MEMOIR

OF

WILLIAM FERREL.

1817-1891.

BY

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BIOGRAPHICAL MEMOIR OF WILLIAM FERREL.

We are assembled to think and speak of one who was known to very few by name or by face, and yet who has done more than any other single person to establish on firm foundations the mechanics of that branch of science which we call Meteorology. Since the days of Galileo men have accumulated observations of the temperature, humidity, pressure, and movement of the air for all parts of the globe. From these have been formed charts of the monthly and annual average features, and even tri-daily charts showing momentary conditions in the rapidly changing atmosphere. Innumerable studies have been based upon purely statistical methods, but whenever an attempt has been made to explain the causes of things and to trace out the interaction of the diverse forces the mind has recoiled from a strictly logical, deductive method, in view of the extreme complexity of the conditions, and has had recourse to crude hypotheses and approximations. All honor, therefore, to our colleague William Ferrel, who, animated by the conviction that no unknown or mysterious forces were present, attacked one problem after another and, by the logic of his thought and the invincible tenacity of his purpose, broke down the barriers of ignorance and opened the way for future explorers. But successful studies in meteorology could only have become possible to a mind disciplined by a long series of struggles with, and triumphs over, less complex problems: this in fact was the work of Ferrel's early life.

From his brother, Dr. Jacob Ferrel, of Kansas City, Missouri, we learn that "William Ferrel, the grandfather of our colleague, "came to America from the north of Ireland about the year 1785 "as a young man and soon married into an English family by the "name of Veach. Two children, a daughter and a son, were the "result of the union. The son was named Benjamin. His father "died when Benjamin was eleven months old. The family was "very poor, and Benjamin was put out for several years to be cared "for by strangers. When Benjamin grew up to years of maturity "he married into a German family by the name of Miller; to him "were born eight children, six boys and two girls, William being

" the oldest. William Ferrel was born in Bedford county, now " Fulton county, Pennsylvania, January 29, 1817.

"At William's birthplace there is no 'town,' not even a house to " mark the spot that gave him birth; nothing but a mound of clay, " the remains of the old stick chimney that belonged to the round " log cabin that was covered with clapboard. The place is known, " not as the birthplace of the American meteorologist, but as 'Col- " onel Knable's old saw-mill,' not a particle of which is now left to " mark the place."

From the accounts that have reached me it is evident that Benjamin Ferrel and his wife were persons of sterling worth and much talent. The father of our colleague was a man of independent judgment and of great tenacity in defending his conclusions; he recognized the mental endowments of his eldest son, and so far as practicable contributed to his education. The faithful mother allowed no favoritism to influence her feelings, but treated all alike, held each in equal esteem, and received the equal devotion of all.

An autobiography, written out a few years ago by Ferrel at the request of a friend,* shows that from childhood his thoughts were given to the reasoning out of the causes that underlie surrounding phenomena. From his boyhood to his death William Ferrel was one who could not be satisfied with anything less than the most thorough attainable study of the subject that engaged his attention. From the beginning to the end of his life he was true to his own nature and lived up to his own principles and, by the steady prosecution of long trains of reasoning, succeeded in placing upon a surer foundation his own and our knowledge of many natural phenomena.

What a profound problem in psychology is suggested to us by the reflection that, in all ages of the world, so many persons are born who instinctively give themselves to searching out nature's laws and methods. The students of evolution have endeavored to demonstrate some few principles in regard to the inheritance of scientific ability, but who shall explain, in a direct and acceptable manner, the remarkable outcrop, here and there, of individuals whose ancestry gives no evidence of the possession of those qualities of mind that distinguish the brilliant descendant. The botanist singles out a peculiar flower or head of grain and calls it a *sport*, an exceptional individual among ten thousand of its comrades. He claims that the ancestor of that

*Mr. Alexander McAdie, of the Weather Bureau.

particular plant was in every respect similar to the other plants, but that through some special influence attending the sprouting and growth of some one seed this sport has been produced. Consider this phenomenon in the material world and ask what corresponding power pervades the spiritual world, banishing intellectual sluggishness and directing our thoughts to the unseen causes of things?

Consider the problems in hydraulics, mechanics, and economics that must have occupied Ferrel's parents when in early married life their chief anxiety was to make a successful and profitable saw-mill. Those problems entered into the life of the child, and from his birth onward he was a mechanician in the highest sense of the word.

We may, then, not need to study the long line of ancestry which would lead us from Ferrel's birthplace in Bedford county, Pennsylvania, back to Ireland, England, and Germany. We have rather to assume that he inherited simply the sturdy enterprise, the obstinate perseverance that *will* conquer a success, the spirit that belongs to the pioneers of our young nation. We must turn to the material surroundings of Ferrel's childhood in order to study the development of his mind. Fortunately we are the better able to do this by means of his above-mentioned autobiography, from which it appears that before completing his elementary schooling he had, at the age of fourteen, been dividing his time between work in his father's saw-mill and work on his father's farm. Already, at this time, he was conscious of a special interest in the scientific items of the weekly newspaper and in the problems of his arithmetic, and was considered the best scholar among the boys of his age.

It was in the summer of 1832 that his boyish attention was arrested by the sight of a book on mensuration, with its geometrical diagrams, and soon afterward by the partial eclipse of the sun, which must have been visible where he then lived, near Martinsburg, West Virginia, from 7 a. m. to 8 a. m. on the morning of Friday July 27, 1832. Certainly few youths have ever, at the age of fifteen, been excited to study out the laws governing the occurrences of eclipses as was William Ferrel on this occasion. His account of the rapid progress made by him in co-ordinating the celestial phenomena is so unique that it must be commended to the admiration of all his friends. The result was that by the end of January, 1834, he had finished his own computations of predictions of the eclipses for the next year, 1835, all of which were remarkably well verified, considering that the only basis for these predictions was the numerical data obtained from old copies of farmers' almanacs, out of which he wove his own theory of the motions of the sun, moon, and earth.

The seventeenth celebration of his birthday, at the close of January, 1834, was marked by his first insight into a treatise on trigonometry and surveying, and although its study required a knowledge of geometry, which he had not yet acquired from books, and had to be prosecuted in the midst of every-day work on the farm, yet before the summer was ended his divine enthusiasm had overcome every obstacle. The work on these problems in surveying evidently suggested the idea of satisfying his unquenchable thirst for further knowledge by a visit to a famous land surveyor, an old Englishman, who lived some miles away in the mountains of West Virginia, from whom he obtained a copy of Simpson's Geometry. A year later, namely, in the winter of 1835-'36, two days were given up to a journey in midwinter to Hagerstown, Maryland, in hopes of purchasing Playfair's Geometry, the study of which, by the light of a blazing fire of lightwood or by the dim light of a tallow candle, occupied the rest of that year.

It was in the spring of 1837, at the age of twenty, that Ferrel first read of the law of gravitation and of the elliptical orbits of the planets and their satellites. It was still two years more before he had saved money enough to defray his expenses in a summer school of mathematics, at Marshall college, Mercersburg, Pennsylvania. Here, among other things, he for the first time saw a treatise on algebra and devoured it.

As Ferrel's course of study was a complete inversion of the ordinary methods of studying mathematics, I am led to remark that it is by no means evident that the order of studies generally pursued in our schools, namely, "arithmetic, algebra, plane-geometry, trigonometry," is that which is natural and easy for the youth or calculated to develop a useful efficiency in these acquirements. There is much to be said in favor of the early introduction into a boy's hands of the little treatise on geometry, by Herbert Spencer. It is very plain that Ferrel's passage from celestial motions and geometry through trigonometry and algebra up to the law of gravitation, and eventually to the differential calculus, was entirely in accord with the history of the progress of mathematics and with the needs of his own intellectual nature.

Until Ferrel entered college his course of reading in mathematics was, as I have before hinted, the outcome of external circumstances. He had not been put into the hands of a professional trainer of the mind and forced to follow some conventional system, but as problems had presented themselves, whether suggested by the saw-mill or the eclipse or the surveying operations, he had worked away upon their solution by such methods as his own ingenuity invented. The same characteristic is found to pervade the greater portion of his subsequent career.

In justification of this we note that Ferrel's college education continued at irregular intervals from the spring of 1839, at Mercersburg, Pennsylvania, until after his graduation, in 1844, at Bethany college, West Virginia, after which, and as he has often told me, not knowing any better method of supporting himself, he followed the pioneer instincts and traditions of his family and settled at Liberty, not far from Kansas City, on the western edge of Missouri. In this frontier town he discovered one day, not displayed for sale, but put away on a back shelf, a copy of Newton's "Principia," which had been left in the village store by some wandering schoolmaster as payment for an unsettled bill.

This marvelous accident (for who could expect to have found that priceless volume upon a western prairie?) saved Ferrel from being lost to science. During the succeeding three years, so far as his time and health permitted, he must have mastered Sir Isaac Newton's great work that stands at the portal of modern science. Only the logical Newtonian mind could grapple with the intricate problems that were about to enchain Ferrel's attention. He states that his studies of the tides began with Newton and suggested to him at once that the tides must tend to retard the earth's rotation.

