr. H. fails to understand himself in this passage; for there is nothing in my views or in the nature of the case, which requires the wind to be stronger on the "southwestern" *side* of a storm than on the "southeastern" side.

that in these violent gales, while blowing northeasterly on our shores, the wind becomes more easterly, southerly, and southwesterly, in proportion to the distance from the coast, thus producing a dangerous cross sea; and "our merchants and insurers" have, unfortunately, been too often cognizant of the destructive effects.

In par. 71–74, Dr. Hare shows that an isolated and defective passage on the phases of hurricanes in the West Indies, which he adduces, is not in all respects reconcilable with the local changes in such storms, considered as moving whirlwinds. There are at least two ways by which this labor might have been lessened or avoided: first, by quoting the next sentence, which suggests qualifications; or second, by referring to the same number of this Journal, Vol. xxv, p. 114–121, where the phases of these gales in the western Atlantic are particularly set forth, with a key for suiting these explanations to the West Indian seas; viz. that in the latter, the direction of the wind, in the corresponding sides and phases of the storms, is "about ten or twelve points of the compass MORE TO THE LEFT, than on the coast of the United States in the latitude of New York."

In the next place, Dr. H. endeavors to show [75–77] that I seem to suppose whirlwinds as capable of being "self-induced." In justice to his readers, however, he should have quoted the entire paragraph from which he has cited my remark "that whirlwinds and spouts appear to commence gradually and to acquire their full activity without the aid of any foreign causes." (This Journal, Vol. xxxiii, p. 61.) But can Dr. Hare prove to us, "the aid of any foreign causes?" It is proper to note here, that by the above remark I did not intend to exclude the influence of atmospheric pressure and elasticity, nor changes of temperature and density in and about the body in which gyration is induced. Neither do I disconnect or "isolate" the spirally ascending central motion from the great body of the tornado or whirlwind, as he attempts to do for me.

Dr. Hare finally declares, [78] "I do not deem it expedient to enter upon any discussion as to the competency of the evidence by which the gyration of storms has been considered as proved." —The friends of science will doubtless be surprised at this. For, if Dr. H. did not intend to discuss the "*evidence*" of gyration, for what useful purpose did he "enter the lists?" or why did he

attempt to show facts in disproof? Was it more important to array criticisms and speculations than to bring the question to the test of strict observation and induction? And will not this evasion be received as proof of the weakness of his cause? He says that the competency of the evidence has by Mr. Espy been "ably contested." But has it been so contested by that writer, as to be decided adversely in the mind of any strict and careful inquirer? Even if Dr. H. should admit gyration to be "sufficiently proved," and "should consider it as an effect of a conflux to SUPPLY an upward current at the axis," would not this imply a *self-elevating power* in this "upward current?" And would not the admission of gyration decide the question in my favor?

But he adds further: "Yet the survey of the New Brunswick tornado, made on *terra firma* with the aid of a compass, by an observer so skillful and unbiassed as Prof. Bache, ought to outweigh maritime observations, made in many cases under circumstances of difficulty and danger." Now let me ask—Is gyration disproved by this survey? I trow not: and apprehend that I have sufficiently shown its results to have been consistent with a general rotative action.*

Still unwilling to admit rotation, he appeals to the storm of December 21, 1836, in the terms which follow.

"In like manner great credit should be given to the observations collected by Prof. Loomis respecting a remarkable inland storm of December, 1836. This storm commenced blowing between south and east to the westward of the Mississippi, and travelled from west or northwest to east or southeast, at a rate of between thirty and forty miles per hour. [?] There appears to have been within the sphere of its violence an area, throughout which the barometric column stood at a minimum, and towards which the wind blew *violently* on the one side only from between east and south, and on the other only between north and west. [?] This area extended from southwest to northeast more than two thousand miles. Its great length in proportion to its breadth seems irreconcilable with its having formed the axis of a whirlwind. [!] The course of this storm, as above stated, was at right angles to that attributed by Redfield to storms of this kind. [!] (Trans. Am. Phil. Soc. Vol. 7.)"

We have it here asserted that "this storm" . . . "travelled from west or northwest to east or southeast:" and that, "The course of this storm, as above stated, was at right angles to that attributed by" me to other storms. While at the same time we are told that the area, "throughout which the barometric column

* Article on the New Brunswick tornado, in this Journal, Oct. 1841, Vol. XLI, p. 69–79.

stood at a minimum," . . . "extended from southwest to northeast more than two thousand miles." Now, in all storms which I have noticed in this part of America, the course and progress of the barometric minimum appears coincident with that of the body or axis of the storm; and as the length of the track thus passed over, is quite a distinct thing from the *length* of the storm itself, or from the "area" of the barometric minimum *at any given moment of time*, it appears to follow from Dr. Hare's own statement, that the course of the proper body or axis of the gale was *northeasterly; coinciding with the course of other storms.* Moreover, I have not yet seen any evidence which shows that even *one* storm of magnitude in the United States has proceeded in a *southeasterly* course; although such a conclusion has been suddenly adopted, ere now,* apparently with the hope of escaping from a dilemma in which some favorite hypothesis had become involved.

I am aware that in his elaborate account of this storm and its attendant phenomena, which I greatly value, although dissenting from some of his conclusions, Professor Loomis alleges that "in this case there was no whirlwind." I will only remark, that to me the characteristics of this storm appear to be those of a diffused overland gale of the whirlwind character; the only observations obtained being on the right hand of the path of its axis. I understand, also, that other inquirers have been led by the evidence to the same result.

