nied by force of any sort, (in other words, retaining its *power* to exert a force when called for,) forever. But if, at any time, the force of gravitation should begin to act, the ball would then exert its *ineptia* upward, *equal* and *opposite* to gravity, and thus it would continue *through all the change of the ball's velocity and direction*, until its final return to the earth; affording, by the way, the true rationale of retarded and accelerated motion, by a constant force.

Having established a proper distinction between *motion* and *power*, it is easy to explain to so intelligent a reasoner as T. W. B., wherein he has mistaken the matter. Had he asserted that, "when any elastic body strikes another body larger than itself, and rebounds, it does not give to that other body all its *power*, by just the quantity with which it rebounds," he would have stated the fact; for whilst the *momentum* of a given body is in the simple ratio of its velocity, the *power* is in the ratio of the *square* of the velocity, and the sum of power possessed by the bodies after the stroke, will be the same (whether the body struck be larger or smaller) as that originally possessed by the striking body; that is to say, when the body struck is *smaller*, and does not receive all the motion of the other, the sum of the products, of their quantities of matter, multiplied into the *squares* of their velocities respectively, after the stroke, will be the same as when it is *larger*, and causes the striking body to recoil, or when they are *equal*, and the striking body remains at rest after the impact.

It is a well known law of falling bodies, that their distances passed over are in the ratio of the *squares* of their velocities; and experiment proves that, if a body is let fall upon an elastic table, it will rebound to the height from whence it fell; showing that the power employed in giving the body velocity, and that employed in taking it away, is proportionate to the square of that velocity.

It was not my intention to have gone so far into the subject; but receiving the impulse from a powerful body, and meeting but little resistance from gravity, and following implicitly the laws of motion, I have passed over a reasonable quantity of matter, (taking the distance, instead of the velocity, as the standard of measure,) by the mere *vis-ineptia*.

Respectfully,

RUFUS TYLER.

FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

On the Evidence of Certain Phenomena. By W. C. REDFIELD.

The "Notes of an Observer,"* containing some strictures on two papers relating to meteorology, which are found in the twenty-third volume of the American Journal of Science, are doubtless entitled to notice from the writer of this article, which would have been more promptly given, had those strictures met his eye at the time of their publication; but he had, unfortunately, no knowledge of their ap-

* Journal of the Franklin Institute, vol. xiii., p. 9.

pearance, till a few days since, when chance brought them under his observation.

The intelligent writer of the notes introduces his strictures on the two papers alluded to, by the following remarks:

"They contain a most laborious collection of facts, which, if well authenticated, will be of immense importance to meteorological science. Some of them, however, are so anomalous, and inconsistent with received theories, that I hesitate to put entire confidence in them, and shall continue to doubt until I have the most certain evidence of the facts."

As was partially intimated in those papers, circumstances do not permit me to set forth in detail the great mass of evidence and authorities by which the statements in those papers are sustained, or even to such an extent as I deem to be highly desirable. It does not, however, seem necessary that facts in this department of science should be rejected, or even doubted, for no better reason than being "inconsistent with received theories;" for while, in the present improved state of physical science, we are so justly rigid in demanding correct inductions from well observed or established facts, before we consent to give credence to *new* theories, it may be well, perhaps, to inquire when, and in what manner, the "received theories" in meteorology have been demonstrated to be true. That the theories in question have long been received, and that they influence and control our modes of thinking and reasoning on these subjects, is doubtless true; and it is believed that the latter often happens, too, in the face of much positive evidence of their fallacy. Nor is such a mental process at all uncommon, even in this age of the exact sciences, and I have had occasion to see the most unsupported and conjectural hypotheses adduced by able writers, as satisfactory disproof of a series of well observed facts. But to return to the "Notes of an Observer," who proceeds to particularize his exceptions as follows.

"For example: In p. 132 he says, 'In large portions of the Pacific Ocean, the tides are exempt from the lunar influence. At Tahiti and the Georgian groupe, near the centre of the Pacific Ocean, the tide rises but one or two feet, and it is high water at noon and midnight throughout the year, and this, too, in the very region where the established theory would lead us to expect the lunar tides to be the most regular and powerful.' Again, in p. 128, he says: 'In Peru, at the height of 18,000 feet, the wind has been found to be fresh from the south-west,' meaning, if I understand the whole paragraph, that this is the prevailing direction of the wind at that place."