It must have been about 1850, when teaching at Allensville, Todd county, Kentucky, that it happened, as he himself has told me, that he learned that some one by the name of Laplace had written something upon the subject of the "tides" which he desired very much to read. He therefore requested the country merchant to purchase him a copy when next he visited Philadelphia, for in those days, much more than now, western merchants made their annual journeys eastward by river, canal, and horseback to purchase their supplies. Bowditch's translation of Laplace's "Mecanique Celeste" proved a much bigger work than Ferrel had expected, but nothing daunted, in fact delighted to have this Thesaurus in his possession, he set about conquering its difficulties. How completely he made both Newton and Laplace his own was shown in less than four years by his first published scientific paper on "the effect of the sun and moon upon the rotary motion of the earth" in Gould's Astronomical Journal. This memoir was dated at Allensville, Kentucky, September 24, 1853, and in it he for the first time gave a mathematical and numerical demonstration of the effect of the tides in retarding the rotation of the earth and the moon. Mayer had preceded Ferrel, but his memoirs were at that time unknown to the scientific world.

During the years 1854 to 1857 Ferrel lived at Nashville, Tennessee, where he established a private school. Here, by another fortunate providence, he added Airy's writings to his small but classic library and wrote his own second paper, also published in Gould's Journal, "On the problem of the tides with regard to oscillations of the second order," which was dated Nashville, May 24, 1856. Here also he wrote for his friend, Dr. W. K. Bowling, the popular essays that we have of late years republished, like monuments disinterred from the sands of Egypt, for modern students to admire. Our colleague now gave up teaching and welcomed the opportunity, brought to him through the kindness of Dr. B. A. Gould, to remove to Cambridge, Massachusetts, where he began that life of entire devotion to science which could alone satisfy his nature. During the thirty-five remaining years of his life his attention was principally given to the cognate problems of the motions of the ocean and of the atmosphere in connection with the rotation of the earth.

To have devoted so many years to one class of problems would seem to imply a very partial study of nature, a very one-sided development of the scientific life, but we can hardly say this of Ferrel. Indeed, every one who starts to investigate astronomy or physics or geology or biology must come back to the study of the atmosphere and the ocean, through the presence of which these various manifestations of nature have become possible. Ferrel was perfectly conscious that, by mastering the simpler phenomena relative to the movements of the ocean and the air, he was laying a solid foundation for all future progress in studying the past and future history of the globe and its inhabitants.

The preceding sketch shows that it is not improper to consider that Ferrel's courses of reading and study were largely suggested and controlled by circumstances outside of himself. Had not Newton, Laplace, and Airy successively and accidentally come into his hands at the right time terrestrial physics would have waited for another master. It was a further interesting and accidental coincidence that while he was studying what these authors had to say the attention of the whole civilized world was fixed upon the brilliant experiments of Foucault, showing the rotation of the earth by his pendulum and gyroscope. The daily press teemed with scientific items, showing the awakened interest in the study of these subjects. Ferrel himself contributed, by his writings in the Nashville Journal, to the popular education on this subject.

The consideration of the important practical results attained by the study of the physical sciences delights us but little more than the study of the mental processes by means of which the student attains the solution of a problem.

Ferrel's habits of thought were pre-eminently of the deductive style. Twenty years of devotion to geometry and mechanics had thoroughly imbued his mind with all the consequences that must flow from that fundamental law of nature, "action and reaction are equal." He had acquired that additional seventh sense, the perception of force, to such a degree that he could quickly decide whether a proposed explanation was satisfactory or not.

Since the days of Sir Isaac Newton the importance of deductive theories has come to be more and more thoroughly appreciated in science. Based upon a few simple and well-established laws of force, the deductive theory may go astray through the intentional neglect of some residual quantity, and is therefore always checked by the appeal to experiment and observation. If observations prove wholly irreconcilable with theory, then it is probable that these neglected quantities or neglected laws of nature are disturbing the simple results of the fundamental principles originally considered. These latter, therefore, must be combined with the former. By this process a more complete theory is built up; every step of its development is deductive, and the final demonstration of its acceptability is found in the agreement of its results with observed facts. This was Ferrel's sole manner of work. He developed a theory of the gyroscope or rotascope, of vision and of the stereoscope; of the irregularity in the period of Algol; of radiation; and then experimented himself or collected data from others to test his theory. He filled out Newton's and Laplace's tidal theories and announced the resulting retardation of the earth's rotation, and then waited for facts to confirm it. He showed that, under any possible law of friction, Laplace's solution for the tides on an uniform ocean covering an ellipsoid of revolution must be erroneous, and then by studying the observed tides confirmed the corrected conclusion. He even dared to apply his knowledge of the tides to the most obscure problem that at present vexes terrestrial physics, namely, the magnetism of the earth, and to predict that the internal motions of a viscous earth would in some way contribute to the explanation of the phenomena.

We therefore recognize the same deductive style as pervading all his work in meteorology. It is not likely that a sudden flash of genius ever revealed to him the possible explanation of a perplexing fact. On the contrary, he was always mentally pursuing his deductive theories out to their remotest consequences and was looking for the phenomena which he knew ought to exist. If in any respect the analytical method was too difficult to give satisfactory results, he resorted to graphic methods, and, if need be, when these failed he deduced only in a general way the logical conclusions that followed from his premises, but in every case it was deduction checked by observation. One of his boldest essays in meteorological predictions was made in 1862, when, after analyzing what little we then knew of the meteorology and geography of the interior of Africa, he predicted that if ever we came to understand the subject more thoroughly we should find that the true cause of the annual inundations of the Nile lay not in the rainfall of the neighborhood of Lake Nyanza, but of a region ten to fifteen degrees north of that, where there must be some other large tributary.

A characteristic trait of Ferrel is illustrated by the evidence that many of his writings remained unpublished for a long time, until he could be persuaded that they were desired and needed, and most of his publications were drawn from him by the urgent requests of friends. His early papers of 1855 on "Vision," and again on "The Variable Star Algol," also his first essay on "Winds and Currents," and his subsequent paper on the "Motions of Fluids and Solids," were all prepared for publication in response to such requests. The paper on "Vision," having been published in a journal unknown to the physicists of Europe, has, I believe, hitherto not been mentioned even in the bibliographies of that subject. It does not even appear in the "Index Catalogue of the Surgeon-General's Library" under Ferrel's name, but will probably be quoted in the forthcoming volume under "Vision." In this essay Ferrel worked out an original explanation of the mathematical and psychological processes by which we recognize the direction, distance, size, and shape of an object, and demonstrated its truth by experiments that suffice to give it a high rank among the theories that are worthy of the study of the oculist and psychologist. He also showed the errors of current theories in so far as he was acquainted with them, and applied his own views to the construction of the stereopticon and other pieces of apparatus.

The observation of the brightness of the variable stars is an important branch of astronomical work that has been specially furthered by Argelander and by our colleague, Dr. B. A. Gould, through his admirable astronomical journal. At Gould's request Ferrel wrote an appeal to astronomical amateurs in the Southern States, calling attention to the interest that attaches to this class of observations. Ferrel could not accept any facts of observation without attempting to gratify his inborn desire to explain the causes of phenomena; he therefore illustrated the interest that he felt in the subject of the variable stars by quoting Argelander's latest results and showing that the shortening periods of fluctuation of Algol must occur if the star is approaching the earth with a steadily accelerated velocity, and, inversely, that if it is receding from the earth the periods must lengthen. This, it will be noticed, is not an attempt to show that the brightness depends upon the distance, as was suggested by Hahn in 1798, but is a combination of Riccioli's hypothesis of 1651 as to the rotation of a spotted sun with Ferrel's own deduction from the known velocity of propagation of light. Fortunately in these days of spectroscopic analysis we are able to test the correctness of Ferrel's hypothesis, since if the star is approaching us while the periods are diminishing, and receding from us while they are increasing, then the displacement of the spectrum lines due to this motion will probably be measurable. The spectroscopic measurement of the approach or recession of Algol should agree with the velocity deduced by Ferrel from its photometric periods, and both combine to determine the dimensions of its elliptical orbit. Ferrel's hypothesis of 1855 has been abundantly verified by the recent work of Vogel in Berlin and Chandler in Cambridge.

It would be difficult in this short biographical sketch to do justice to the many items in regard to which Ferrel's studies have a decided scientific importance. The smaller works, such as those on "Vision" and "Variable Stars," although but little known, lead us to logical conclusions that are eminently worthy of consideration. The greater works may be classified as those bearing, first, on the rotation of the earth; second, on the tidal problems; third, on meteorological questions.

