



# XV. Additional objections to Redfield's theory of storms

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To cite this article: Robert Hare M.D. (1843) XV. Additional objections to Redfield's theory of storms , Philosophical Magazine Series 3, 23:150, 92-106, DOI: [10.1080/14786444308644700](https://doi.org/10.1080/14786444308644700)

To link to this article: <http://dx.doi.org/10.1080/14786444308644700>



Published online: 30 Apr 2009.



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XV. *Additional Objections to Redfield's Theory of Storms.*  
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50. **I**N a communication to the London and Edinburgh Magazine and Journal of Science for December 1841, I endeavoured to point out various errors and inconsistencies in the theory of storms proposed by Mr. Redfield, or in the reasoning and assumed scientific principles on which that theory had been advanced. Of these errors I will present a brief summary.

51. I conceive that Mr. Redfield has erred in ascribing atmospheric currents, whether constituting trade winds or storms of any kind, "solely to mechanical gravitation as connected with the rotatory and orbital motion of the earth†."

52. In ascribing those atmospheric gyrations, of which according to his hypothesis all storms consist, to "opposing and unequal forces," without specifying the nature or accounting for the existence of these forces, although implying that they originate as above mentioned.

53. In assigning to all fluid matter a tendency to "run into whirls and circuits, when subjected to opposing and unequal forces," when this allegation, if true at all, can only be so in some peculiar cases of such forces.

54. In alleging all storms to be whirlwinds, and yet representing a "rotative movement in air as the only cause of destructive winds and tempests," so that a whirl is the only cause of its own violence‡.

55. In averring, in reference to the alleged gyration and vortical force of tornadoes which are by him treated as hurricanes in miniature, that "all narrow and violent vortices have a spiral involute motion quickening in its gyration as it approaches the centre or axis of the whirl," whereas it must be evident that when gyration in a fluid does not result from a contemporaneous centripetal force, arising from an ascending or descending current at the axis, but on the contrary exists only in consequence of a momentum previously acquired, the consequent velocity in any part of the mass affected, will be less in proportion to its proximity to the axis: also that the only case in which it can increase with its proximity, is where the mass is fluid and it proceeds from some competent cause acting at the axis.

56. In representing that the upward force of tornadoes

\* Communicated by the Author. Mr. Redfield's papers will be found in Phil. Mag. S. 3. vol. xx. p. 353; vol. xxii. p. 38.

† See paragraph 60 of this essay.

‡ Silliman's Journal, vol. xxi. p. 192: "Storms and hurricanes consist in the regular gyratory motion or action of a progressive body of atmosphere, which action is the sole cause of the violence which they may exhibit."

is the effect of a vortical or gyrotory action \*, when it must be quite plain that a "vortical" action or whirling motion instead of causing the air upon the terrestrial surface, necessarily subjected by it to a centrifugal force, to seek the centre, would induce that portion of the atmosphere which should be above the sphere of the gyration, to descend into the central space rarefied by the centrifugal force.

57. In admitting the gyration, which he considers as the cause of storms, to quicken as it approaches the axis of motion, without perceiving that this characteristic is irreconcilable with his inference that gyration caused by forces acting remotely from the axis is the proximate cause of all the phenomena in question.

58. In the number of Silliman's Journal of Science for April, 1842, Mr. Redfield has hinted that the pains which I have taken to confute his doctrines are disproportioned to the low estimation in which I have professed to hold them. I should be glad if this view of the subject should render my strictures agreeable to him; and am sincerely sorry that, consistently with truth, I cannot directly take a course more favourable to his meteorological infallibility. I admit that his essays have met with an attention which may have justified him in pluming himself on their success. Had it been otherwise, I should not have thought it worth while to enter the lists. 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 favourable ear to any hypothesis, however in itself absurd, provided it be associated with observations. But to proceed with the "reply," so called, the author alleges that in the absence of "reliable facts and observations" in support of my objections to what he considers as the "established character of storms," he had hesitated to answer them. This cannot excite surprise, when it is recollected "that the whole modern meteorological school," and likewise "Sir John Herschel," are accused by him of a "grand error," in not ascribing all atmospheric winds "solely to the gravitating power as connected with the rotary and orbital motion of the earth."