Without inquiring whether the closing inference here quoted be justly warranted by the paragraph to which he alludes, I have to remark that it is *possible* that my statement in regard to the tides at the Society Islands, and certain other parts of the Pacific, may prove not to have been sufficiently guarded. We should naturally think that, even in the absence of such lunar influence, the momentum of the tides from other portions of that great ocean would necessarily affect the equilibrium of the surface at these islands; and Prof. Whewell,

in his able elucidation of the cotidal lines, has given the time of high water at these islands, without any direct intimation of such phenomena. But, on the other hand, we have the authority of gentlemen who have been attached for many years to the English missions at these islands, and who, from their known habits of life, must be supposed to be thoroughly conversant with the facts of the case, in support of the statement which I have made. The following statement of the Rev. William Ellis, one of the gentlemen alluded to, and who has returned to England, may be found in his *Polynesian Researches*, at p. 28-9, vol. i. of the second London edition.

"Among the natural phenomena of the South Sea Islands, the tide is one of the most singular, and presents as great an exception to the theory of Sir Isaac Newton, as is to be met with in any part of the world. The rising and falling of the waters of the ocean appear, if influenced at all, to be so in a very small degree only, by the moon. The height to which the water rises, varies but a few inches during the whole year, and at no time is it elevated more than a foot, or a foot and a half. The sea, however, often rises to an unusual height, but this appears to be the effect of a strong wind blowing some time from one quarter, or the heavy swells of the sea, which flow from different directions, and prevail equally during the time of high and low water. But the most remarkable circumstance is the uniformity of the time of high and low water. During the year, whatever be the age or situation of the moon, the water is lowest at six in the morning, and the same hour in the evening, and highest at noon and midnight. This is so well established, that the time of night is marked by the ebbing and flowing of the tide, and in all the islands the term for high water and midnight is the same."

A fact which is thus substantiated, and which has become incorporated into the very language of a whole people, it would be difficult to call in question. It is supported also by the testimony of Messrs. Tyerman and Bennett, in their *Journal of Voyages and Travels*,* to say nothing of facts collected from other sources, or which have the same bearing, though relating to other regions. I will mention, however, that Mr. Whewell quotes the observation of Capt. Beechy, that at Papiate, one of the Society Islands, it is high water every day at half an hour before noon, and low water at six in the evening; and he also informs us that Lieut. Malden (Lord Byron's voyage) gives a similar account of the tides at Owhyhee, situated in a corresponding latitude and position in the northern Pacific. In the last number of the *American Journal of Science*,* we have also a further confirmation of the fact in question, as given us by Mr. John Ball, of Troy, New York, at the close of his interesting account of the country west of the Rocky Mountains; he says, "A return from the Columbia river by water around Cape Horn, touching at the Sandwich and Society Islands, gave some opportunity to observe the winds, and other phenomena." "During three weeks stay at Tahiti, the tide was observed to rise about one foot, and always highest at twelve o'clock, noon and midnight, and I was informed that this is always the case."

* See Boston edition, vol. ii., p. 225.

* Vol. xxviii. p. 8.

It must, therefore, I think, be admitted, that there is a suspension or neutralization of the lunar tide-wave in the region in which those islands are situated. We find, too, that in the Atlantic it is high water on the coast of Surinam about five o'clock on the days of the new and full of the moon, and the flood runs to the westward. At the windward islands of the West Indies, the tide is some one or two hours later, and, though exposed to the whole tide range of the Atlantic, the tides are very weak and irregular, not rising more than at the Society Islands. On the southern coast of the United States, and at the island of Bermuda, in the Atlantic, it is high water about seven o'clock, the flood tide in the offing at the latter place *running to the northeast*. On the southern coast of Rhode Island and Massachusetts, it is high water from seven to eight o'clock. On the southeastern coast of Nova Scotia and Newfoundland, it is high water from eight to nine o'clock, the flood tide off the latter coast also running northeastwardly. At the Azores, or Western Islands, in lat. 38° N., near the middle of the Atlantic, it is high water about 12 o'clock, *and the flood runs to the eastward*.† Finally, it is high water on the western coasts of Ireland and Spain about two o'clock, all on the same days. These statements are approximated from the American Coast Pilot, and other authorities, care being taken to avoid the retarding effects of local obstructions as far as possible, by timing from the most extraneous positions of coast, towards the open ocean. Viewing these phenomena in connexion with some other facts, had led me to suspect that the great tide wave performed an actual circuit in each of the great oceanic basins, on both sides of the equator, passing *westwardly* in the equatorial latitudes, and returning *eastwardly* in the higher latitudes, above 25° or 30° north and south, and analogous to the course which is pursued, as can be demonstrably shown, by the great *currents*, both of ocean and atmosphere. If such be the operation of the tides, certain regions in mid-ocean would form the foci, or neutral points, in these great elliptical circuits, and would be but slightly, if at all, affected by the ordinary tides. The elaborate investigation of cotidal lines in which Professor Whewell is engaged, will probably show whether this conjecture is well founded, or whether the course of the great tide wave be from the Southern Ocean, northwardly, through the entire length of the Atlantic, and in disregard of the direct lunar influence in this ocean, as would seem to be indicated in his late paper on that subject. The greatest difficulty attending the inquiry, is in procuring correct observations from those islands and external points of coast, which bear most decidedly upon the question; and whatever may be its results, I am happy to find that a course or method of investigation which has governed my own inquiries in meteorology, has been adopted on this kindred subject, by so able a hand.