The manner in which Dr. Hare has described this case, shows very strongly the importance of the inquiry, *What are storms?* For, was it the area of the minimum depression of the barometer—or the area of violent winds—or the area of the rain—or the area passed over by the wave of barometric oscillation—or the area of extraordinary changes of temperature—which constituted the proper limits or identity of this storm ?†

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* Not, however, by Prof. Loomis.

† So far as definitions only are concerned, and these are important in science, it may be proper to adduce the following from Webster, our lexicographer.

"STORM, *n.* A violent wind; a tempest. Thus *a storm of wind*, is correct language, as the proper sense of the word is rushing, violence. It has primarily no reference to a fall of rain or snow. But as a violent wind is often attended with rain or snow, the word *storm* has come to be used, most improperly, for a fall of rain or snow without wind."

We now arrive [par. 79] at Dr. Hare's own views of the origin of storms. These, whether "thunder gusts, tornadoes, or hurricanes," . . . "he had considered, and still considers, to be mainly owing to electric discharges between the earth and the sky, or between one mass of clouds and another." (This Journal, Vol. XL, p. 44.) With this theory or hypothesis, I have no particular concern in this defensive discussion; and shall therefore make but few remarks on the subsequent portion of his paper, which is mainly a reprint of matter which was subjoined to his "objections and strictures," as these first appeared in the Lond. Ed. and Dub. Phil. Magazine.

In either "disruptive" or "convective" discharges of electricity, I discern nothing which can originate or maintain those violent movements of the air which constitute a storm. If the atoms of air are to perform the functions of electrified "pith balls," or "pendula," and thus make a hurricane, (!) it would seem necessary to place them in such space as would admit of their free action, and where their motions could hardly constitute the *wind* or movement in mass of a dense body of atmosphere which is under a compression more than equal to twenty eight inches of the barometric column. There can be no previous "blast of air" to aid the "convection," as this convection is itself supposed to furnish the blast. Nor has any "alternate" or vibratory motion in the air, passing to and fro between the electrified surfaces of the earth and the clouds, been discovered in storms; which, on the "convective" hypothesis, ought to constitute their chief violence. Besides, the cloud *itself*, the probable *result* of the tornado or storm, must first be produced, ere such "convection" could be called into action.

"The disruptive process," as "exemplified by lightning," appears wholly incompetent in itself or its causes "to produce convective discharge upon a scale," equalling in constancy and mechanical effect the force which is "exhibited in tornadoes and hurricanes." [81.] And if it were otherwise, the action of a hurricane or tornado, on this hypothesis, must cease on the occurrence of a "disruptive" discharge; but such discharges appear to cause no cessation in the mechanical force of these storms.

The rising of "misty vapors resembling steam," from the surface of a river, in a tornado, again comes to us transformed into "the rising of the water:" [82] although, had the water thus

risen, I see not how it could give strength to Dr. Hare's electrical hypothesis; there being a known upward mechanical force in the *wind*, at the center of a tornado.

The allegation that the injurious "effects upon the leaves of trees," [83] . . . "cannot be explained without supposing them to have been the medium of an electric discharge," appears quite gratuitous: the mechanical violence of the wind which is witnessed in the severest storms, bruising and tearing off the leaves by thousands, seems an obvious and adequate cause of the injury which the remaining leaves sustain.

If a "convective discharge takes place between a stratum of air in proximity to the earth and a stratum in the region of clouds," [84] it must, as before suggested, be through a reciprocating or vibratory medium, the *downward* motions of which should every where be nearly equal to the *upward;* and there would be no occasion for the *horizontal* motion which constitutes the main force of hurricanes, tornadoes, and other storms. Such vertical motions could hardly take place; and if occurring, could not escape detection.

The reasoning adduced by our author against Mr. Espy's theory, [85] seems conclusive: but it appears not to strengthen the electrical hypothesis.

The alleged electrical relations of the earth's atmosphere, [88–90] if correctly stated, must always exist; and cannot serve to explain the action of storms, which, conforming to the usual course of the great winds, pursue regular geographic routes, unchanged by the electrical qualities of the surface over which they pass.

Neither violent winds nor rains are commensurate with, nor always incident to "a local diminution of atmospheric pressure." Instead of alleging the latter as "demonstrably a cause of wind and rain," would it not be more philosophical and correct to consider diminutions of pressure as the *effect* of certain mechanical movements in the atmosphere? which often occasion rain as well as winds. [91.]

Have "those enormous discharges of electricity which take place during hurricanes," as alleged, [92] been proved to occur either uniformly or generally? And could these discharges cause or constitute the hurricane? If so, let the *modus operandi* be shown.

If it be true, "that moist, foggy or cloudy air is not a conductor of electricity," [93, etc.] does it not follow that any "convective discharges" will seem to be confined solely to the vibratory or alternating motions of the air itself. It appears inconceivable, how the air, either in mass or atoms, can exhibit such movements of transfer and with all the power and velocity of a hurricane, either in *one* direction only, or in opposite directions, at a given time and place. Besides, an effective vibratory action between two distant surfaces with both which the vibrating body is in full contact, appears impossible. Nor would even such alternating action enable the convective discharge to pass through the vibrating stratum of air.—These passing remarks, however, as already suggested, are not needful for sustaining my views of whirlwind storms against the animadversions of my opponent.

Since the discoveries of Franklin, an electrical origin and character has often been conjecturally ascribed to storms. A want of originality in advancing this electrical hypothesis, will not weaken any evidence which shall be adduced in its favor; but, until it shall have been satisfactorily supported by observed phenomena, it will probably continue to be rejected by scientific inquirers.

There seems to be an evident improvement in Dr. Hare's views of whirlwind action, since entering upon this controversy. Nor do I doubt that the subjoined notices of the effects of the Providence tornado, as observed "on *terra firma* with the aid of a compass," will receive from my readers an impartial consideration.