59. For this denunciation he has no better ground than that on which he deems his theory to be above my reach, that is to say, because himself and others have made some observations showing that in certain storms, agreeably to log-book records, certain ships have had the wind in a way to indicate

\* See paragraph 92 of this communication.

gyration. Being under the impression, that in many instances no better answer need be given to Mr. Redfield's opinions than that created in the minds of scientific readers by his own language, I will here quote his denunciation of the opinions of the meteorological school and of Herschel.

60. "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. \* \* \* An adequate and undeniable cause for the production of the phenomena \* \* I consider is furnished in the rotative motion of the earth upon its axis, in which originate the centrifugal and other modifying influences of the gravitating power, which must always operate upon the great oceans of fluid and ærial matter, which rest upon the earth's crust, producing of necessity those great currents to which we have alluded."—(See Silliman's Journal, vol. xxviii. p. 316.) Speaking of Sir John Herschel's explanation of the trade winds and others, Mr. Redfield alleges, "Sir John has however 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."

61. Is it not surprising that it did not occur to the author of these remarks, that an astronomer so eminent as Sir John Herschel would be less likely than himself to be ignorant of any atmospheric influence resulting from gravitation or the diurnal and annual revolutions of our planet—and that when he found himself in opposition to the whole school of meteorologists, a doubt did not arise whether the "grand error" was not in his views of the subject instead of that which they had taken?

62. It seems to have been forgotten, that all the aqueous portion of the terrestrial surface being, no less than the superincumbent atmosphere, subjected to the gravitating power and the rotary and orbital motions of our planet, no impulse can be given to the one which is not received by the other; and that as the heavier the fluid the greater the influence, if this be competent to create gales in the atmosphere, it must be no less competent to produce torrents in the ocean. Moreover, do not his opinions conflict not only with the whole school of meteorologists, but also with a portion of the modern school of geology? Agreeably to the last-mentioned school, the external portion of the earth consists of a comparatively thin shell of earth and water floating upon an ocean of matter kept in fusion by heat; the oblate spheroidal form of our planet being due to the perfect equilibrium of the "gravitating, ro-

tary, and orbital" forces which are most inconsistently represented by Mr. Redfield as having upon the atmosphere an opposite effect.

63. But notwithstanding the opinions expressed in the paragraphs above quoted, and in the following, Mr. Redfield alleges in his reply to my objections, that it is an error to consider him as rejecting the influence of heat. 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. "Were it possible to preserve the atmosphere in a uniform temperature all over the surface of the globe, the general winds would not be less brisk than at present, but would be more constant and uniform than ever."—(Silliman's *Journal*, vol. xxviii. p. 318.)

64. Mr. Redfield alleges that the proper inquiry is, What are storms? not How are storms produced? And yet it will be found that his great object has been to show that they arise from gyration caused by unequal forces generated in some inexplicable mode by gravitation and the complicated motions of our planet. But 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! I do not therefore blame Mr. Redfield for pursuing both inquiries simultaneously, inconsistently with his own rule above stated, but I am astonished that he should, without any new experiments or any demonstrations, by an *ipse dixit* undertake to make a novel application of the gravitating power, and the forces arising from the earth's motion; and to inform one of the most eminent astronomers of the age that he had committed an error in overlooking their all-important meteorological influence.

65. Turning from an endless controversy with a writer with whom I differ respecting first principles, I shall address myself to that great school of meteorologists who concur with me in the "grand error" of considering heat and electricity as the principal agents of nature in the production of storms, and who do not concur with Mr. Redfield in considering gravitation and the earth's annual and diurnal motion as the great destroyer of atmospheric equilibrium. So far as it may conduce to truth, I shall incidentally notice some parts of Mr. Redfield's reply; but my main object will be to show the inconsistency of his theoretic inferences with the laws of nature, and the facts and observations on which those inferences are alleged to be founded. To follow him in detail through all the misunderstandings which have arisen, and which would inevi-

tably arise during a continued controversy, would be an Ixion task.