The second statement objected to by the writer of the notes, that "in Peru, at the height of 18,000 feet, the wind has been found to be fresh from the south-west," was given on the authority of Samuel Curron, Esq., whose interesting narrative of his ascent to the Peak

† See Penny Cyclopædia; article, Azores.

of Misté may be found in the Boston Journal of Philosophy and the Arts, vol. i., see p. 364-5. The authorities for the Sandwich Islands and Peak of Teneriffe, which were included in my statement, are also at hand; and I may add that the Rev. John C. Brigham, the present Secretary of the American Bible Society, in crossing the Andes, from Buenos Ayres to Chili, at the height of 17,000 feet, found the wind blowing strong from the west, as he has himself informed me.*

There are other facts which seem to indicate the prevalence of a southwesterly wind over the higher regions of tropical America, such as the fall of ashes, in 1812, at Barbadoes and elsewhere, from the volcano of St. Vincent; and the late fall of volcanic ashes at Jamaica, and other places, in January of the present year, which appears to be traced to the tremendous outbreaks of volcanos on the continent, near the Bay of Honduras.

It is unfortunate that those who visit high mountains, so generally neglect to inform us of the direction of the wind in those regions. I have been led to suspect, however, that in some, at least, of the cases mentioned by me, those winds were occasioned by the diurnal influences of heat and cold upon the stratum of air *lying upon the inclined surface of the mountain*. This was first suggested by the statement of Mr. Brigham, who was informed by the natives that this wind blew only in the day-time, and it seems not unlikely that most elevated peaks are subject to a similar influence. We have, however, far better evidence afforded to us of the course of the higher strata, in the movements of the clouds at different altitudes, which should have been recorded more generally than has yet been done. The fact appears, however, to be well established, that the great trade wind of the tropical latitudes does not prevail at any great altitude, nor does it usually cross any elevated region of country, to say nothing of its being arrested or deflected by its own gravitation in mid-ocean, in the Indian seas, and on the bosom of the great Pacific. But to return to the only remaining position which is called in question by the author of the notes, and which is in the following words.

“The regular semidiurnal variations of the barometer is at its maximum about 10, A. M., and at its minimum about 3, P. M.; at New York it is nearly the same; *but at Edinburgh, the effect is reversed, the minimum being at 10, and the maximum at 3 o'clock,*’ &c. I was (says the writer of the notes) startled at this statement, and immediately began to look for authorities to confirm or refute it, when I happened on the following passage in the second report of the British Association for the Advancement of Science, p. 131.”

This is followed by a condensed statement of the observations made near Edinburgh, by Professor Forbes, since 1827, which have been published at length in the Edinburgh transactions; from which it appears that near Edinburgh, in latitude 56°, the mean annual oscillation between 10, A. M. and 4, P. M., is .0106 inches; and

* See Missionary Herald for 1826, vol. xxii., p. 153-4, where the *direction* of the wind is not mentioned.

that the St. Bernard observations, 8000 feet above the level of the sea, and those of Capt. Parry, in the Arctic regions, both indicate a true negative oscillation, &c.

I readily acknowledge a partial inaccuracy in my statement, which should have read, "but in high latitudes the effect is reversed," &c., the locality or latitude of Edinburgh not being that in which the negative oscillation is established; and the error was subsequently so corrected. It may, however, be proper on this occasion to explain the cause of the inaccuracy. A short time before I was called upon to sketch the facts in meteorology, to which the foregoing exceptions had been made, the paper of Professor Forbes, above referred to, had met my eye, under circumstances, however, which precluded its perusal; and on glancing over its pages, and the illustrations which accompanied it, I perceived that the Professor had shown a negative oscillation of the barometer, taking effect somewhere in the higher latitudes, and his extensive series of observations having been made near Edinburgh, I was led to infer that this conclusion, or result, had been directly deduced from these observations. Under this impression, I ventured to pen the statement in its original form, which I should not have done, however, unless I had at that time felt myself certain of an opportunity to give the paper of Professor Forbes a thorough perusal, before my statement should pass through the press; but the concurrence of an unlooked-for accident, with a more speedy publication than I had anticipated, prevented my design from being realized.