Dating from his acquaintance with the works of Laplace, Ferrel began to be not merely an independent investigator, for that he had always been, but one who was generally in advance of the rest. He became a pioneer, "blazing" paths for the scientific progress of the age, and worthily wielded the implements he had so directly received from Newton and Laplace. His public scientific life-work began with studies upon "the tidal effect of the sun and moon upon the rotation of the earth," the similar effect of the earth on the ancient liquid moon, and, again, on the "influence of the tides in causing an apparent acceleration of the moon's mean motion." The problem of the rotation of the earth and the position of its polar axis had hitherto been studied on the assumption that we have to deal with a homogeneous, rigid body. It had been, as was thought, clearly demonstrated by Laplace that the inter-action between the earth and the atmosphere or the ocean could not affect the earth's rotation, much less terrestrial latitudes or longitudes. The clear perception of our colleague taught him that if there were such a thing as friction or any similar resistance within the ocean, then that must cause a gradual diminution of the rate of rotation, such that at the present time the day is somewhat longer than in former ages. This should produce an apparent acceleration, which is added to the real acceleration of the mean motion of the moon or, in fact, of any other celestial body. The independent demonstration of this principle by Delaunay was announced some time after the publication of Ferrel's paper of 1864, but the history of Ferrel's connection with this problem shows that he was in possession of this principle many years before. He himself has told us of his diffidence in presenting any paper whatever for publication and how, week after week for several months, he carried this written memoir in his pocket to successive meetings of the American Academy before he could summon courage to read it as his first contribution to that society.

The acceptance by astronomers of the general idea or principle that the earth's rotation could be affected by any appreciable interaction between its solid and liquid portions (in view of the fact that Laplace had considered it negligible) was as much of an epoch in physical astronomy as the subsequent works of Thomson on the "Tides on an Elastic Sphere" or of Darwin "On the Tides in a Viscous Globe." Undoubtedly there are some phenomena that must be attributed to the earth's plasticity and viscosity, and others that must be attributed to the earth's elasticity; but we have not yet fathomed all the phenomena that are characteristic of the earth viewed as a rigid solid with a fluid covering. The theory of the rotation of a rigid ellipsoid of revolution as it left the hands of Poisson in 1838 was crystallized by C. A. F. Peters in 1842 into the formulæ of his *Numerus Constans Nutationis.* Since then Peters, Clerk Maxwell, Gyldén, Nyrén, and doubtless others, have extended the laws of rotation of a symmetrical, rigid earth, first, to Euler's case, when the axis of rotation does not agree with the principal axis of inertia, giving rise to the ten-month periodic variation of latitude; second, to cases in which, owing to internal disturbances a given, rigid earth suddenly or even slowly changes into another mechanically different rigid earth (by reason of some alteration in the distribution of its mass and therefore of its axes of inertia), in consequence of which other periodic changes of latitude are brought about. All these studies assume a nearly symmetrical ellipsoid of revolution whose figure is determined by the general distribution of terrestrial gravity. From some notes that have come into my hands I infer that Ferrel had undertaken the collection of data preparatory to the solution of the question: "What is the effect upon the rotation of the earth, considered as a rigid ellipsoid, of the attraction of the sun and moon upon continents and oceans, considered as rigid masses irregularly superposed upon the assumed symmetrical ellipsoid?"

I have elsewhere indicated my belief that if he had lived to finish this study he would have elucidated two problems that are at present actively engaging the attention of the world. He would, namely, have shown that on the one hand these great superficial irregularities produce complex, periodic terms to be added to the ordinary formulæ for precession and nutation which we now begin to recognize as changes in latitude. On the other hand, the immense forces involved in the regular phenomena of precession and nutation, as well in these irregular changes in latitude, produce a system of strains throughout the earth by virtue of which the strata are alternately compressed and stretched. Sometimes these strains are sufficient to cause fractures, but they are always sufficient to develop the phenomenon known as piezo-electricity, and these piezo-electric currents are the ultimate cause of the electrified condition of the globe and its atmosphere and in part of its terrestrial magnetism.

These studies into the action of the sun and moon upon the rotation of a rigid globe do not in any way render unnecessary the profound investigations into the phenomena attending the rotation of an elastic earth or of a viscous globe. The earth is really both rigid and elastic and viscous, for these three terms do not relate to properties of matter *per se*, but relate to the phenomena exhibited by that matter when acted upon by exterior forces during short or long periods of time. Our globe, whirling rapidly around, carrying a perturbing mass on its surface, is by the latter subjected to a daily strain, both radial, longitudinal, and latitudinal. It is the repetition of this strain, day after day, and especially its periodic increase with conjunctions of the sun and moon, that eventually produces appreciable effects.

We cannot too deeply regret that Ferrel was not spared to finish the discussion of these problems upon which he had already thought and written in his "Tidal Researches" in 1874. The last paragraph of this great work gives us the first published rational basis for the so-called Halley's theory of the secular changes in the earth's magnetism. Although it is probable that this theory is far from representing nature, yet it is very indicative of the wide range and boldness of Ferrel's thoughts to find that he has been logically led to dwell upon it.

The second general field of work in which Ferrel engaged was the study of the tides as such. This subject he developed, both analytically and numerically, and finally even mechanically, so that his machine for the mechanical prediction of the maxima and the minima of the tide remains to this day in constant use at the office of the Coast and Geodetic Survey. This apparatus was devised quite independently of the harmonic analyzer of Sir William Thomson and fulfills a different purpose, although in some respects they have similar features. The date of its actual invention does not appear from Ferrel's papers, but merely the statement that the plan in its final shape was submitted to the Superintendent of the Coast Survey in 1880. I happen to know, however, that already in 1861, when I was in daily personal intercourse with Ferrel in Cambridge and when he was earning a scant livelihood by computing lunar and planetary ephemerides for the Nautical Almanac, he was even then speculating on some combination of wheels that should save all this unnecessary mechanical computation, so that I presume the idea of the computing machine for calculation of ephemerides was an early inception, and that its growth in his own mind was gradual until the times became favorable for the actual realization of a similar tidal machine.

Ferrel had occasion in 1856, and again in his "Tidal Researches" of 1874, and in the Philosophical Magazine of 1876, to announce his own independent personal conviction in reference to a point that has given rise to much discussion, namely, the proper determination and interpretation of certain arbitrary constants that appear in the development of the tidal function into a series of periodic terms. His views were opposed to those expressed by Sir William Thomson, but Ferrel's letter of November 17, 1875, as printed in the Philosophical Magazine of February, 1876, could not have been drawn from him easily, so averse was he to any appearance of controversy. He had not the least desire to pose before the world as a wrangler over scientific points on which his own convictions were clear and settled. Nothing better illustrates Ferrel's innate shyness than the fact that both in 1876 and again in 1884, when Sir William Thomson (now Lord Kelvin) visited Washington nothing could induce him to voluntarily call upon one for whom he had such profound respect and yet with whom he had had occasion to differ so decidedly, for fear lest some constraint might be felt by either one during such a meeting. However, a meeting was brought about in 1884, without his connivance, and, to Ferrel's profound delight, the conversation was as unconstrained as could be desired. In the last year of his life, having perceived that his views as to the errors of Laplace and Thomson were still unappreciated in England, Ferrel returned to this subject, and in two papers in Gould's Astronomical Journal for 1889 and 1890 showed, as it would seem, with conclusive clearness that in the presence of frictional resistances and for oceans of finite depths any initial tidal motion would be gradually annulled, and that practically no tides are observed by us except those that are directly maintained by the sun and moon, or the so-called forced tides as distinguished from free tidal waves due to possible initial motions. On the other hand, Ferrel showed that if no friction exists, even then the free tidal waves disappear, except in the case of an ocean of a certain critical depth such that the waves may coincide in period with the forced diurnal and semidiurnal tides, in which case the tides may, of course, have any value whatever.

The third class of Ferrel's important works relates to the subject of meteorology. It is probable that his attention to the subject of storms as such was, in a general way, excited early in life by the wide-spread dissemination of Espy's theories (see Popular Science Monthly, April, 1889, xxxiv, p. 834). These latter were gradually developed during the interval from 1820 to 1850, and Espy's delightful lectures throughout the country, as well as the heated controversies maintained by his opponents, must have been known to Ferrel.

But Ferrel expressly tells us that his active interest in the problems relating to the general circulation of the atmosphere was more especially developed by reading Maury's "Physical Geography of the Sea." (Ferrel refers apparently to the second edition, dated December, 1854, and published in 1855. His own article was published in October, 1856.)

Ferrel's first meteorological essay utilizes a characteristic, discreet selection of well-established facts taken from Espy's third report, from Redfield's writings, and Maury's "Physical Geography of the Sea." He explicitly rejects numerous hypothetical explanations which are inappropriate and unsatisfactory, and shows that the general laws of winds, pressures, and storms are deducible from a few simple principles. In those days it required close study and a very clear insight into the subject and a very independent mind to prevent one from becoming a partisan either of Maury or of Espy or of Redfield, not to mention Dr. Hare, the electrician of Philadelphia, or Dove, the statistician of Berlin. Precisely such a mind we find in Ferrel, who, from his surroundings in Nashville, was enabled to look out upon the world of meteorological disputants and to insist upon logic and reason in place of a war of words. In reading this clearly written essay we realize that his perception of the relative magnitude of the forces involved enabled him to select the more important results of meteorological observations and to combine these with laws of nature that his own researches convinced him were equally important, although hitherto hidden from scientific vision. We may in later years, with Ferrel's help, have in some details progressed beyond his early essay of 1856 on "The winds of the atmosphere and the currents of the ocean," but in all its important features it still stands as the first satisfactory general explanation of the atmospheric and oceanic motions based upon a proper consideration of the effects of density or buoyancy in producing currents and of the rotation of the earth in modifying these when once started. This essay, which explains not only the winds, upper currents, and hurricanes, but the distribution of barometric pressure, depending upon the winds, although it is free from mathematical formulæ, is yet based at every step upon the formulæ that he subsequently published. This memoir was practically unknown to meteorologists until inserted in the collection of popular essays which I prepared as "Professional Paper No. XII of the Signal Service." A similar unfortunate fate seems to have befallen Ferrel's mathematical paper of 1858 and its special pamphlet edition of 1860, since the "Mathematical Monthly," like the "Nashville Journal of Medecine and Surgery," were not periodicals likely to circulate among meteorologists. Although the special edition of 1860 was originally sent by Ferrel to those who he thought would be interested in it, yet I believe the first published reference to this work is to be found in appreciative notes introduced by Professor Everett into his translation of Deschanel's Philosophy, ten years afterwards. Since that date Archibald, in England, and Sprung, in Germany, have contributed to give a further impulse to the effort to make Ferrel's researches more widely known, and at the time of his death it could safely be said that he had been recognized throughout the world as having given to the science of meteorology a foundation in mechanics as solid as that which Newton laid for astronomy.