66. Speaking of the trade winds and monsoons, our author states, "It is to the operation and effect of these great and regular moving masses, that we are disposed mainly to ascribe the more active and striking meteorological phænomena of every latitude." And again, "At these seasons the northern margin or parallels of the trade winds sweeping towards the gulf, must necessarily come in collision with the great archipelago of islands which skirt the Carribean Sea; \* \* \* (Silliman's Journal, vol. xx. p. 31,) the obstruction which they afford produces a constant tendency to circular evolution. \* \* \* These masses of atmosphere thus set into active revolution continue to sweep along the islands with increased rapidity of gyration until they impinge upon the American coast." "We have assumed that the leading storms of the northern and western Atlantic and the American coast originate in detached and gyrating portions of the northern margin of the trade winds, occasioned by the oblique obstruction which is opposed by the islands to the direct progress of this part of the trade, or to the falling of the northerly and eddy wind upon the trade, or to these causes combined."—(Silliman's Journal, vol. xx. p. 48.)

67. I trust it will be sufficiently evident, that although great and regularly moving masses of air, by encountering obstructions, may undergo a transient deflection, and that a portion accidentally caught in a strait with high cliffs on either side might, like the tide in the Bay of Fundy, acquire a local and temporary acceleration, yet that it would be utterly impossible for a durable whirlwind to be thus excited. Obviously for the endurance of a whirl, if not for its production, the continuous application of at least two forces would be requisite, of which one must be endowed with a centripetal efficacy in order to counteract the concomitant centrifugal momentum. It will be evident that although a local obstruction may cause an eddy or whirl in its vicinity, the rotary momentum thus created must soon be exhausted. But admitting that a blast by being deflected by an island could become a permanent whirlwind, obviously the resulting velocity could not be so great as that of the generating current. The moderately blowing trade wind could not, by contact with an inert body, acquire an increase of velocity adequate to form a furious hurricane capable, as represented, of travelling circuitously for more than two thousand miles.

68. The hurricane once created, agreeably to the imagination of Mr. Redfield, its subsequent progress is described in

the following language:—"This progress still continues while the stormy mass is revolving around its own moving axis; and we can readily comprehend the violent effects of its unresisted rotation, while this velocity becomes accelerated by nearly all the oblique forces and perhaps resistances of the circumjacent currents or masses of moving atmosphere. These storms cover, at the same moment of time, an extent of contiguous surface, the diameter of which may vary from one to five hundred miles, and in some cases have been much more extensive. They act with diminished violence towards the exterior, and with increased energy towards the interior of the space which they occupy." (Silliman's Journal, vol. xxv. p. 114.)

69. Thus it is assumed, that a mass of air from "one to five hundred miles in diameter" being made to whirl with the velocity of a most furious gale, is not only "unresisted" by the waves, forests, hills and mountains which it may encounter, but is actually "accelerated by nearly all the oblique forces and perhaps RESISTANCES" which it may meet. Yet it must be quite clear, that any reaction with currents not moving the same way, or moving with an inferior velocity, or obliquely, could only be productive of retardation.

70. The following inconsistencies will show how far Mr. Redfield's account of the phenomena of storms is to be deemed sufficiently accurate or consistent to upset the established principles of science.

71. "The rotation of a continued whirlwind involves not only changes in the position and condition of its constituent particles, but a constant accession of the exterior atmosphere to the body of the whirlwind, together with a discharge equally constant spirally at one extremity of its axis of rotation." (Franklin Journal, vol. xix. p. 122.) Ibid., p. 120: "Nor is it my intention to deny any movement or upward tendency at the centre of a whirlwind storm, for of such a movement, apart from theory, I have long since obtained good evidence." Ibid., p. 122: "In regard to the depression of the barometer which I have ascribed to the rotary action of whirlwind storms, Mr. Espy has himself shown, that the centrifugal action in a storm which gyrates horizontally must tend to withdraw or rarefy the air at the centre by causing a transfer or accumulation towards the exterior of the storm, thus causing a higher state of the barometer around the exterior border, than at the centre of the gale. This connexion and result is in strict accordance with the facts of the case as exhibited in all storms of this character, so far as my observations and information extend."

*Phil. Mag.* S. 3. Vol. 23. No. 150, Aug. 1843.

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72. On opposite sides of the same leaf we find the preceding quotations. Agreeably to the first, there is a constant accession of air from the exterior atmosphere to the body of a whirlwind, attended by an upward force and compensated by a discharge at one extremity of its axis of rotation; agreeably to the last, the centrifugal action tends to withdraw the air of the centre by causing a transfer or accumulation towards the exterior border.

