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LIX. *On Dr. Hare's "Additional Objections" relating to Whirlwind Storms.* By W. C. REDFIELD*.

IN my reply to Dr. Hare's first series of "Objections" and "Strictures," I attempted to show that these could have no weight or efficacy in disproving the whirlwind character of violent storms and tornadoes; and, furthermore, that convincing evidence of whirlwind action in the tornado of New Brunswick was found in those very facts which he had set forth and relied on for disproving its rotation†.

Besides correcting, on that occasion, certain grave errors into which my opponent had fallen, I also referred to the additional proofs of rotation which are found in my published survey of the effects of this tornado‡. This was deemed sufficiently conclusive in replying to Dr. Hare, who had chosen to "enter the lists" as my assailant and in support of his own and Mr. Espy's notion of the centripetal course of the wind in these storms; for the effects produced by the New Brunswick tornado had been greatly relied on by each of these writers, as proving such centripetal course in the wind of tornadoes.

At that time I possessed, in my field-notes and surveys, abundant evidence of a constant rotative action in several other tornadoes, and plans or diagrams which exhibit portions of this evidence had long been prepared; but I saw no defect in the proofs of rotation previously shown that required their publication.

One of the tornadoes which I had thus prepared to illustrate, was that which passed near Providence in 1838, some account of which had been published by Dr. Hare; and, as partiality for his own electrical hypothesis may have induced him to engage in this controversy, I now refer to a paper con-

* Communicated by the Author.

† This Journal for May 1842, p. 353-369.

‡ For this survey, &c. see this Journal for January 1841, p. 20-29.
Phil. Mag. S. 3. No. 155. *Suppl.* Vol. 23.

taining what I deem decisive evidence of the whirling character of the Providence tornado, published since the first appearance, in America, of his "Additional Objections," and found in this Journal for January 1843, p. 38-52.

It has probably been perceived, that in advancing his "additional objections," which are found in this Journal for August last, Dr. Hare seems virtually to abandon the main question of rotation as an issue of fact, as rested on his previous allegations relating to the New Brunswick and Providence tornadoes: for he appears now to rely chiefly on a *petite guerre* of criticisms, which have little, if any, relation to definite observations; the only evidence on which the question really depends.

I might justly complain of that apparent want of candour which has prevented Dr. Hare from correcting, in any manner, the several mistakes and errors, whether of fact, quotation, or induction, which were pointed out, long since, in my reply to his first series of "objections" and "strictures." It is this want of candour in the discussion that seems to demand these defensive notices and remarks, which perhaps are more necessary from the fact that few persons, probably, engage in a careful and strict analysis and comparison of the observations which have been made in storms.

Dr. Hare now says he had "endeavoured 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." But 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 endeavoured 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 the effect of these winds on the barometer.

Referring to a supposed approval of my views by men of science, Dr. H. says [§ 58], "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 experience, some men of science of the present time are prone to lend a favourable ear to any hypothesis, however absurd in itself, provided it be *associated with observations*." As already stated, it is "observations" and their results which I have chiefly endeavoured to promulgate. But if it has been attempted to associate a favoured "hypo-

thesis," whether "absurd" or otherwise, with observations on storms, I apprehend it has been by my opponents, notwithstanding that the seeming dislike to observations may appear unfavourable to this conclusion.

In the same paragraph are alleged no less than three quotations in forms of words and connexion such as I did not use; and at least three following paragraphs of the "additional objections" appear devoted to the unamiable attempt to render me obnoxious to distinguished men, which perhaps may render proper the following statement and explanation.

I had incidentally remarked, on the occasion of Mr. Espy's first attempt to discredit certain facts and results which I had stated*, 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 which are found to prevail over a great portion of the globe." And, in allusion to the views found in Sir John F. W. Herschel's treatise on Astronomy, I also said, "Sir John, however, has erred, like his predecessors, in ascribing mainly, if not primarily, to heat and rarefaction those results which should have been ascribed solely to *mechanical gravitation*, as connected with the rotative and orbital motion of the earth's surface, the influence of which he but partially recognizes in connection with this and another subject of inquiry." 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. I have reason to believe that Sir John Herschel has not thought himself *accused* or *denounced* in these passing and somewhat hurried remarks.