When I came to the assistance of the Signal Office in January, 1871, it was my first care on the one hand to apply Ferrel's methods of looking at the atmospheric motions to the maps and predictions that were made by me daily. In a little pamphlet written in April, 1871, I quoted the general results of his studies, so as to make them more widely known, but especially was I solicitous to secure his own further attention to the meteorological problems that were now daily before us. He was, indeed, at that time too much engrossed in his tidal researches to allow of an immediate return to meteorology, but a few short papers in 1871 and 1874 show that he did not neglect to read and study upon this subject. His principal thought between the years 1867 and 1875 was undoubtedly given to the tides. At the close of these tidal studies and while engaged in the construction of his tide-predicting machine he took up a new work for the Coast Survey entitled "Meteorological Researches for the use of the Coast Pilot." This work was published in three parts as appendices to the successive Coast Survey reports for the years 1875, 1878, and 1881, but the actual dates of finishing and publishing these appendices were 1878, 1880, and 1882 respectively. The composition of the first of these three must have been in the year 1875.

We may therefore consider that Ferrel's work in meteorology was renewed in 1875, and continued unabated during his employment by the Coast and Geodetic Survey and subsequently while attached to the Signal Office under General Hazen, from August, 1882, until his resignation, in September, 1886.

Problems in tides and currents, rotation of the globe, flow of the atmosphere, even those in terrestrial magnetism and gravity, or internal heat and viscosity, are all apparently correlated in such a way that the same mathematical formulæ are everywhere being applied to certain modifications of the same physical principles. The analysis that applies to hydro-dynamics also applies to electricity, and will eventually resolve the problems of elasticity. Therefore I say that Ferrel, having once mastered the methods founded by Newton, Laplace, and Airy, found in the atmosphere a further field for the application of these.

We may rapidly review a few of the problems that Ferrel has elucidated in meteorology. His attention was first drawn to the remarkable fact that the barometric pressure is less in the equatorial zone than at a greater distance from the equator, whereas near the poles it is again low. This phenomenon he deduced as a simple result of the theorem that every motion along the surface of the earth is accompanied by a deflection to the right or a tendency to such deflection in the northern hemisphere, but to the left in the southern hemisphere. This general proposition had, unknown to Ferrel, been announced by Poisson and may have been known to Laplace—it was even known to Tracy, of New York; but, ignored and forgotten by all the world, it became in Ferrel's hands the key to several unexplained phenomena and is now quite frequently known as "Ferrel's law." From this law Ferrel first deduced the relation between the winds and the barometric gradients and gave us the means of determining what is called the coefficient of resistance of the surface of the earth to the winds. He was thus able to compute approximately the general velocity and character of the currents in the upper portion of the atmosphere inaccessible to observation by means of clouds or even balloons, and showed what intimate relations there must be between these upper currents and the development and motions of general storms. He demonstrates the true cause of the low barometer about the south pole and the lesser depression at the north pole as resulting from the deflecting action of the revolving earth. He shows why the pressure is low at the equator and especially why the barometric oscillations are so regular in that region. He explains how it is that revolving hurricanes have a tendency to move toward the poles on account of internal moving forces, aided further by the distribution of aqueous vapor and the use of warm buoyant air in advance. Inversely anti-cyclones must move from the poles. He shows the mechanical cause that underlies the formation of the belts of calms near the tropics. All this was in or before 1860.

When Ferrel again took up meteorology, in 1875, he was thoroughly imbued with the development that Thomson, Peslin, Reye, and Hann had given to Espy's condensation theory. He elaborated the study of the phenomena of cyclones and anti-cyclones in an elastic moist atmosphere subject to friction and applied the theory to the formation of thunderstorms and tornadoes as well as to extensive hurricanes. Concerning this work meteorologists throughout the world may well adopt the language of Prof. Wm. M. Davis: "The student may search the literature of the science "through and through; he will find nowhere else any adequate "consideration of the cause of the terrific blast of the tornado. "He may wander from one unsatisfying theory to another as the "doubter wanders from creed to creed, finding no rest for his unhappy disbelief until at last he reaches the true faith, on which "he rests with confidence and comfort."

There is one feature of Ferrel's work on the origin and maintenance of storms that was overlooked or misapprehended by most of us until attention was called to it a few months before his death, and undoubtedly our imperfect apprehension of his own clear conceptions troubled him not a little during the sickness that clouded the last year of his life.

So fully had he developed the action of the latent heat of condensing vapor that we forgot that he had in mind any other possible source of disturbance, so that when Hann stated the apparent insufficiency of this heat, and when Siemens, Oberbeck, and possibly others stated that apparently the general circulation of the atmosphere by generating large whirls may initiate a cyclone, or when Helmholtz showed that the trough of an atmospheric wave may give rise to expansions, coolings, and clouds, we all welcomed the suggestion as apparently helping us out of certain difficulties. But it now seems that Ferrel had long since investigated the effect of the general circulation and had laid it aside as of minor importance before devoting himself to the development of Espy's theory. He had shown its efficiency in producing the antarctic whirl, but its unimportance in ordinary cyclones. It may be that with a better knowledge of the mechanics of the atmosphere we shall eventually be better able to show to what extent our storms depend upon moisture, and to what extent on the general circulation of the air, and to what extent on the local circulations, but it is not likely that we shall be able to overturn Ferrel's formula expressing the relation between the wind and the barometric gradient, and which shows that the general circulation can only produce very moderate high pressures and low pressures, and that it needs to be supplemented by the heat of condensing vapor before we can get at the explanation of our storms. A slight fall of pressure, due to the general circulation, may decide as to the time and place where a storm is to originate, but cannot explain its subsequent development. Even the radiation of heat by the air, about which we have lately learned a little, is mentioned by Ferrel as a factor to be considered in studying areas of high barometer and cold air, but of its numerical value he had no exact data and classed it with other matters secondary to the aqueous vapor that he was studying.

Intimately connected with the origin and maintenance of the storm is the question of its progressive path from day to day. On this matter Ferrel maintains that the motions of the cyclones must be principally controlled by the general circulation of the atmosphere, which will carry them westward within the tropics, but eastward in the temperate zones. He had, indeed, shown that there is a slight excess of pressure within a cyclone, tending to push it away from the equator, and others had found that the distribution of temperature or of rain seemed to control the motions; but Ferrel concludes that all these are of less importance in comparison with the general circulation of the atmosphere. By this latter expression I understand that he includes, not merely the translatory motion, but the general whirling motions of the atmosphere so far as they depend on the difference of density of the air in the warm equatorial and the cold polar regions.

The motion is similar to that of a river in which, when we see little whirls traveling down stream, we say they are carried by the general current down stream as though the whirl had no influence on the stream; but when that same river swells to a mighty rush of water, in which enormous whirls occur, we think of these whirls as the characteristic feature of the river and as having so great an influence upon the flow of water that we can hardly bring ourselves to speak of the whirls as being carried along *by* the general current of the stream, but would rather say they are carried along *in* the general current of which they constitute an integral part. In this latter way Ferrel thinks of the general circulation of the air from the equator to the poles. Hurricanes, thunderstorms, and tornadoes, cold waves and high pressures, all form parts of this general circulation, and the cyclonic eddies are said to be carried along *in* it or *with* it rather than *by* it. It does not appear that Ferrel has neglected to consider these important factors. The so-called "new views" had all been considered by him twenty years ago. It was not his habit to publish his explanation of a given phenomenon until he had also tested all other explanations of all its important features. His mechanics of the general currents of the air and of the motions within cyclones and tornadoes will undoubtedly stand the criticisms of future times as safely as will the results of his studies on the tides of the ocean and the rotation of the earth.

The last ten years of Ferrel's life were years of great productiveness in the development of his views on meteorology. His larger works were his "Meteorological Researches," his "Recent Advances," his unpublished course of lectures, and his "Popular Treatise on the Winds;" but his minor works were also important, such as his "Psychrometric Formulæ and Tables," his "Barometric Hypsometry" and the allied "Method of Reduction to Sea-level," his studies on the "Arago-Davy Actinometer" and on the "Law of Thermal Radiation."