73. In tornadoes the author admits the undeniable existence of an ascending column at the axis (92.), and we are told that a whirlwind storm "operates in the same manner and exhibits the same general characteristics as a tornado\*;" but this idea is evidently irreconcilable with that of a withdrawal of air from the centre, agreeably to one of the contradictory allegations above cited.

74. Nor are the following observations more consistent. "During the passage of these eddies or storms over the place of observation the barometer sinks while under their first or more advanced portions and rises as they pass over or recede." (Silliman's Journal, vol. xxv. p. 129.) "The barometer, whether in higher or lower latitudes, always sinks while under the first portion or moiety on every part of its track excepting perhaps its extreme northern margin." "The mercury in the barometer always rises again during the last portion of the gale and commonly attains the maximum of its elevation on the entire departure of the storm."

75. But if "a higher state of the barometer be created around the exterior border of a whirlwind than at the centre," and if of necessity the exterior border be first encountered, how does it happen that precisely about this space, agreeably to the statement last quoted, the barometrical column should sink? And if, agreeably to the statement quoted previously, the air be rarefied about the centre and accumulated towards the border, in passing from the one border to the other through the centre, would not the mercury in the barometer first rise, then sink, and afterwards rise again, instead of falling during its exposure to one moiety of the storm, and rising during exposure to the other?

76. It may be presumed, that respecting the state of the barometer and the movement of the air, within the sphere of his whirlwinds, Mr. Redfield's views are not in accordance with any settled notions. His theory leads to the idea of a centrifugal force, rarefying and removing the air from the centre, while his observation of the ascending current in tornadoes has tended to create an opposite impression.

\* Silliman's Journal, vol. xxv. p. 117.



77. Considering the inconsistencies of Mr. Redfield's "reliable facts and observations," I hope I may be allowed to show what ought to ensue according to his own premises. Evidently in a whirlwind, constituted as are those to which we have reference, the centrifugal force will cause an accumulation of air towards the exterior until the otherwise uncoun-  
 teracted pressure of the accumulation, tending to restore the level, is *in equilibrio* with the centrifugal force. Moreover, the reaction of the fluid lying in the same plane beyond the whirl, will cause the fluid to be higher, or if elastic, denser at an intermediate point than the general level. In the case of an elastic fluid like the air, condensation will be substituted for accumulation, and will amount to the same thing in effect. It would follow, that as the whirl should advance, the barometer would rise until the front limb of the zone of greatest condensation should arrive; subsequently it would fall till the central space should arrive, and then another rise and subsequent fall would ensue during the approach and departure of the rear limb of the zone of greatest condensation.

78. One fact is mentioned among the contradictory evidence above quoted, which seems to be supported by universal experience. The barometrical column does fall at the commencement of a storm, and of course this fact does not accord with the idea that storms are whirlwinds produced by mechanical forces remote from the axis and attended necessarily by a centrifugal action which would accumulate the air towards the exterior.

79. Respecting another characteristic, the "reliable facts and observations of our theorist" are no less irreconcilable than in the case last considered. I allude to the changes in the direction of the wind which ensue from the commencement to the end of a hurricane, and especially on the outer limbs on each side of the line of progression.

80. Thus, speaking of the progress of a storm from south-west to north-east along the coast of the United States, he alleges that "along the central portions of the track the first force of the wind is from a point near south-east, but after blowing for a certain period it changes suddenly, and usually, after a short intermission, to a point nearly or directly opposite to that from which it has previously been blowing; from which opposite quarter it blows with equal violence till the storm has passed over or abated." Again, "It is demonstrably evident, that at any point over which the centre of a whirlwind may pass, the wind must suddenly change to a direction almost exactly opposite to that which has been felt during the preceding part of its progress." (*Silliman's Journal*,

vol. xx. p. 22.) "It sometimes happens, when the central portion of an extensive storm passes over or near the point of observation, that the comparative calm, or lull which prevails about the apparent centre of rotation is preceded by a gradual rather than a sudden abatement of the wind." "Every experienced navigator will shrink with instinctive apprehension from the very idea of those moments of awful stillness which place him in the central vortex of the hurricane." (Franklin Journal, vol. xix. p. 116, and Silliman's Journal, vol. xx. p. 47.)

81. Amid the neutralization of evidence which inevitably results from the conflicting statements above quoted, I will endeavour to point out the results which ought to ensue if the inferences of the advocates of the whirlwind doctrine were correct.