I have now given the authorities and explanations to which the author of the notes seems entitled, and it may not be improper to state, for the satisfaction of those who may have read my articles on the storms of the American coast,* that the method pursued by me in investigating the physical character of those storms, has been to procure a number of copies of clean charts of the Atlantic, and to map out all the facts which I was able to collect in relation to any one of these storms, upon one of these charts, in their true time and location, so as to obtain a connected view of these facts, both as regards their consentaneous and consecutive relations. The results have been highly satisfactory—so much so, indeed, that I have not met with the statement of a single fact which is at variance with the explanation which I have given of the operation of these storms, *except in two or three instances, which proved, on further inquiry, to have been erroneously stated.* The historical records of more than a century past have been freely resorted to, and the inquiry has also been extended to other coasts and seas, and has shown the existence of an unvarying system, which I have not yet attempted to describe, except in the most summary manner.

It may well be supposed that, in pursuing this inquiry by the method of a simple induction of particulars, as here stated, I have not been able to preserve an unshaken confidence in some of those "received theories," which appear to have been founded on vague gene-

* American Journal of Science, vol. xx., p. 17-51; vol. xxi., p. 191-3; vol. xxv., p. 114-121.

realizations, or unproven and untenable hypotheses; and I can hardly think that the reasonings which have at various times been adduced in support of these theories, from the time of Halley downwards, can be deemed either conclusive or satisfactory by any unbiassed mind, that shall give them a strict and impartial examination.

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. Nor is it necessary to perceive, or point out, an adequate and undeniable physical cause for the production of these phenomena, before we can discover the inconsistency and fallacy of the reasonings by which the old system of meteorology has been supported. Such a cause, however, I consider, is furnished in the rotative motion of the earth upon its axis, in which originates the centrifugal and other modifying influences of the gravitating power, which must always operate upon the great oceans of fluid and aerial matter which rest upon the earth's crust, producing, of necessity, those great currents to which we have alluded.

I have long entertained this conviction, but do not remember to have seen this great physical influence recognised in any degree, in its application to this subject, except by Sir John F. W. Herschell, in the third chapter of his popular treatise on astronomy,* where, by the aid of this rotative influence, he has been able to give us the most imposing support of the received theory of winds which has ever appeared, and in which the connection of the trades with the returning westerly winds, is, with some exceptions, correctly developed. 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 partially recognizes in connection with this and another subject of inquiry. I may also add, that, had this able philosopher been fully conversant with the facts which relate to the *course* and other phenomena of hurricanes, he would probably have withheld the hypothesis which he has given in a note appended to the chapter which I have alluded to, although one of the principal suggestions in this note has, undoubtedly, a proper connection with the subject.

As I can but seldom allow myself to enter upon the discussion of these matters, the preceding suggestions may be taken for what they are thought to be worth by those under whose notice they may chance to fall; but, to prevent being misunderstood, I freely admit that heat is often an exciting, as well as modifying, cause of *local* winds, and other phenomena, 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

* Art. 179 to Art. 200.

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 the atmosphere at a uniform temperature over the whole surface of the globe, the general winds could not be less brisk, and would become more constant and uniform than ever.

New York, April 8th, 1835.

Experiments on the Efficacy of Perkins' Steam Boilers, or Circulators. By A. D. BACHE, Prof. of Nat. Philos. and Chem., University of Pennsylvania.

TO THE COMMITTEE ON PUBLICATIONS.

GENTLEMEN,—The following experiments were made soon after the description of the new steam boiler or circulator of Perkins, reached this country; they were undertaken at the request, and for the satisfaction, of my friend, Mr. M. W. Baldwin, and I did not at the time make them public, because it was understood that this boiler would receive a full trial, on a proper scale, upon locomotives in England. From an article in a recent number of the *London Mechanics' Magazine*, I infer that the results of these experiments have not been stated, except in the newspaper paragraphs which we have seen here. As the results of my experiments were not particularly favourable to those circulators, it is probable that their publication may elicit further information in regard to the trials made in England; and at all events they will furnish, while uncontradicted by experiments on a larger scale, approximate indications of the performance of the boilers in question.

The following description of and remarks on the steam boiler of Mr. Perkins, is extracted from an anonymous pamphlet, entitled "*Steam Navigation. Improvements by Jacob Perkins. Part I. The Boiler.*"*

"To a boiler is fitted a hollow lining, not extending throughout the boiler, but in proportion to circumstances, in order that a strong circulation in the water may be caused, and that, by driving a perpetual current forcibly against the inner surface of that portion of the boiler exposed to the fire, the whole of the heat may be taken up by the water. And here must be recorded the result of farther experiments. If two vessels be filled with water, one of them placed within the other, and heat be applied to the outer vessel in such a manner that none can reach the inner vessel, except through the water contained in the outer one, no steam bubbles can be made to arise in the inner vessel, whilst, in the outer one, steam bubbles are formed rapidly. The fluid in the outer vessel being composed of a mixture of bubbles and water, that in the inner vessel being water alone, the contents of

* See also *Journal of the Franklin Institute*, vol. xi., p. 47, January, 1833.