On the Evidence of a general Whirling Action in the Providence Tornado.

On the 30th of August, 1838, between the hours of 3 and 4 P. M., a violent whirlwind or tornado visited the town of Providence, in the State of Rhode Island. It was preceded by a violent shower of rain of short duration, after which the tornado appeared, appended to another cloud, and passed through the southern part of the town nearly from west to east.

Its earliest ravages reported, were in Johnston, at the farm of Mr. Randall, about seven miles west from Providence. From this point it passed on through Cranston and Providence, where,

crossing the river into the State of Massachusetts, it passed through Seekonk, Rehoboth, Swansey, Somerset, and as far, at least, as Freetown, beyond Taunton river; a distance of twenty five miles from the point first mentioned.

The width of its visible track, as indicated by the prostration of trees, fences, and other objects, varied from a mere trace in its narrowest, to two hundred yards or upwards in its widest portions. Having, a few days after the occurrence of the tornado, carefully examined the track for the distance of about seven miles, on each side of Providence river, I propose to offer some of the results of this examination, together with such remarks as may seem justly deducible from the effects observed.

So far, however, as the impressions made on an accidental eye-witness of the tornado may be important, we have a valuable account furnished us in the letter of Zachariah Allen, Esq., of Providence, which is given in Dr. Hare's notice of this tornado. [This Journal, Vol. xxxviii, p. 74–77.] Mr. Allen had the advantage of viewing its progress from a point near its path. He calls it a "whirlwind," and describes its phenomena in a manner perfectly consistent with this appellation. "The circle formed by the tornado" on the river, he describes as "about three hundred feet in diameter," and mentions, that the "misty vapors" . . . "entering the whirling vortex, at times veiled from sight the center of the circle, and the lower extremity of the overhanging cone of dark vapor:" and that "Amid all the agitation of the water and the air about it, this cone continued unbroken," &c.

This "cone" of the tornado of which he so often speaks, it should be noted was an *inverted* one, the smaller end of which was sweeping on the earth's surface.* Thus he gives the instance, "when the point of the dark cone of cloud passed over the prostrate wreck of the building, the fragments seemed to be upheaved," &c. It will be seen here that the *prostration* of the building had preceded the arrival of the center or "point" of the

* We may properly conceive of this "cone," in tornadoes or water-spouts, as including not only the visible clouded condensation here described, but also the invisible portion of the whirlwind which surrounds the narrow and depending portion of the visible cone, below the general line of condensation. This *entire* body of the whirlwind is generally a truncated cone; its smaller and most active end sweeping along the surface of the earth or sea.

"cone;" showing that the whirlwind often acts on a large area, with great force, *externally* to the lower part of the *visible* cone, or the column of vapor at its axis. Moreover, the substances which by the center of the tornado were "uplifted high in the air," were "left to fall from the OUTER EDGE of the black conical cloud."*

Mr. Allen says further, "The progress of the tornado was nearly in a straight line, following the direction of the wind, with a velocity of perhaps eight or ten miles per hour. Near as I was to the exterior edge of the circle of the tornado, I felt no extraordinary gust of wind; but noticed that the breeze continued to blow uninterruptedly from the same quarter from which it prevailed before the tornado occurred. I also particularly observed that there was no perceptible increase of temperature of the air adjacent to the edge of the whirlwind, which might have caused an ascending current by a rarefaction of a portion of the atmosphere."

Soliciting a careful attention to the observations of Mr. Allen, who is well known for his intelligence and his habits of correct observation, I proceed to give some account of my own examinations of the traces of this tornado.

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* Mr. Allen states that the form of the cloud and of the cone of vapor depending from it so nearly resembled the engraved pictures of 'water-spouts' above the ocean, that he should have come speedily to the conclusion that one of these 'water-spouts' was approaching, had he not been aware that "this phenomenon occupied a space in the heavens directly above a dry plain of land." Perhaps it might be inferred that Mr. A. had partaken of the too common notion, that the misnamed *water-spout* is, or should be, literally a *spout of water*. This phenomenon, so much talked of among mariners, proves to be nothing more nor less than the visible inverted "tapering cone of vapor" or condensation, noticed by him as "extending from the cloud to the surface of the earth," *at the axis or ascending portion of the whirl;* if we may at all rely on the results of extensive examinations and comparisons of the accounts of 'water-spouts' and their effects. The same appearance was observed in the New Brunswick tornado by experienced seamen navigating the Raritan river, who at once pronounced it to be a water-spout, and took their measures accordingly. It is probable, however, that most of the 'water-spouts' noticed at sea, are inferior in size and energy to these destructive tornadoes.

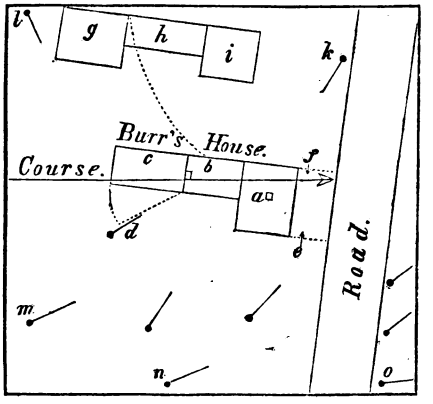
A 'water-spout' was seen by Messrs. Tyerman and Bennett near Borabora in the Pacific, which extended nearly horizontally from one cloud to another directly over their heads; and no harm done! The most credulous will hardly conceive this to have been *a column of water*, or even approximately such: besides, no sea-water has ever been known to fall from the clouds. Similar 'spouts' have been seen by others; and I once beheld a magnificent example of this kind, in one of the interior towns of Connecticut; which probably indicated an axis of rotation nearly horizontal.