82. When a rotary motion is communicated to a solid by a force applied to any part whatever, the tangential velocity at any point will be directly as its distance from the centre. In a fluid, when the force productive of rotation is applied at any point remote from the axis, the motion at the axis can be no quicker than in the case of a solid, but may be slower, since the parts do not of necessity move simultaneously. In the case of a fluid body kept in motion by a momentum resulting from forces previously applied, as in the instance of a Redfield whirlwind, any zone, which has been made to revolve by the direct application of force, will be retarded until it causes, in the adjoining zones, a due proportionable velocity. This will not be attained until the whole rotates like a solid. There is however this difference, that the external portions of the whirling zone being pressed by the centrifugal force against other portions of the same fluid, the one will conflict with the other, so as to cause the velocity to be communicated and to lessen outwards from the zone (in which the moving power is or has been applied) till it becomes insensible. This result must ensue the more speedily, since the momentum receives no reinforcement, while the mass which it actuates increases with the square of the distance from the axis.

83. It follows that at any station over which, or near which the centre of a whirlwind shall pass, there will be a breeze scarcely perceptible at first, but which will strengthen gradually into a gale of pre-eminent fury. Subsequently a declension must take place until the centre arrives; here again there would be no perceptible wind. The centre having moved away, the wind must increase again to a maximum of force and then decline to a breeze.

84. Mr. Redfield alleges, that the storm of August 17th, 1830, whirling to the left, travelled from south-west to north-east at the rate nearly of twenty-seven miles per hour; that its greatest diameter was from five hundred to six hundred miles; that of its severe part was from one hundred and fifty to two hundred and fifty miles. Thus it may be assumed, that in order for an observer to be exposed successively within the severe portion on the south-eastern and north-western limbs, the storm would have had to move at least one hundred miles, requiring nearly four hours. Hence if the storm in question were a whirlwind, instead of the change having been sudden, several hours would have been required for its gradual accomplishment.

85. To prove therefore that a sudden change ensued from one violent wind to another of the same character blowing in an opposite direction, is to demonstrate that the storm in which it took place was not an extensive whirlwind. Yet this characteristic is universally admitted to belong to hurricanes, and especially to those upon our territory in which a south-easter is followed by a north-wester. Hence the seaman's saying which Mr. Redfield sanctions in quoting, "a north-wester does not remain long in debt to a south-easter."

86. But if the storm above alluded to moved from south-west to north-east as Mr. Redfield's doctrine requires, and the velocity of the wind on the south-eastern and north-western limbs of the whirl were as great as described, that on the south-western side must have been more than a fourth more violent, having the general motion of the storm superadded to its appropriate gyrating velocity. Yet there is no evidence that any such superiority existed. On the contrary, the violence of the south-easter and north-wester seems to have been pre-eminently the object of attention.

87. Agreeably to Mr. Redfield, hurricanes have a diameter varying from one mile to five hundred miles, the diameter of the severe part of the storm of August 1830, being from one hundred and fifty to two hundred and fifty miles. Of course a portion of the eastern as well as the western limb of such a storm might be comprised between the Alleghany mountains and the Atlantic shore; and in no case would the inner portion of the south-eastern and more violent limb be beyond the cognizance of our merchants and insurers. It would be a matter of course that in every violent north-east gale, arising as represented from the progression of the north-western limb along our coast, fears would be entertained lest vessels, inward-bound, should be met by a much more violent south-wester. But experience shows, that every north-easter brings in a

crowd of vessels having only to complain of the violence, not of the direction of the wind.

88. It has been assumed, that a storm whirling to the left and travelling north-easterly, must, at stations passing nearly under the centre, first blow as a south-easter and afterwards gradually change to a north-wester. Meanwhile on the south-eastern or left limb it will blow only from the south-west, and on the north-western or right limb it will blow only from the north-west. Consistently, when the storm travels from south-east to north-west, as hurricanes are represented to travel in proceeding from the sphere of their origin in the West Indies to the coast of North America, it will at stations within a certain distance of a line described by the centre, blow from the north-east first. On the south-western limb it will blow first as a north-wester; on the north-eastern limb as a south-easter. Moreover, that on the last-mentioned limb the greatest violence will occur, since the general motion of the whirlwind will there cooperate with that of the whirl. Yet in the following paragraph Mr. Redfield informs us (Silliman's Journal, vol. xxv. p. 128), that "in the West Indies hurricanes begin to blow from a northern quarter of the horizon, and then changing to west and round to a southern quarter and then their fury is over."