Even if Dr. H. could have succeeded by this *ruse* in covering his apparent discomfiture on the main question of rotation, was it required for the elucidation of science, or consistent with the rules of candour and courtesy, that he should persist in repeated efforts to excite an odium in the minds of his readers?

I had pointed out Dr. Hare's error in alleging that I reject the influence of heat on winds. In now repeating this allegation [§ 63], he complacently 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†

* Silliman's Journal, vol. xxviii. p. 316.

† 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.

when the preceding and following opinions were published must be quite evident." And he then quotes, somewhat inaccurately, part of the subjoined extract as sustaining this allegation; the correctness, or pertinacious unfairness of which, I shall leave unprejudiced readers to determine from the very evidence to which he refers. I had said in immediate connection with the foregoing, as follows:—

..... "But, to prevent being misunderstood, I freely ADMIT that HEAT IS OFTEN AN EXCITING as well as modifying CAUSE of *local* WINDS, and other phænomena, and that it HAS an incidental or subordinate ACTION (though not such as is usually assigned) in the organization and DEVELOPMENT OF STORMS, and that, in certain circumstances, IT INFLUENCES the interpositions 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 EXHIBITED by regular and GENERAL WINDS. But, so far from being the great prime mover of the atmospheric currents, either in producing a supposed primary north and south current, or in any other manner, I entertain no doubt, that if it were 'possible to preserve [this is the part Dr. H. 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.'"—*Silliman's Journal*, 1835, vol. xxviii. p. 317.—And with all this before him, he now reasserts that I then rejected THE INFLUENCE OF HEAT!

It appears to dissatisfy Dr. Hare that I should have deemed the first 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?" But, waiving any want of analogy between fire and storms, suppose that in treating of fire one chemist should ascribe it to the heat of combustion, another to the smoke and aqueous vapour evolved, while a third should view it as being caused by electricity; would not the proper inquiry then be, what is fire, and what are its obvious phænomena? It appears evident that the laws and phænomena of storms must be first ascertained and established, ere we can advantageously investigate their origin or primary causes.

If Dr. Hare chooses to consider this an endless controversy which he has waged, and that to follow its misunderstandings would be an Ixion task, ought he not to reflect, that grace to acknowledge those "misunderstandings" which it had brought to light would doubtless have shortened its duration?

Paragraphs 66 to 70 Dr. Hare has devoted to some superfluous suggestions found in my earliest paper, 1831, which were virtually withdrawn more than three years since*. He has also joined [§ 68] a passage from that paper with another from a subsequent one, and quotes both as from the latter. The "unresisted rotation" here refers to the seeming non-resistance of the air to a body turning on its own axis; and the *rotative* velocity of a moving body was correctly viewed as being sometimes "accelerated" by the oblique "resistances" of other bodies.

In § 70-73 my opponent labours to convict me of inconsistencies in passages culled from my reply to Mr. Espy; as if any inconsistencies of mine could disprove the rotative character of storms. The alleged inconsistencies result from his confounding cases which I view as distinct, and from some inaccuracies in my choice of terms. The like purpose is evinced in § 74-78, with a collection of passages on the barometer, where Dr. H. seems to confound the space "around the exterior border" of the gale with its "first portion" and "last portion."

In his criticisms on my statements of the changes of wind in storms [§ 79-85], Dr. Hare fails to appreciate the proper distinction between "*suddenly*" and *immediately*, in passages which in their original state and connection 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 carefully 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, which are produced by a gravitating 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 takes place at the *inner margin* of the violent part of a regular and extensive whirlwind storm, at the border of the central lull or remission of the gale.

His implied allegation [§ 86] that "there is no evidence" that the wind was more violent on the *south-eastern*† side of

* See note prefixed to my article on Hurricanes in the Nautical Magazine for January 1839, and Silliman's Journal, vol. xxxv. p. 201-202.