From a point on the rocky "ledge" north of the turnpike road and nearly three miles westerly from Providence, to the house of John Burr on the Cranston road, a distance of about one and a quarter miles, I found the course of the tornado to have been S. 86° E. by compass, over a plain country. The magnetic variation being here about 8° westerly, makes the true course E. 3° N. From this point to Providence river, a distance of about two miles, the course was five degrees more northerly.

I agree with Dr. Hare that the general effects observed on this track were "quite similar" to those of the New Brunswick tornado; and will give such of my sketches, formerly prepared, as will best illustrate this similarity and the general effects here mentioned.

The following is a sketch of some of the effects on the farm of Mr. Burr: His house is about one mile and a half from the Providence bridge.

Fig. I. *Providence Tornado.**



In this figure, *a* represents a wooden dwelling-house of two stories with chimney at its center: *b* a dwelling added to *a* and extending to the rear: *c* a lighter building about 16 feet by 30, attached to the rear of *b*: *g* was a large wooden barn: *h* a long building or shed extending from the barn to the carriage-house *i*. The width of the visible track was here about five hundred feet, and the course of the center or axis of the tornado appeared to have passed somewhat diagonally over the three first named buildings.

The house *a* withstood the shock, receiving some damage; the chimney top of *b* was thrown on the roof of *a*, perforating the same, while *b* was unroofed and greatly injured, and a long timber or *sill* from the shed *h* broke endwise into the upper part of the house *b* from a northwesterly direction. The building *c* was turned more than twenty feet to the *left about*, as regards the axis of the whirlwind, against the top of the prostrated pear tree *d*, and was there overturned upon it. There were twenty one persons in *a* and *b*, including a school of children, none of whom were seriously injured.

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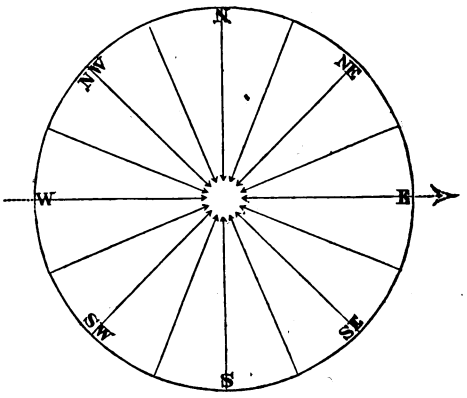
* On these plans the large dot at the end of the several short lines, shows the original position of the root of the tree; the pointed end of the line shows the direction of its top.

The barn *g* and the shed *h* were destroyed, and the materials swept off toward the first named buildings. A corn-house, standing on the same side with the barn, is stated in the Providence papers to have been blown over to the *west*, but I can find no notes of my own respecting the direction of its fall.

The effects here exhibited appear to me to be due to a progressive whirlwind, revolving to the left; for we may notice, as in the New Brunswick tornado, a more onward direction in the trees prostrated on the *right* of the axis, *d*, *m*, *n*, *o*, &c., than on the left side; while the outermost prostrations on the right, *n*, *o*, point still more nearly than the average on this side, to the course of the tornado: And on the left side of the track we have the tree *k* in a direction inclined several degrees *backward* from the course of the storm. The value of these indications of whirling action I have endeavored to point out in my remarks on the New Brunswick case. [This Journal, Vol. XLI, p. 70–75.]

At the front of the house *a*, however, were two slatted door-yard fences, extending from the house to the road. The fence *e* was overthrown northward toward *f*, and the fence *f* in the contrary direction towards *e*: both directions being *transverse* to the line of the axis, which passes between them. Such cases have been adduced as supporting a directly inward course of the wind in the body of the tornado; or, as indicating two bodies of opposing wind meeting on a central line; but I draw a different conclusion.

Fig. II.



Let Fig. II represent, horizontally, the directions of such center blowing winds in the body of the tornado, and let it be supposed as passing over the area of Fig. I, without revolving, so as the course of the center will coincide with the arrow which indicates the course of the axis on that figure. It may thus be seen that on this hypothesis the wind must strike the fences *e*, *f*, either parallel to their length, or but little oblique; a direction of wind which seldom or never

prostrates fences, even in the path of a tornado. Besides, *near the center* of such an inward blowing tornado, where only it could act on these fences with *lateral* force, such winds must necessarily become neutralized both by blowing against each other and by turning upward to escape, thus having little effect at this point, within four feet of the ground. I say nothing here of the possibility of any winds blowing *with violence* in such central directions; which I could never conceive: For the entire spaces between the centripetal lines of arrows must be conceived as being filled by the affluent winds; the lines only indicating their directions.

But on the other hand, let us suppose a strong whirlwind passing in the same direction: the front half of which, both on and near the line pursued by its axis, must necessarily sweep laterally across this line, first *northwardly* towards *f*, if it be revolving to the left; and the last half of the whirl on its arrival will sweep *southwardly* towards *e*. That only the fence *e* was thus prostrated by the first wind of the tornado may be explained by the protection afforded to *f* by the house, against the *advancing* whirl, and perhaps here, also, by the spirally *upward* tendency towards the center, in the wind which thus came round the southeast corner of the house, prostrating *e* in its course. But on the passing of the axis of the whirl, the wind would recur with increased force from the opposite direction, upon the fence *f*, prostrating it towards *e*; while the latter, being already down, and in turn partially protected by the house, would remain as it first fell.