89. This account of the direction of the wind in West India hurricanes agrees with that quoted by Espy from Edwards's History of Jamaica, vol. iii.: "All hurricanes begin from the north, veer back to west-north-west, west, and south-south-west, and when got to south-east, the foul weather breaks up."

90. It must be evident, as stated among my "objections," that when a whirl is first originated, whether it describe a helix, as would result from its progressive circular motion, or a circle, as represented by Mr. Redfield in his charts\*, it must at thirty-two stations equidistant from each other and the centre of gyration, blow from as many points of the compass. However, when once under way, it being granted that the whirling is always from right to left, evidently at any station near the line described by the centre, it will begin to blow at right angles to that line or from the north-east. As the centre advances this wind would gradually subside, and, after the centre should have gone by, it would begin to blow from the south-west with increasing force till the severepart of the south-eastern limb should be passed. On this part of the track only one change would take place. But at two stations sufficiently remote from the central line, the wind in passing from north-

\* Franklin Journal, vol. xix. p. 120.

east to south-east would undergo an intermediate deviation, but necessarily of an opposite nature, since for the same reason that at one there would be first more northing and then more westing, at the other there would be more easting and more southing, *pari passu*. But on the outward north-eastern and north-western limbs, or in other words, on the right and on the left external borders, there would be no change. On the one it would blow from the north-west only, on the other only from the south-east. On this last-mentioned limb the blast would be pre-eminent in violence, since in that direction the gyrative and progressive motion of the whirlwind would concur.

91. Nevertheless, agreeably to the observations which have lifted the whirlwind theory above the reach of my strictures, hurricanes in the West Indies begin (at every place) from a northern quarter, and changing first west, and afterwards to a southern quarter, terminate their fury. Thus, agreeably to the evidence of Mr. Redfield, the fury of the hurricane is the least where, according to his hypothesis, it should be the greatest.

92. Having cited and endeavoured to show the futility of the only explanation which can be found in Mr. Redfield's essays of the mode in which whirlwinds are induced, I will quote a passage from which it would seem that they are supposed capable of being self-induced. Whence it would follow, that without any extraneous aid, his "rotary movement, which is the sole cause of destructive winds and tempests," could spontaneously excite itself and the adjoining elements into a destructive commotion. From this statement, it appears that the author was not aware that in making it he gave a blow to his favourite idea of opposing and unequal forces, arising from gravitation and terrestrial motion, being the cause of stormy atmospheric gyration.

93. "We may observe, also, that whirlwinds and spouts appear to commence gradually and to acquire their full activity without the aid of any foreign causes; and it is well known they are most frequent in those calm regions where apparently there are no active currents to meet each other, and they are least frequent where currents are in full activity." (Silliman's Journal, vol. xxxiii. p. 61.)

94. Treating of whirlwinds excited by fire, the author thus expresses himself:—"The foregoing results can only be explained by a violent vortical action steadily maintained. \* \* \* The ascending power of the vortical column or whirlwind is strongly exhibited. \* \* \* But the spire of a columnar vortex exhibits a penetrating and ascending power which far exceeds, both in its intensity and the extent of its action, any other as-

cending movement that we witness. This effect appears to be owing to the spiral motion of the column which presses onward in the direction of its axis, till it reaches a limit of elevation yet unknown." (Silliman's Journal, vol. xxxvi. p. 56.) Would it not be as reasonable to expect the spiral of iron usually employed to open bottles, spontaneously to penetrate a cork without being actuated by the operator's hand, as that the aerial spiral, which agreeably to the description above given, constitutes a tornado, should, "without any foreign aid," "or any currents to meet each other," be endowed with the force which he has described? Admitting the storm-producing efficacy of a collision between trade winds and islands, admitting that gravitation, and rotary and orbital force are to be substituted for all other agency, how are those causes to extend influence to his aerial isolated spiral, so as to beget the wonderful vortical force portrayed?