† This I believe to be Dr. Hare's meaning; for the word "south-western," which he here uses, I deem to be a misprint; else Dr. H. fails to understand himself in this passage; for there is nothing in my views, as set

the gale of August 17th, 1830, than on its north-western side, is opposed by the testimony of Captain Waterman of the Illinois and by the log-book of the ship, as compared with observations made at the same time on the opposite or north-western side of the gale. It was on or near the central line or axis-path of this storm, that only south-easterly and north-westerly winds were successively exhibited; a fact which appears quite sufficient to settle the main question between me and my opponents.

Dr. Hare infers that "in no case would the inner portion of the south-eastern and more violent limb" of a gale or hurricane "be beyond the cognizance of our merchants and insurers;" and then says, that "experience shows that every north-easter brings in a crowd of vessels having only to complain of the violence, not the direction of the wind" [§ 87]. But do the alleged "crowd of vessels" come from far in the *south-eastern offing*? The storm of August 17th, 1830, was at New York a strong "*north-easter*;" 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 south-west*, then to *west* and *north-west*," a "perfect hurricane!" "*Experience*" has shown, in a multitude of cases, that in these violent gales, while blowing north-easterly on our shores, the wind is found more easterly, southerly, and south-westerly, in proportion to the increased distance from the coast. This produces a dangerous cross sea; and "our merchants and insurers" *have*, unfortunately, been too often cognizant of the destructive effects.

In [§ 88-91], Dr. Hare has succeeded in showing that a summary passage on the phases of hurricanes in the West Indies, from which he adduces an extract, is not reconcileable with all the local changes in such storms, considered as moving whirlwinds. There are two ways, however, by which this labour might have been lessened or avoided: first, by quoting the next sentence, which suggests qualifications, and second, by referring to the same number of Silliman's Journal [vol. xxv. p. 114-121], where the phases of these gales in the western Atlantic are particularly set forth, together with a key for suiting these explanations to the storms while in West Indian seas, viz. that in the latter region, the direction of the wind, in the corresponding sides and phases of the storms, is

forth, or in the nature of the case, that requires the wind to be stronger on the "*south-western*" side of a storm than on the "*south-eastern*" side, but rather the contrary.

found "about ten or twelve points of the compass MORE TO THE LEFT [on the compass card], than on the coast of the United States in the latitude of New York."

In the next place, Dr. H. endeavours to show [§ 92-94] 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" (Silliman's 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 variations 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 whirlwinds as he attempts to do for me.

Dr. Hare finally declares [§ 95], "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 may well 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 a series of 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, or with such scrutiny and arrangement of the facts alleged as would allow them to speak in their own true language*? 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 favour?

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 skilful and unbiassed as Professor Bache, ought to outweigh maritime observations, made in many cases under circumstances of difficulty and danger." Now let me

* Perhaps a partial exception ought to be acknowledged here as relates to one case. See Journal of the Franklin Institute (Philadelphia) for June 1839, p. 372-374.

ask, Is gyration disproved by this survey? I trow not; and apprehend that I have sufficiently shown its results to have been accordant with a general rotative action*.

Still unwilling to admit rotation, he refers 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 Professor 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 north-west to east or south-east, 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 south-west to north-east 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. vii.)"

We have it here asserted that "this storm" . . . "travelled from west or north-west to east or south-east:" 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 stood at a minimum," . . . "extended from south-west to north-east 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 *north-easterly*; *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 *south-easterly* course; although such a conclusion has been suddenly adopted, ere now†, apparently with the hope of escaping from a difficulty in which some favourite hypothesis had become involved.

* Article on the New Brunswick tornado, in this Journal, January 1841, p. 20-29.

† Not, however, by Prof. Loomis.

I am aware that Professor Loomis alleges, in his elaborate account of this storm and its attendant phenomena, which I greatly value, although dissenting from some of his conclusions, 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 evidently 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 storm, and his erroneous allegation in regard to its course, show 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*?