In passing over the track of the tornado between Burr's house and Providence river, several instances and groups of prostration were observed. But owing to the open character of the grounds throughout most of the track, the memorials afforded by the trees were less frequent than have been seen in other cases.

Near the Pawtuxet turnpike, the tornado encountered a new house belonging to Mr. Gardner. This house was in the southern portion of the track *on the right of the axis*, and was removed and turned several feet, *towards the left.*

It is proper to mention here that the *order of changes* in the wind's *direction*, viewing the tornado either as a whirlwind, or, as claimed by Mr. Espy and seen in figure II, would at any *fixed point* on this the right side of the track, be successively *towards*

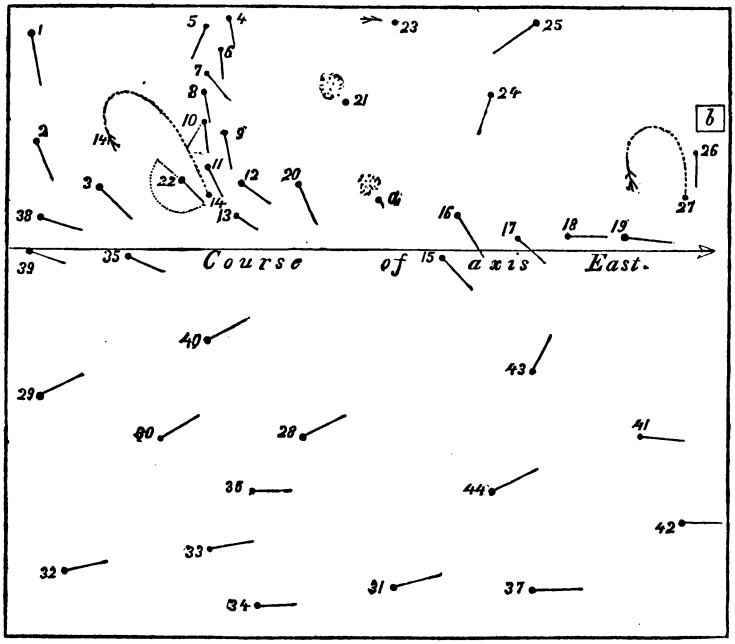
the right, as relates to the center of the tornado. But this building having received its motion by yielding to the wind, shows the true course of the latter as whirling to the left.

Passing by the prostration of the range of buildings near the river, described by Mr. Allen, I proceed to notice the effects which appeared on crossing to the Massachusetts side.

From the bank of the river to the house of Abraham Tifts on the Lyon farm, three fourths of a mile, the grounds were open and unbroken, being mostly under cultivation and with few trees exposed to the tornado, excepting an orchard of scattered apple trees westward of Tifts' house. The traces of the wind in and adjacent to this orchard were very distinct in their character, and I subjoin here the sketch on which they are represented.

Fig. III. *Providence Tornado.*

North or left side.



South or right side.

Explanations of Fig. III.—The cases of prostration 4 to 14, were from a line of small locust trees on the west border of an old apple orchard, and are severally shifted a little out of line for the sake of a distinct exhibition of their directions.

From thence to near Tifts' house at *b*, the ground is but slightly foreshortened, and the relative positions of each tree, on the left of the centre, is approximately

shown. The figure was drawn from my field notes on account of the distinct phenomena which were exhibited on this part of the track, and which, in cases *a*, 14, 22, 21, 23, and 27, show conclusively the first action of the whirl *across the path of the axis*, and sweeping towards the northern border of the track. On the opposite or right side of the axis, southward of 15, there were no trees exposed, and the effects of the tornado were here visible only on the crops and fences. Therefore the cases shown on the figure south of the axis, and also westward of 22 on the left side, were brought in from the more western parts of the track between the orchard and the river, and include all the prostrations from the latter to Tifts' house; and their relative distances from the axis or center of the track are but approximated.

Case 14, represents a small locust tree broken off at an old wound near the root and carried *outward and backward* into the adjoining fallow field, having struck into the ground seven times in its course, leaving distinct traces. It was finally left at a point N. 57° W. from its stump, at the distance of forty yards, with its top turned southwardly, in conformity with its two last traces in the soft ground.

Case 10, a small locust tree was prostrated S. 25° W., leaving its mark in the fallow ground. It was subsequently shifted, by the progressive change in the whirlwind, to S. 11° E.

Case *a*, an old apple tree with but a single branch projecting southwardly from its trunk; this branch was taken off by the onset of the tornado and struck into the ground northwest from the trunk, depositing its apples at this spot. The limb itself was missing.—Case 21, apples deposited as in case *a*.

Case 22, a small wild cherry tree, was found lying on and against the stump of 14, having first been thrown *from the latter* by the onset of the wind and subsequently swung round by the south to its present position, as appeared by the impressions made in the ground. Its final position was such, as if occurring at the outset would have prevented 14 from being carried off northwesterly.—Case 23, the branch of an apple tree was thrown west.—At *b* is shown the relative position of Tifts' house.

Case 27, shows the original position of a large pear tree, the stem of which was broken off and first thrown northward, where it ploughed up the soft ground of the garden by its force, and continued its circuit to a point northwest of its original position, where it remained with its top turned toward the south.

For the purposes of a general comparison, the observed or first known directions of the prostrations on the two sides of the track may be summed up as follows.