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. By Mr. Espy that has been ably contested. I have given some reasons for doubting the accuracy or consistency of Mr. Redfield's representations, though I have no doubt they have always been made in perfect good faith. I have already alleged, that were gyration sufficiently proved, I should consider it as an effect of a conflux to supply an upward current at the axis. 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. 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. of the American Phil. Society, vol. vii.)

96. Having said so much against the whirlwind theory of storms, it may be expected that I should, on this occasion, say something respecting the opinions which I entertain of their origin. To a certain extent this will be found in my communications published in Silliman's Journal, vol. xxxii. p. 153, vol. xl. p. 137, also in my Essay on the Gales of the United States. I still believe our north-eastern gales were correctly represented in the last-mentioned essay as arising from an exchange of position made between the air of the Gulf of Mexico and that of the territory of the United States which lies to the north-east of that great estuary; and that the heat given out during the conversion of aqueous vapour into rain, by imparting to the atmosphere as much caloric as could be yielded by twice its weight of red-hot sand, is a great instrument in the production of the phenomena; also, that the cold resulting from rarefaction is a cause of the condensation of that vapour, and of course of clouds. On this last idea, derived from Dalton, Mr. Espy has founded his ingenious theory of storms; alleging, erroneously, as I think, the buoyancy, resulting from the heat thus evolved, to be the grand cause of rain, also of tornadoes, hurricanes, and other electrical storms. In the essay above mentioned, I erred in ascribing too much to variations of density arising from changes of elevation, and twenty years' additional experience as an experimenter in electricity, has taught me to ascribe vastly more to this agent than I did formerly.

97. In November last, I verified a conjecture of my friend Dr. J. K. Mitchell, that moist, foggy or cloudy air is not a conductor of electricity, its influence, in paralyzing the efficacy of electrical apparatus, arising from moisture deposited on adjoining solid surfaces.

98. A red-hot iron cylinder, upon which evidently no moisture could be deposited, suspended from the excited conductor of an electrical machine, was found to yield sparks within a receiver replete with aqueous vapour, arising from a capsule of boiling water.

99. Hence it appears that bodies of air, whether cloudy or clear, may be oppositely electrified from each other or from the earth. This would explain the gyration on a horizontal axis which seems to be attendant on thunder gusts, and may account for the ascent of the south-easter and descent of the north-wester in the great storm of December 1836, described by Professor Lomis.

100. Such gyration may be a form of convective discharge, in which electrical reaction is assisted by calorific circulation and the evolution of latent heat, agreeably to Dalton and Espy.

101. Squalls may be the consequence of electrical reaction between the terrestrial surface and oppositely excited masses of air, and the intermixture of masses so excited in obedience to the same cause, may be among the sources of rain, hail, and gusts. The specific gravity of a body of air, electrified differently from the surrounding medium, may be lessened by what is called electric repulsion; the particles inevitably moving a greater distance from each other, as similarly electrified pith-balls are known to do.

102. Hence a cause of rarefaction, buoyancy, and consequent upward motion, in a column of electrified air, more competent than that suggested by Espy.

103. Should it be verified that a gyration from right to left takes place during convective discharges of electricity in hurricanes, it may be referrible to the disposition which a positive electrical discharge from the earth to the sky would have to gyrate in that direction.

I have prepared some strictures on Dove's Essay on the Law of Storms, which will be the subject of a future communication.

XVI. *On a Method of Etching on Hardened Steel Plates and other Polished Metallic Surfaces by means of Electricity.*  
By J. H. PRING, M.D.

*To the Editor of the Philosophical Magazine and Journal.*

SIR,

**I** HEREWITH transmit to you a rough specimen of what I conceive to be a novel employment of the power of electricity, and shall be gratified should the process by which it was effected prove susceptible of any useful application to the arts.

The method which I employed in the production of the characters on the accompanying plate\* was the following:—Having six batteries of the kind invented by Mr Smee, the platinized silver plate of each being about three inches square, I attached the steel plate to be etched upon to the zinc extremity of the batteries, a coil of covered wire, of considerable length, being previously interposed between the steel plate and the zinc: then taking the wire connected with the platinized silver in my hand, I used it as an etching-tool on the steel plate,—an electrical spark of great brilliancy, accom-

\* This was a steel plate, on which the words "Etched by means of Electricity. Bath, 30th June 1843. I. H. P." together with some ornamental devices, had been produced by the above method. It gave only a faint, just legible impression by the copper-plate press.—EDIT.