In Ferrel's every-day life there seemed to all who knew him to be a certain amount of isolation too large to satisfy us, and he himself felt this. Preoccupied by problems, and therefore necessarily alone with his own thoughts, he had, even from boyhood, neglected the development of the social side of his nature. It was only when we searched him out and interrupted his studies that we found him kind and helpful and appreciative of human intercourse. It is only after we have lost him that we fully realize the privilege that we have enjoyed, now these thirty years past, of appealing to him at any time.

Comparatively few found opportunity to intrude upon the uniform simplicity and retirement of his life. Unmarried, and generally far from his relatives, he passed his days in a systematic regularity from which he knew that he must not depart if he were to master the problems of the ocean and the air. A half hour daily in the parlor of the friends with whom he lived, or a short call upon others whom he held in esteem, broke up the steady current of his thoughts sufficiently to keep him in good condition for mental work.

Much affection and care did he bestow upon his near relatives. He seems to have been very sensitive to the claims of kinship. Twenty-five years of faithful service were first given to the family homestead; then followed fifteen years of study and teaching in the West; after this he resigned himself entirely to the promotion of the sciences, a study to which he brought those habits of steady and intense mental work that he had learned at home.

From 1856 to 1886 thirty years of hard work well done passed smoothly by. He then voluntarily retired from official life, thinking to live quietly with his brothers and sisters. Five years were thus spent. Beloved by them and appreciated by all, he expired at their home, at Maywood, Kansas, September 18, 1891, and lies buried in the cemetery of that town.

Monuments are often erected to the memory of distinguished scientists, but I know of none that would be more appropriate to Ferrel than a complete edition of his scattered publications by which to perpetuate his name and influence among the philosophers of the future.

AUTOBIOGRAPHICAL SKETCH.

(Written in January, 1888, by William Ferrel at the request of his friend, Mr. Alexander McAdie.)

I was born of humble parentage, in Bedford (now Fulton) county, Pennsylvania, on the 29th of January, 1817. My father's father was of Irish descent, as indicated by the name; his mother was English. My mother was of entirely German descent. I commenced going to school at the usual age, to the schools of the neighborhood, which, however, were of a very inferior order, the teachers mostly being able to teach only reading and writing and a part of the arithmetics. It was thought that I made very rapid progress, and I soon had the reputation of being the best scholar in the neighborhood.

While we lived in Pennsylvania my father was interested in the lumber business and a saw-mill.

When I was twelve years of age he bought a farm in Berkeley county, Virginia (now West Virginia), to which we moved in the spring of 1829. Although we moved entirely across the State of Maryland (here only three miles wide), the whole distance was only fifteen miles.

I was now kept closely at work on the farm, but I went to school here two winters and completed my common-school education.

The school-house was an average one of the country at that time—a rude log cabin with oiled white paper instead of glass for window panes. My last teacher took me through arithmetic and the English grammar.

Having now mastered my school books, I had nothing to engage my mind in the way of study and nothing to read except the weekly Virginia Republican, published at Martinsburg. This I was always very anxious to see, and could scarcely wait for its coming to hand, mostly for the scientific items in it which I occasionally found.

About this time I saw somewhere a copy of Park's Arithmetic, with a short sketch of mensuration at the close. At the sight of the diagrams I was at once fired with an intense desire to have the book; but I had no money, and at this time I was too diffident to ask my father for even a half dollar or to let him know that I wanted the book. Some time afterward I earned fifty cents in the harvest field of one of our neighbors, and with this I determined to buy the book. The first time I had a chance to go to Martinsburg I inquired at a store for the book, but the price was sixty-two and a half cents. I told the storekeeper I had only fifty cents, and so he let me have it at that price. It was a light task to learn all that was in it.

On the forenoon of the 29th of July, 1832, as I was going to the field to work, I observed that the sun was eclipsed. I had not known that such an event was going to occur. This excited me and set me to thinking. I had not read or thought anything about astronomical subjects before, but knew somehow that an eclipse of the sun was caused by the moon's passing between it and the earth, and that the lunar eclipses were caused by the moon's passing through the earth's shadow. I had nothing to aid me in the study but one German calendar, such as farmers use, and a copy of Adams' Geography, in the appendix of which there were solutions of problems on the globes. From these latter I learned the relation between the equator and the ecliptic and the signs of the zodiac. In the former I traced the motions of the sun and moon. The times of the sun's entering the equinoxes and solstices were accurately given; for the other signs of the zodiac only the day was given. I discovered that the motion of both sun and moon in their orbits was irregular, and that it was greatest and least at opposite points.

My theory was that the earth and moon move with uniform velocity in circular orbits, and that those orbits were eccentrically situated with regard to the sun and earth. With regard to the moon's path, I knew that it crossed the ecliptic, but I did not know at what angle, and I also at first supposed that the node was fixed. At the beginning of the next year, when the next calendar came to hand, I discovered from the predicted eclipses that it must recede. I saw from the calendars that there was some cycle of nineteen years, and suspected that this had something to do with the moon's node. This would make the node recede about nineteen degrees in a year, and this was what the next year's eclipses seemed to require.

I now found calendars in the neighborhood for a few of the preceding years and studied the eclipses and traced the motions of the sun and moon through all of them. I now commenced to make out a solar ephemeris, not from my theory, however, for my mathematics then were not adequate, but from the calendars. Except for the four points named, the time to the nearest day only was given for the times of the sun's entering the signs of the zodiac; but, taking advantage of the shifting caused by the leap-years, I was enabled to fix these times within one-eighth of a day, or the places within 7′ of an arc. I thus made out my rude ephemeris for one of the four years and then made the proper allowances for change of place for the other years, corresponding to the beginning or middle of the day.

In the calendars the places of the moon each day were given to the nearest degree. In tracing the moon's motions I perceived that the angular velocity was not only different in different parts of its orbit, but that it did not perform complete revolutions in equal times, occasioned by the changes of the places of perigee and apogee, which were given in the calendars. I perceived, however, that it performed fifteen revolutions and forty-two degrees over in very nearly equal times (very nearly 413 days), and this corresponds approximately with fourteen conjunctions of the sun and moon. Commencing, therefore, with some conjunction or opposition of the sun and moon, new moon and full moon, and counting forward 413 days and a little over, the exact period having been determined, and knowing the moon's position at conjunction from my rude solar ephemeris, I could locate the moon in its orbit at the end of that time and also, from my solar ephemeris again, the sun's position at that time. This brought the moon within about six degrees, a little more or less according to the season of the year, of a conjunction with or opposition to the sun according as I started from the one or the other. For these few degrees I could compute pretty accurately the time required for the moon to pass over them, taking into account its varying velocity with its different relations to perigee or apogee, and also determine the time when the moon would overtake the sun, or, in other words, the time of new or full moon. Knowing the places of the moon's nodes and consequently the time of the sun's being near them, I could go back to some new or full moon the year before, which was about 413 days before, and compute the new and full moons at or near the times of the sun's being at the nodes and so determine the central times of the eclipses. I had to determine the angle of inclination of the moon's orbit to the ecliptic and also the places of the moon's node, allowing an annual recession of 19°, which would best satisfy the eclipses as given in the calendars for four or five years which I had. In doing this I thoughtlessly assumed that the earth's shadow at the distance of the moon had the same diameter as the earth. Upon this assumption I spent a vast amount of time in determining these elements, but could get no positions of the nodes or inclination of the orbit which would satisfy the eclipses. The amount of study I gave to the subject, both day and night, was very great, but I at last gave the matter up in despair.

Some time after I was at work one day, toward evening, on the threshing-floor and saw the shadow of a distant vertical plank against the wall and observed that it was much smaller than the width of the plank, and the reason of it occurred to me at once. I then saw the error of my assumption with regard to the earth's shadow in my eclipse investigations and was now very anxious to go over again all my computations with the true diameter of the earth's shadow, for, knowing the distance of the moon and the angular diameter of the sun, I was able to determine this. As soon as I could find time I went over the whole work, and everything came out as satisfactorily as could reasonably be expected with my methods. I put the angle between the moon's orbit and the ecliptic at 5°, this seeming to be the value which satisfied best the conditions of the eclipses. This was in the winter of the first part of the year 1834. I now ventured to predict by my method the eclipses for the next year, 1835. I determined that there would be three eclipses—two of the moon and one of the sun. I computed the times of the middle of the lunar eclipses, the relations of the moon's path to the shadow and duration of the eclipses. In the case of the solar eclipse I merely predicted the time of the sun's being centrally eclipsed on the meridian. I made a record of the whole in a book and waited for the next calendar for comparison with its predictions. All the circumstances of the lunar eclipses agreed remarkably well, and the greatest error in the predicted times was only nine minutes.

I had heard a neighboring youth, a little older than myself, speak of a book that he had seen with a great many diagrams in it and which he said was called trigonometry. Hearing of the diagrams I was very desirous of obtaining it, and so the first time I had occasion to go to Martinsburg, after finishing my eclipse computations, I inquired for a treatise on trigonometry. It happened they had on hand (in an ordinary store) a copy of Gummere's Surveying, which I bought. Although it was not exactly what I called for, yet it had a good deal of trigonometry in it and suited me better than what I called for would have done.