Left or North side of the Track.			*Right or South side of the Track.*		
Case.	Direction of first prostration.	Inclination inw'rd and backw'rd fr'm course of tornado.	Case.	Direction of first prostration.	Inclination inw'rd and backw'rd fr'm course of tornado.
38	S. 74° E.	16 degr's.	29	N. 65° E.	25 degr's.
39	S 70° E.	20	32	N. 77° E.	13
35	S. 67° E.	23	30	N. 60° E.	30
1	S. 10° E.	80	33	N. 80° E.	10
2	S. 23° E.	67	34	N. 88° E.	2
3	S. 45° E.	45	36	East,	0
4	S. 12° E.	78	40	N. 65° E.	25
5	S. 35° W. (backw'rd)	125	28	N. 63° E.	27
6	S. 5° E.	85	31	N. 75° E.	15
7	S. 40° E.	50	44	N. 63° E.	27

Left or North side of the Track.

Case.	Direction of first prostration.	Inclination inw'rd and backw'rd fr'm course of tornado.
8	S. 11° E.	79 degr's.
9	S. 10° E.	80
10	fell S. 25 W. turned to S. 11 E.	115
11	S. 26° E.	64
12	S. 55° E.	35
13	S. 55° E.	35
14	first thrown N. 23 W. (backward)	247
15	S. 45° E.	45
16	S. 30° E.	60
17	S. 55° E.	35
18	East,	0
19	S. 85° E.	5
20	S. 27° E.	63
21	N.55°W.(backw'rd)	215
22	first fell N.W. turned to S. 37 E.	225
a	N.45°W.(backw'rd)	225
23	Branch of apple tree thrown west	183
24	S.20° W. (backw'rd)	110
25	S.55° W. (backw'rd)	145
26	South,	90
27	first thrown N.10 W.	260

Right or South side of the Track.

Case.	Direction of first prostration.	Inclination inw'rd and backw'rd fr'm course of tornado.
37	N. 87° E.	13 degr's.
43	N. 30° E.	60
41	S. 85° E.	- 5
42	East,	0

Mean direction of prostration on the right side of the track N. 73° E.: average inclination inward from course of tornado, *seventeen degrees.*

Mean direction of first prostrations on the *left side* of track, S. 4° W.: average inclination inward and backward from course of tornado, *ninety four degrees.*

Relative inclinations of the two sides to the line of axis, more than *five* to *one.*

It is proper to mention, that the average inward inclination of *all* the prostrations on the *right* side of the track for a distance of four miles east of the river was thirty degrees.* This however does not affect the conclusions in favor of rotation to the left.

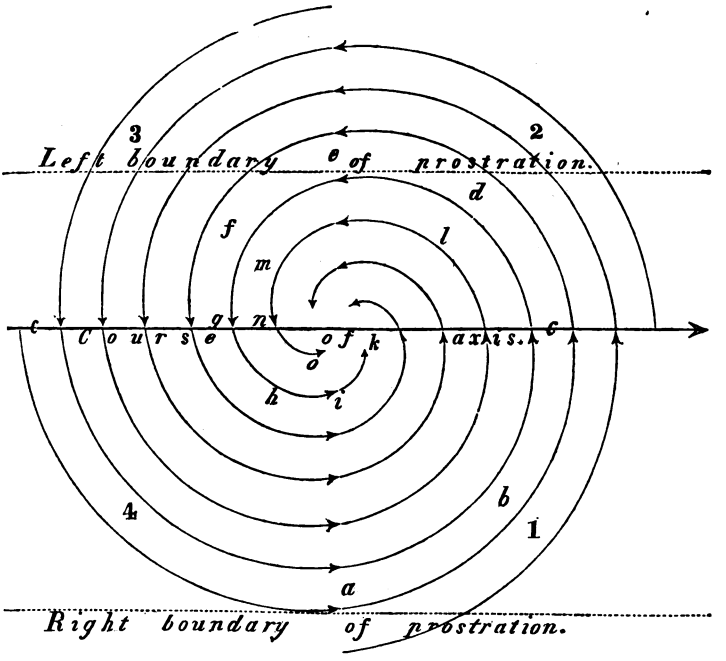
These average results, on the two sides, together with the observations already adduced, appear to me to afford decisive evidence of whirlwind rotation in this tornado, in the direction from right to left or which is contrary to the hands of a watch. In reference to this evidence and that exhibited in my paper on the New Brunswick tornado, I add from my prepared sketches the following figure, as an approximate illustration of the whirling action in these tornadoes, so far as this may be shown *horizontally* and by a stationary figure.

Let the involuted lines or arrows on this figure be supposed to represent the motion of the wind at or near the bottom of a vertically cylindrical portion of the center of a tornado, comprising a length of radius equal to the greatest width of the prostrating power on the right of the axis of its path. Now if the tornado

* This larger average gives a relative degree of inclination on the two sides of *three* to *one.* Nearly the same difference is found in two outside bands of prostration, of equal widths, (Tables I and V,) shown in my survey of the New Brunswick Tornado. See this Journal, Vol. XLI, p. 78.

be considered as whirling in the manner here represented, but *without any change of location*, its action may be supposed as concentrically equal on all sides; the motion, however, becoming quickened towards the center in the *inverse ratio* of the successive concentric areas: that is, each particle of air as it revolves about the axis, *continuing to describe nearly equal areas in equal times*, in its progress towards the center, where it rises spirally in the direction of discharge; this direction being vertically at the center, the point or area of least atmospheric resistance or pressure. Thus, the course of a single particle, horizontally, may be *a b c d e f g h i k*;—and so on or between each of the four involuted lines which constitute the figure.

Fig. IV.



For further reference, we may divide this figure by the cross lines of arrow heads, into the four quadrants 1, 2, 3, 4.

We will now consider this whirl as having *a constant progressive motion* on the line of the long arrow *c c*, at a rate equal to one fourth or fifth of its average rotative velocity. It will then follow, that as the force of the whirl on the trees and other ob-

jects encountered by it, is *as the square* of the wind's velocity at the point of impingement, the relative effects on the two sides of the line of the axis, which before were equal, will now be greatly altered.