Those readers who may desire to ascertain the general course of the wind in the body of a great storm, without resorting to a process of induction from characteristic facts on one hand, or to the aid of ingenious hypotheses which regard certain alleged but unknown movements of the air in connection with the higher atmosphere on the other, are referred to a schedule and map of observations made at about forty separate localities, at the hour of noon, in the storm of December 1839, which are found in this Journal for January 1841. These observations are believed to exceed in number and accuracy any that have yet been obtained in equal limits, and they are arranged on the map so as to speak their own proper language as simultaneous observations†. Hence they appear to show conclusively, that the violent easterly winds in this American storm were resolved, through a circuitous geographical course, into the strong north-westerly winds which immediately followed the easterly part of the gale; instead of mounting to unknown regions, before opposing winds, as has been alleged by others.

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

† For more extended remarks relating to these observations, see Silliman's Journal for April 1842, p. 112.

The arrows marked on the small geographical sketch which is here annexed, show the direction of wind at some of the principal points of observation eastward of the Hudson river, near the close of the day, when the body of the storm was further advanced in its north-easterly course. The concentric lines, drawn at intervals of thirty miles, are designed to afford better means of comparison for the several observations.

The observations which I have obtained of this storm, and its remote effects, are far more extensive to the southward and eastward than the limits of the map; showing also that in this portion of the storm, the winds in the early part of the gale were blowing from south-easterly and southerly quarters. It is worthy of remark, that if only those observations which are southward of the parallel of Long Island had been obtained and considered, this storm would appear to show an inequality in its phases and an absence of violent north-easterly winds, similar to what is found in Professor Loomis's account of the storm of December 1836, which he was led to pronounce as no whirlwind.

The observations on the map referred to have a further value, inasmuch as they belong to a case which Mr. Espy has exhibited as one of his inward and upward blowing storms; for they show, on a strict comparison and investigation as to *time* and *locality* in the storm, that, besides inaccuracies, the *coup d'œil* of the observations delineated in his diagrams is illusory, and gives to consecutive winds, which follow each other over the same localities, or in different parts of the storm's path, the appearance of simultaneous and opposing winds, blowing in opposite courses towards each other.

In the case of tornadoes it is necessary to resort to a process of induction to determine both the relative positions in the tornado of the several fallen bodies at the instant of their

prostration, and the general character of the prostrating force which these may conjointly indicate. The course of induction suggested in my two papers on the tornadoes of Providence and New Brunswick, as taken together, are believed to afford sufficient grounds for a correct determination, when applied to the traces of other tornadoes. It is also satisfactory to find, that in the surveys exhibited in the above cases there are several traces of individual objects moving in the tornado, which fully confirm the accuracy of the more general induction.

As regards Dr. Hare's own views of the electrical origin of storms, some notice has been taken of these in Silliman's Journal for October 1842, p. 261-263. Since the discoveries of Franklin, an electrical origin and character has often been conjecturally ascribed to storms. A want of originality in advancing this hypothesis will not weaken any evidence which shall be adduced in its favour; but until it shall have been satisfactorily supported by observed phenomena, it will probably continue to be rejected by scientific inquirers. And were it possible to show an electrical origin in great storms and tornadoes, it would in no wise alter the known fact that a determinate rotative action has been noticed in these storms.

LX. *Notice of some Experiments on Subterranean Electricity made in Pennance Mine, near Falmouth.* By R. W. Fox, Esq.*

I HAVE already communicated to the Geological Society of London† some results produced by the electric action of two nearly east and west metalliferous veins which have been partially explored in Pennance mine. I have since made other experiments in the same mine, in which ore-points, consisting of copper and iron pyrites in the two veins, were connected by a pair of copper wires, which in most instances acted on a galvanometer or other apparatus at the surface, an end of each wire having been brought up through a shaft for the purpose; about 50 fathoms of wire were employed, although the ore-points in the different veins were only about 14 to 18 fathoms asunder in a direct line.

A galvanometer of not much sensibility was generally used; the needle, which was $2\frac{1}{2}$ inches long, moved on a pivot, and had a coil of fine wire passed 48 times round it. Another galvanometer, consisting of a suspended astatic needle and 140 coils of wire, was also employed occasionally.

* From the Transactions of the Royal Cornwall Polytechnic Society.

† The communication here alluded to will be found in our report of the proceedings of the Geological Society, pres. vol. p. 457.—EDIT.