This was in February of 1834. During the balance of the winter and the spring I completely mastered it, working out all the examples except a number of miscellaneous examples at the close of the work, for which no rules had been given and which required a knowledge of geometry. During the summer, as I had a little time to spare, I dwelt upon these, giving weeks sometimes to a single proposition. It happened that during the summer I was engaged a great part of my time on the threshing-floor, which had large doors at both ends, with wide and soft poplar planks. Upon these I made diagrams, describing circles with the prongs of the pitchfork and drawing lines with one of the prongs and a small piece of board. One by one I mastered in this way these problems, all except three. For more than a quarter of a century these diagrams were visible on these doors, and in returning occasionally to the old homestead I always went to take a look at them.

In studying Gummere's Surveying I found many references to Playfair's Geometry. Late in the fall of 1834 I went to see a famous land surveyor, who lived in the mountains, to borrow the Geometry, if he had it. He was an Englishman and then an old man, and though he had a number of mathematical books they were all old authors. He did not have Playfair's, but he loaned me Simpson's, which I studied during the winters of 1834–'35.

The next winter, 1835–'36, I rode twenty miles in midwinter to Hagerstown, Maryland, to get Playfair's Geometry. It required two days to make the trip. This I studied during the balance of the winter and afterward occasionally, when I had any spare time. In all my winter evening studies I had mostly the light only of a blazing fire, brightened up frequently by throwing into it a piece of lightwood; at other times I had the dim light of a tallow candle.

Some time before this I found a copy of Hutton's Mensuration in the neighborhood, which I borrowed and studied.

In the spring of 1837 I found in Martinsburg a very elementary work on Natural Philosophy, treated in the manner of a dialogue. From this I first learned the law of gravitation, and that the moon and planets move in elliptical orbits.

In the spring of 1839, having saved a little money made by teaching, I resolved to go to Marshall College, Mercersburg, Pennsylvania, to spend the summer in the study of mathematics. The college had a winter and summer session, with a vacation spring and fall. I entered the preparatory school connected with the college and first took up the studies of algebra, geometry, and trigonometry. I now saw for the first time a treatise on algebra. It required only a small part of my time to keep along with the classes, and so I commenced to study the Latin and Greek grammars, mostly for the sake of having something to occupy my time, for I did not know that I would spend here more than one session. In the fall I returned to Virginia and taught the next winter, and also studied my Latin and Greek grammars and my researches. The next spring, with the promise of some pecuniary aid from my father, I returned again to Mercersburg. I now continued my Latin and Greek in the preparatory school, but entered the college classes in mathematics. By the rules of the institution irregular students who did not contemplate a full course could enter any of the classes for which they were prepared. At the close of this session, with the progress which I made during the session and the preceding winter while teaching and in consideration of my standing in mathematics, I was admitted to the sophomore class, though I was allowed to continue my mathematics with the juniors and seniors. During this year Professor Budd formed a volunteer class of such juniors and seniors as were willing to take up the study of certain branches not contained in the regular course. We recited every Saturday morning. He occasionally gave problems to the class for solution during the week, not contained in the text books.

On one occasion he gave the problem: Given the distance of a well from the three angles of an isosceles triangle to determine the triangle. I solved the problem and gave the solution to a few others of the class, but most of them had no solution. Professor Budd, after inquiries from the class, found that I was the only one who had succeeded in solving it. He said he had given it to several classes before, but had never obtained a solution. It was easy to me at the time, for it was one of the problems which I had solved while at work on the threshing-floor, with the use of diagrams on the barn doors, long before I had seen a college and algebra or geometry, except Simpson's Geometry, which I obtained the next winter.

At the commencement of the college, in the fall of 1841, I had been admitted to the junior class, but I had completed the full college course in mathematics and also much not in the course in the volunteer class.

My money was now exhausted, and I left with no intention of ever returning. I returned to Virginia and taught the next two years. Meanwhile a new college had been gotten up in Virginia, Bethany College, and I resolved to return to it and spend a year at least. Being in my own State and under the control of the "Christian Church," of which most of our family were members, was the only reason why I returned to this instead of Marshall College. Reverend Alexander Campbell was at the head of the college. I was admitted to the senior class and was one of the first graduating class of the college. This was on the 4th of July, 1844.

In the fall of this year I went west and settled in Liberty, Missouri, where I taught nearly eighteen months, but was obliged to give up my school before the last session closed on account of my health.

While in Liberty I found in a store a copy of Newton's "Principia." It had been ordered by an itinerant, who said he was about to open a school there, but before it, and a large number of other books which he had ordered arrived, he had disappeared. The storekeeper was glad to get five dollars for the work, less than half its first cost in Philadelphia. I now entered upon the study of this, but had not made much progress before my health failed. In the spring of 1846 I left Liberty with the intention of returning to Virginia. At St. Louis, on the way, I called on Professor Stewart, who had been my instructor in mathematics at Bethany College, but who had resigned and now had a private school in St. Louis. He persuaded me to go to Clarksville, Tennessee, and the country around about and gave me a letter to a friend of his at Clarksville. After calling on him and spending a week there I went up into Todd county, Kentucky, just over the State line and only fifteen miles from Clarksville. I here commenced teaching a small school, but large enough for the condition of my health. This was a fine part of the country and there was considerable wealth. Most of the labor was slave labor. I remained in this county about seven years, teaching first at Hadensville and then at Allensville; both were villages. Soon as my health admitted I took up Newton's "Principia," and I now became first interested in the tides and conceived the idea that the action of the moon and sun upon the tides must have a tendency to retard the earth's rotation on its axis. Knowing that Laplace had treated the subject extensively in the "Mecanique Celeste" I was very desirous of obtaining a copy, mostly to see what he had on the subject. I accordingly instructed a village merchant, on going to Philadelphia for a supply of goods, to procure me the work, having little idea of the magnitude of the work or the cost. On learning the cost at Philadelphia he did not procure it for me until after writing to me and hearing further from me. I had now plenty to study, in connection with my teaching, for several years. In the summer of 1853 I wrote my first scientific paper for publication in Gould's Astronomical Journal, Cambridge, Massachusetts, "On the effect of the sun and moon upon the rotary motion of the earth." I showed that, upon a certain reasonable hypothesis, it would be very sensible; but as no retardation had been observed except what was accounted for in another way I concluded the effect was counteracted by the earth's gradual contraction. It was a carrying out the idea first conceived on reading Newton's "Principia." I had, however, been anticipated in the fundamental idea by Mayer, but I was the first to treat the subject in a completely mathematical manner, and it was entirely original with me.

In the spring of 1854 I located in Nashville, Tennessee, and opened a private school. Prof. W. K. Bowling, M. D., of the medical college here, became a warm friend of mine. He published the Nashville Medical Journal. He was desirous of giving it a general scientific character and desired that I should contribute something on almost any scientific subject. I accordingly contributed several essays on vision, including the principle of the stereoscope. While in Nashville I found in a book store a volume containing Airy's "Figure of the Earth" and his "Tides and Waves."

I studied these in connection with Laplace, especially the part with regard to the tides. I now furnished a second paper for Gould's journal, "The problem of the tides with regard to oscillations of the second kind." In this paper I showed that Laplace's celebrated result that the diurnal tide in an ocean of uniform depth would vanish, is simply one of an infinite number which would satisfy the differential equations of condition, in the case of no friction, which was the case treated, each depending upon the initial motions of the water, but that in the case of the least friction these motions would gradually vanish and those only would be left depending directly upon the acting forces. In my result there were terms depending upon an arbitrary constant, and I showed that this constant could have a value which would make the diurnal tide vanish. The terms depending upon this constant were entirely independent of the forces and, in the case of nature with friction, would vanish and the terms only would be left depending upon the forces. The terms depending upon the arbitrary constant depended simply upon the initial motions which the water might be supposed to have at the start.

At another time I found in the same store Maury's "Physical Geography of the Sea." This first turned my attention to meteorology. From this I learned that the atmospheric pressure was greatest near the parallels of 30° and less at the equator and in the polar regions, and I at once commenced to study the cause of it. In my study of the tidal theory I saw that there were certain terms in the equations depending upon the earth's rotation which affected the tides, and that there must be similar terms in the complete equations of the atmospheric motions and pressures.

In conversation one day with my friend doctor Bowling I told him I had read Maury's book, and he at once was desirous of knowing what I thought of it. I told him that I did not agree with Maury in many things. He then desired me to "pitch into him," as he expressed it, and furnish a review for his journal. This I declined to do, but at length consented to furnish an essay on certain subjects treated in the book and notice Maury's views a little in an incidental way. This was the origin of my "Essay on the Winds and the Currents of the Ocean," published in the *Nashville Journal of Medicine* and republished by the Signal Service as Professional Paper No. XII. About this time there was considerable discussion with regard to the gyroscope. I commenced to look up that subject and prepared a paper for doctor Bowling's journal on "The Gyroscope." From my researches I concluded that if there was an arm extending back from the pivot upon which the instrument revolved, on the opposite side and a weight were attached a little heavier than the other side, the gyroscope would gradually rise up instead of sinking down, and the gyrations on the pivot would be the contrary way. I had a gyroscope made to test this and found that I was right.