For, if at a given distance *on the right* of the advancing axis, the former velocity was 80, it will now, as relates to the earth's surface, have become 100; and at the same distance on the *left* side the velocity of the wind will be reduced to 60, as relates to the earth's surface. Thus the squares of these effective velocities will give a power relatively equal to 100 at the former point and only 36 at the latter; both being equally distant from the axis. Hence, although the *rotative* velocity of the whirl decreases rapidly as we recede from its axis, yet its *prostrating* power will, by its progressive motion, become greatly extended on the *right* side of the advancing axis, and proportionally contracted on the left side. Thus the respective boundaries of the prostrating power on the two sides of the tornado, when thus in motion, may be those indicated on the figure; which nearly correspond to the effects which have been observed in several cases.

It may be seen further, that *nearly all the prostrations* near the line of the axis and elsewhere, must, by the advancing motion of the tornado, receive a direction *more onward* than is represented by the arrows or lines in the figure, which can repr[illegible]t only a stationary rotation.

In further considering these effects, in different portions of the whirl, as it encounters objects in its advance, we shall find the maximum effects to be mainly on the line *a*, *i*, *o*, at the rear of the *first quadrant*. Hence, if a tree on this side the axis should fail to be prostrated till after the first quadrant had passed over, it would not be likely to fall in the fourth quadrant, on the further advance of the tornado, unless very near to its axis. Moreover, if one tree should fall when under the more advanced portion of the first quadrant, another if prostrated later in the same quadrant, must necessarily fall in a *more onward* direction than the first, and if sufficiently near will lie across the latter.

It may likewise be seen, that the wind of the whirl in passing into the *second quadrant*, on the left side of the track, is sweeping *backward*, and with its effective power thus greatly reduced, as regards fixed objects on the earth's surface. Thus the limits of prostration are not only narrowed, but the effective power is

here greatly reduced, and gives *fewer prostrations* than under either the first or third quadrants. The *minimum* of effect occurs on the arrival of the line *e k*, at the rear of the second quadrant.

But on the arrival of the *third quadrant*, the prostrating power on the left side becomes more and more efficient by the ceasing of the backward and the accession of the progressive movement; and at or near the line of *f m*, it again takes effect, with rapid increase. The destructive force is also much augmented here by the greater velocity of the heart of the whirl, near its axis, and the impetus must rapidly increase in energy to its maximum effect, as at *m n o*, taking off any tree which may here remain, and carrying aloft, or sweeping onward, the objects previously prostrated on the line *c x k*.

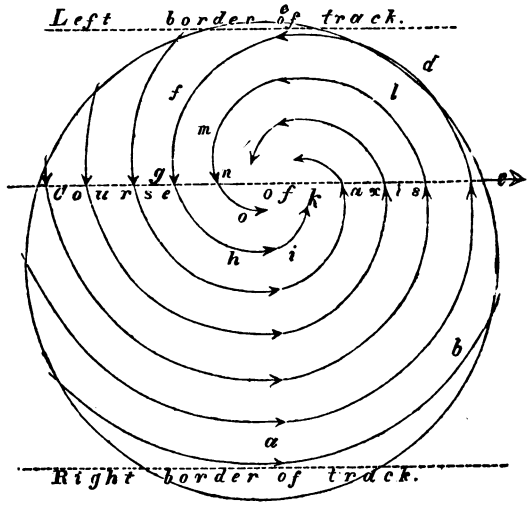
If a tree on the *left* side of the track falls on one previously thrown down by the tornado, the last fallen will also have the more onward direction, as on the other side: unless both have fallen in the second quadrant, where few prostrations occur.—The *fourth quadrant*, for causes noticed in considering the *first*, can have little prostrating effect, except perhaps on the small area near its axis.

If we now conceive of our figure as applied only to the limits of prostration or destruction which constitute the *visible path* of the tornado, it becomes apparently and relatively unequal, in its right and left hand quadrants, the axis appearing greatly eccentric, and in the same degree, at least, as the *left* band or belt of prostrations is found narrower than that on the right of the axis. This apparent, but illusive form of the whirl, may be illustrated by fig. V; which is drawn on the same lines with the preceding figure.

It will readily be seen that this eccentricity of the axis, on the visible track, will be in proportion to *the progressive velocity of the tornado;* other things being equal. Thus, if Mr. Allen be nearly right in his estimate of the rate of progress in the Providence tornado, the eccentricity shown in its path would be generally less than is shown in figures IV and V. On the other hand, if the progressive velocity should be as great as Professor Loomis informs me he ascribes to the tornado of February last, in Ohio, viz. about forty miles an hour, the eccentricity would in such a case be greatly increased, showing the axis as far out-

ward, perhaps, as would be in line with *m*, or *f l*, in these two figures.

Fig. V.



From this examination it appears to result, that an observer who follows the track of a tornado after its departure, will find on one side of the apparent axis of its path, *if it be a whirlwind*, a continued series of prostrations pointing almost invariably onward and inward, with various degrees of inclination to the course of the path. While on the other side of the axis, a narrower band or belt of prostrations will be found, which are also inclined mainly inward and onward, but showing greater inclinations from the line of progress, together with frequent cases which incline more or less backward and sometimes even outward from the course of the tornado.

It may also appear, that a want of proper attention to the necessary conditions of the prostrating power in a progressive whirlwind, can alone induce us to ascribe such effects to supposed antagonistic winds, blowing simultaneously in opposing directions.