In the spring of 1857 professor Winlock, superintendent of the

American Ephemeris and Nautical Almanac, sent me an invitation, through Dr. B. A. Gould, with whom I had correspondence, to take a part in the computations of that work. I was now connected with a partner in a commercial school in Nashville. I left the school with him and went to Cambridge, Massachusetts, where the office then was, and remained through the summer. I had the choice in the fall of taking a certain amount of job-work yearly, to be done at home in Nashville, or of full employment if I wished to move to Cambridge. I took the latter, but took work with me to do in Nashville while I settled up my affairs there and made arrangements to move. I gave up the school to my partner and returned again to Cambridge in the spring of 1858. I now commenced the preparation of my paper on the "Motions of Fluids and Solids Relative to the Earth's Surface." This was published in parts in *Runkle's Mathematical Monthly*, which was now being started. A few copies were also printed in a connected form and distributed; a full abstract, omitting the mathematics, was published in the January number of Silliman's Journal for 1861.

In this paper the subject contained in my previous popular paper, just referred to, was treated in a mathematical way. In my former paper I had shown that if a body moves on the earth's surface either toward the east or west it is deflected toward the right in the northern hemisphere and the contrary in the southern, just as it is if it moves either toward the north or south, but I had not perceived that these deflecting forces were always equal. This was brought out in the latter paper, and the important result obtained that "*in whatever direction a body moves on the earth's surface there is a force arising from the earth's rotation which deflects it toward the right in the northern hemisphere, but the contrary in the southern.*" This result was published in the monthly in May, 1858, about six months before the subject was fully discussed in the French Academy, in which a number of eminent scientists took part and in which the same general result was arrived at. A reprint of this paper, with notes by Prof. Frank Waldo, has been published by the Signal Service, Professional Paper No. VIII.

In December, 1864, I read a paper before the American Academy of Arts and Sciences, Boston, entitled "Note on the Influence of the Tides in Causing an Apparent Acceleration of the Moon's Mean Motion."

At this time it had been shown by Delaunay and Adams that the lunar theory did not give the observed acceleration of about 11″ per century, as had been supposed, but only about 6″, and there was needed something to account for the balance. The object of the paper was to show that upon a very reasonable and probable lagging of the tides the effect would be sufficient to cause this apparently by changing a little the length of the day, the unit of time. This paper was read only a few weeks before Delaunay read a similar paper before the French Academy.

About this time I discovered two converging series with simple laws expressing the ratio between the circumference and the diameter of a circle. These I showed to Professor Peirce, who requested me to allow him to lay them before the National Academy of Sciences, then about to meet. They were afterwards published by the Smithsonian Institution under the title "New Converging Series expressing the Relation between the Diameter and the Circumference of a Circle," *Contributions of the Smithsonian Institution, No.* 233. The one of these is very convergent, as much so as any now known except one, that of Machin. About this time I commenced my "Tidal Researches," which were originally intended for publication by the Smithsonian Institution. These occupied my spare time from other duties for several years.

In the summer of 1867, soon after Professor Peirce took charge of the Coast Survey, he offered me a partial position in connection with that work, the duty assigned to me being a special discussion of the tidal observations of the Coast Survey. The understanding was that I was still to retain my Nautical Almanac work.

My reports of these discussions appeared in the appendices of the Coast Survey reports. My "Tidal Researches" were completed some time after I was connected with the Coast Survey, and so it was thought best that they should be published by the Coast Survey instead of the Smithsonian Institution, and so they were published as a separate appendix to the Coast Survey Report of 1874 and comprised 270 quarto pages. Only 500 copies were printed and distributed. I now engaged to prepare a series of papers for the Coast and Geodetic Survey, "Meteorological Researches for the Use of the Coast Pilot." These appeared in three parts in the reports of the Survey as they were completed.

Part I. "On the Mechanics and the General Motions of the Atmosphere," 1875.

Part II. "Cyclones, Tornadoes, and Waterspouts," 1878.

Part III. "Barometric Hypsometry and Reduction to Sea Level," 1881.

In the year 1880 I first became interested in the subject of my "Maxima and Minima·Tide-predicting Machine." A paper was read on the subject at the meeting of the American Association for the Advancement of Science, which took place at Boston that year. It was afterwards decided to have the machine constructed by the Coast Survey for its use, and it is now being used in tidal predictions. The report upon the theory and construction of the machine is contained in the report of the Coast and Geodetic Survey for 1883.

In the year 1882 I engaged in the work of the Signal Service Office, reserving, however, a part of my time for the Coast and Geodetic Survey; for the construction of the machine was then only fairly commenced, and it was necessary for me to superintend the work and to report upon the whole matter, and also to apply it in practice and to instruct some one in the use of it. I had several years before this resigned my Nautical Almanac work, which was merely computing and a constant interruption and annoyance in my other researches. My work for the Signal Service is contained in the professional papers and in the reports of the Chief Signal Officer.

Of the leading traits of my character a prominent one all through my life has been a great diffidence and a backwardness in coming in contact with strangers or in putting myself forward in any manner. An example of this occurred in the reading of my first scientific paper before the American Academy at Boston, referred to in the preceding pages, with reference to the effect of "Tidal action in causing an apparent acceleration of the moon's motion." Although the paper contained an original and important suggestion and I had it all written out, yet I carried it to the meetings of the Academy time after time with the intention of reading it, and my courage failed, and if I had deferred it one time more I would not have anticipated Delaunay on the same subject. All the government positions I have had were offered me without solicitation on my part.

Another has been great perseverance in anything in which I get interested, in never giving up anything until I make something out of it or satisfy myself fully that I cannot.

I have always been interested mostly in original researches—anything in which there was a prospect of discovering something or explaining something which had never been explained.

I have always been inclined to original methods instead of following usual methods which had been followed or laid down before.

In the demonstrations of propositions at college, at recitations I never followed closely the methods given in the text books and often gave a demonstration entirely different.

I have never been a great reader or a hard worker except when I got interested in some subject of research. I then devoured everything I could find to read on the subject and studied on it almost night and day.

Much of my time has been wasted, especially the earlier part of it, because not having scientific books and scientific associations I often had nothing on hand in which I was specially interested.

(Signed) WILLIAM FERREL.

PUBLICATIONS OF WILLIAM FERREL.

1. On the effect of the sun and moon upon the rotatory motion of the earth. (Dated Allensville, Kentucky, September 24, 1853.)
   Gould's Astronomical Journal, 1853, III, 138–142.

2. On the hypothesis of the internal fluidity of the earth. (No date.)
   Nashville Journal of Medicine and Surgery, September, 1854, VII, 199–203.

3. On vision. (No date.)
   Nashville Journal of Medicine and Surgery, January, February, and March, 1855, VIII, 22–28, 92–102, 192–201.

[In the article "Binocular Vision," N. J. M. and S., October, 1855, IX, 318–321, the editor, Dr. W. K. Bowling, defends Ferrel's priority over Prof. W. B. Rogers.]

4. On the variable star Algol. (No date.)
   Nashville Journal of Medicine and Surgery, April, 1855, VIII, 277–282.

5. The problem of the tides with regard to oscillations of the second kind. (Dated Nashville, May 24, 1856.)
   Gould's Astronomical Journal, 1856, IV, 173–176.

6. The rotascope. (Dated Nashville, July 17, 1856) [with an editorial postscript by Prof. W. K. Bowling].
   Nashville Journal of Medicine and Surgery, September, 1856, XI, 209–211.

7. An essay on the winds and currents of the ocean. (Dated Nashville, October 4, 1856.)
   Nashville Journal of Medicine and Surgery, October and November, 1856, XI, 287–301, 375–389.
   Reprinted in Professional Papers of the Signal Service, No. XII, Washington, 1882.

8. A complete solution of the problem of the rotascope, based upon well-known mechanical principles. (Dated Nashville, October 24, 1856.)
   Nashville Journal of Medicine and Surgery, December, 1856, XI, 463–473.

9. The influence of the earth's rotation upon the relative motion of bodies near its surface. (Dated Cambridge, November, 1857.)
   Gould's Astronomical Journal, 1858, V, 97.

10. The influence of the earth's rotation upon rotating bodies at its surface. (Dated Nashville, May, 1858).

Gould's Astronomical Journal, 1858, V, 113–114.

11. The motions of fluids and solids relative to the earth's surface. [Written in 1858 and 1859, but the concluding section is dated Cambridge, February, 1860.]

The Mathematical Monthly, January, 1859,–August, 1860: I, 140–148, 210–216, 300–307, 366–373, 397–406; II, 89–97, 339–346, 374–390.

Separate reprint under the title "The . . . . surface; comprising applications to the winds and the currents of the ocean." 4to, 72 pp., New York and London, 1860.

Republished, with notes by Prof. Frank Waldo, as Professional Papers of the Signal Service, No. VIII, Washington, 1882.

12. The motions of fluids and solids relative to the earth's surface. (No date.)

[This is a popular presentation by the author of the results deduced in the preceding mathematical memoir No. 11, and is not a simple abstract.]

American Journal of Science, January, 1861 (2), XXXI, 27–61.

Republished in Professional Papers of the Signal Service, No. XII.