Leaving, for a moment, the more tangible features of this inquiry, we may now take some notice of the more outward portions of the "cone" or whirlwind, which are supposed not to be comprised in figure IV. Assuming here the involuted and inward motion, with its upward discharge at the centre, it follows that the impulsive accession of air which is necessary for main-

taining a violent whirlwind action, must come in horizontally, and in the same gradually involuted courses; or, must descend in like manner from a higher region, in and around the outward parts of the whirling cone. I have long since been led to believe that this impulsive accession comes from *both* these sources, but *chiefly from the latter;* and that this motion of accession and support is spirally *downward* in the outward portions of the whirl. The latter being, in its higher portions, often greatly expanded, as noticed by Mr. Allen.

The evidence on which this opinion rests, can be but partially alluded to here; but I will suggest the following considerations: —1. The ascertained existence of a stratum of unusually cold air in the higher region of clouds, on some particular days remarkable for the occurrence of numerous thunder gusts and tornadoes:* 2. The observed descent of a portion of the clouds in front of the nucleus or body of a heavy squall or tornado, which may sometimes be traced by the eye as low as the existing limit of condensation will afford opportunity for observation: 3. The fact noticed by Mr. Allen and others, that adjacent "to the exterior edge of the circle of the tornado" or whirlwind, the previous breeze often continues "to blow uninterruptedly from the same quarter" as before:† 4. The last fact, when taken also in connexion with certain peculiar and striking effects in the outward portions or edge of the tornado, a knowledge of which I have gathered from various sources: 5. The coldness of the air which has been noticed at the edge of a whirlwind: 6. The instant penetration of the lower end of the whirlwind into thick forests, and into hollows and ravines, which has been frequently noticed: 7. The direct memorials of downward action in the outward portions of the whirl which I have myself met with, on the tracks of different tornadoes.

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* This change of upper temperature I think can be clearly made out on the day of the New Brunswick tornado, which was but one of many tornadoes and thunder gusts which appeared in this part of the United States on the same day; and on the preceding day in Illinois and other western states.

In the New Haven Gazette are accounts of *five* severe tornadoes which occurred in the states of New Jersey, Connecticut, Massachusetts and Rhode Island, on the afternoon of August 15, 1787. I can also refer to many more recent cases of this kind.

† The observation here quoted is one of many which show the error of the very hasty generalization which alleges a circuit or annulus of calm air to have been observed *on all sides* of tornadoes and hurricanes.

In most of the foregoing remarks it has been my design to view the tornado as it moves onward, in full action. Of the origin or incipient causes of the whirl, it is not necessary here to inquire: although some clue to these is perhaps afforded us in the considerations above noticed.

Recurring once more to the track of the Providence tornado, I have to state that eastward of Tift's house the course of the track soon became S. 65° E. magnetic, for more than two miles. It then took the course of S. 75° E., and further onward the tornado passed directly over the house of Solomon Peck, about four miles from Providence. This house was partly unroofed; chimney thrown down; windows broken *inward*, as in many other cases; and much other damage was also done to Mr. Peck's property. In passing onward towards Taunton river the tornado appears to have preserved an inclination to the south of east: the track, though slightly sinuous, appearing, like that of the New Brunswick tornado, to form part of a great curve, with its convex side to the northward.

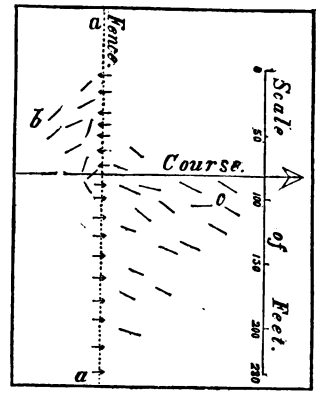
On the track from the Lyon farm to Peck's house there were many interesting memorials which might confirm the deductions already made. On some portions of the track, also, the tornado appeared to have risen almost entirely from the surface, its reversed apex leaving but a narrow trace, and on some fields, even no trace at all. But in these cases, as on the tracks of other tornadoes, the compass bearing did not fail to lead the explorer to new ravages, where, at times, the energy of the tornado appeared to be greater than before.*

Before we take leave of the traces of this tornado I would adduce another of my prepared sketches, which shows the rotative effects in a manner which I think should satisfy the most strenuous opposer of whirlwind action. In this sketch, Fig. VI, we have represented a portion of the track which crossed at right angles a line of weak post-and-rail fence, *a*, *a*. On the *right* of the axis, this fence was prostrated *eastwardly* or in the direction of the course of the tornado, as shown by the short arrows which may represent the posts of the fence; the rails also having been scattered onward and inward, towards *c*, in the general manner rep-

* This is not uncommon in tornadoes, and is especially noticed in the account of two "Trombes" which are given in Pouillet, Elemens de Physique et de Météorologie, § 655.

resented in the figure. On the *left* side, however, every post was prostrated *westwardly*, and the rails were likewise blown slightly backward toward *b*, in the same general direction. The scale of feet, which measures across the track, was obtained by estimating twelve feet to each length of the rails. The locality of this sketch was perhaps a mile eastward of the Lyon farm. —The application of the foregoing views of rotation to this case, it can hardly be necessary to point out.

Fig. VI. *Providence Tornado.*



I have noticed many effects of similar kind on fences; but that the *backward* prostration on the left side of the track should have taken full effect in this case, and mainly, perhaps, under the second quadrant, I ascribe to the age and general weakness of the fence.

Additional memorials might here be adduced in evidence, and of similar character to the foregoing; but having already occupied more space than I intended, I must now leave the question of a general whirlwind rotation in this and other tornadoes to the candid consideration of impartial inquirers.

New York, July 12, 1842.

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