13. Narrative of the American expedition to Northwest British America to observe the total eclipse of the sun, July 18, 1860. (No date.) (Report to Captain C. H. Davis, United States Navy, Superintendent Nautical Almanac).

American Journal of Science, January, 1861, XXXI, 139–142.

14. Convenient formulæ for interpolation. (No date).

The Mathematical Monthly, September, 1861, III, 377–384.

15. On the cause of the annual inundation of the Nile. (Dated Cambridge, December 12, 1862.)

American Journal of Science, January, 1863 (2), XXXV, 62–64.

16. Note on the influence of the tides in causing an apparent acceleration of the moon's mean motion. (No date.)

Proceedings of the American Academy of Arts and Sciences, Boston, December 13, 1864, VI, 379–383.

17. Communication supplementary to the preceding. (No date.)

Proceedings of the American Academy of Arts and Sciences, Boston, February 14, 1865, VI, 390–393.

18. On certain new converging series, expressing the ratio between the circumference and diameter of a circle. (No date).

Read before the National Academy of Sciences, Northampton, August, 1865, Rep. Nat. Acad. Sci. for 1883, p. 36.

Smithsonian Contributions, XVIII, No. 233, printed April, 1871.

19. On certain formulæ of interpolation. (No date.)

Proceedings of the American Academy of Arts and Sciences, Boston, September 12, 1865, VII, 2–12.

20. On an annual variation in the daily mean level of the ocean and its cause. (No date.)

Proceedings of the American Academy of Arts and Sciences, Boston, October 10, 1865, VII, 31–36.

21. On a table for facilitating the conversion of longitude and latitude into right ascension and declination. (Read before the National Academy of Sciences, Northampton, August, 1866—title only.) Report Nat. Acad. Sci. for 1883, 37.

22. On tidal researches. (Read before the National Academy of Sciences, Washington, April, 1869—title only.)

Report Nat. Acad. Sci. for 1883, 39.

23. Some results of the discussion of the Boston dry-dock tide observations.

Proc. A. A. A. S., Salem, August, 1869, 276 (title only.)

24. Discussion of the tides of Boston harbor. (Dated Cambridge, May 2, 1870.)

U. S. Coast Survey Report for 1868, pp. 6, 7 and Appendix No. 5, 51–102.

25. Explanation of the observed fact that a man's left side walks faster than his right (title only). (Read before the National Academy of Sciences, Washington, April, 1871.)

Rep. Nat. Acad. Sci. for 1883, p. 41.

26. Determination of the moon's mass from tidal observations. (Title only). Read before the National Academy of Sciences, Washington, April, 1871.

Rep. Nat. Acad. Sci. for 1883, p. 41.

27. On the moon's mass as deduced from a discussion of the tides of Boston harbor. (Dated Cambridge, June 24, 1871.)

U. S. Coast Survey Report for 1870, Appendix No. 20, 190–199.

28. The cause of low barometer in the polar regions and in the central part of cyclones. (Dated Cambridge, ...)

Nature, July 20, 1871, IV, 226–228.

Van Nostrand's Engineering Magazine, November, 1871, V, 525–527.

Professional Papers of the Signal Service, No. XII, Washington, 1882.

29. On the effects of winds and barometric pressure on the tides of Boston and on the mean level of the sea.

Philosophical Society of Washington, Bulletin, May 4, 1872, I, 53–54.

30. Maxima and minima of tides on the coast of New England for 1873. (Dated Cambridge, September 2, 1872.)

U. S. Coast Survey Report for 1872, Appendix No. 7, 73, 74.

31. Tidal researches. Read before the National Academy of Sciences, Cambridge, November, 1872 (title only).

Rep Nat. Acad. Sci. for 1883, p. 42.

32. Meteorological effects upon the heights of the tides. (Dated Cambridge, January 2, 1873.)

American Journal of Science, May, 1873 (3), V, 342–347.

33. Report on meteorological effects upon tides from observations. (Dated Cambridge, May 31, 1873.)

U. S. Coast Survey Report for 1871, Appendix No. 6, 93–99.

34. Table of tide predictions for Boston harbor for 1874. [Contributed by William Ferrel to the annual volume of "Tide Tables for the United States."]

See U. S. Coast Survey Report for 1873, 62.

35. On the tides of Tahiti. Read before the National Academy of Sciences, Washington, April, 1874 (title only).

Report Nat. Acad. Sci. for 1883, p. 44.

36. On the laws of cyclones. Read before the National Academy of Sciences, Washington, April, 1874 (title only).

Report Nat. Acad. Sci. for 1883, p. 44.

37. Tidal researches. (Dated Washington, May, 1874.)

[Referred to in U. S. Coast Survey Report for 1874, page 12, as a separately printed Appendix.] Washington and Cambridge, 1874, 4to, 270 pages.

38. On the law connecting the velocity and direction of the wind with the barometric gradient. (No date.)

Abstract in Bull. Phil. Soc., Washington, June 20, 1874, I, 106–109.

39. Relation between the barometric gradient and the velocity of the wind. (No date.) Read before the American Association for the Advancement of Science, at Hartford, August, 1874.

Proc. A. A. A. S., Hartford, August, 1874, p. 144 (title only).

American Journal of Science, November, 1874, VIII, 343–362.

See Hann's presentation of the result in Zeit. Oest. Gesell. Met., 1875, X, 81–88, 97–106.

40. The constant currents in the air and the sea. (Dated Washington, November 7, 1874.)

Nature, January 7, 1875, XI, 186–187.

41. Beziehung zwischen den barometrischen gradienten und die windgeschwindigkeit (letter dated Washington, April 27, 1875).

Zeitschrift Oest. Gesell. fur Meteorologie, August 15, 1875, X, 254–255.

Translation in Ann. Rep. Sec. Smithsonian Inst. for 1877, 445–446.

42. Discussion of the tides in New York harbor. (Dated Washington, June 30, 1875.)

U. S. Coast Survey Report for 1875, page 8, and Appendix No. 12, 194–220.

43. Tides of New York harbor. (Title only.)

Proceedings A. A. A. S., Detroit, August, 1875, p. 121.

44. On a controverted point in Laplace's theory of the tides. (Dated November 17, 1875.)

L., E. & D. Philosophical Magazine, March, 1876 (5), I, 182–187.

45. On the progressive motion of storms. (Title only.) Read before the National Academy of Sciences, Washington, April, 1877.

Report Nat. Acad. Sci. for 1883, p. 47.

46. Methods, discussions, and results.

Meteorological Researches for the use of Coast Pilot: part I, Washington, 1877. On the mechanics and the general motions of the atmosphere. (Preface by Superintendent C. P. Patterson, dated July 21, 1877.)

Also published in U. S. Coast Survey Rep., 1875, as Append. 20, 369–412, Washington, 1878.

Review by M. Thiessen, Zeit. Oest. Gesell. Met., 1879, XIV, 386–390.

Notice of Thiessen's review, Coast Survey Rep., 1879, 4, Washington, 1882.

47. The theory of waterspouts. (Title only.) Read before the National Academy of Sciences, Washington, April, 1878.

Report Nat. Acad. Sci. for 1883, p. 48.

48. Observation of total solar eclipse, July 29, 1878, on the summit of Gray's peak, Colorado.

(Noticed in U. S. Coast and Geodetic Survey Report for 1879, 65.)
[Probably an unpublished manuscript report.]

49. Discussion of tides in Penobscot bay. [At Pulpit cove.] (Dated Washington, December 31, 1878.)

U. S. Coast and Geodetic Survey Report for 1878, Appendix 11, 268–304, Washington, 1881.

[See reference to progress on this memoir in Report C. and G. Survey, 1880, p. 2.]

50. Tornadoes, waterspouts, and hailstorms. (Title only.)

Proc. A. A. A. S., St. Louis, August, 1878, 123.

51. On hollow waterspouts and sandspouts. (Title only). Read before the National Academy of Sciences, Washington, April, 1880.

Report Nat. Acad. Sci. for 1883, p. 51.

52. On cloud-bursts. (Title only.) Read before the National Academy of Sciences, Washington, April, 1880.

Report Nat. Acad. Sci. for 1883, p. 52.

53. Methods and results.

Meteorological Results for the use of the Coast Pilot. Part II: On cyclones, waterspouts, and tornadoes. [No date, but contains data added in 1880. See notice of its completion in Rep. C. S. O., 1881, p. 2.] Washington, 1880.

Also published as U. S. Coast Survey Report for 1878, Appendix No. 10, 176–267, Washington, 1881. [This volume was bound and distributed in July, 1882.]

Review of the above by (. . . . ?) in The Scientific American, 1881, XLIV, 304; by E. D. Archibald in "Nature," 1882, XX, 155, 291; also XXVI, 9, 31; by A. Sprung, Zeits. Oest. Gesell. Met., 1882, XVII, 161–175, 276–282.

54. Maxima and minima tide-predicting machine. [Sketch of a proposed construction.] (Title only.)

Proc. A. A. A. S., August, 1880, Boston, p. 276.

55. Meteorological researches. Part II: Cyclones, tornadoes, and waterspouts. [Abstract of memoir No. 53, by the author.] (No date.)

American Journal of Science, July, 1881 (3), XXII, 33–48.

Professional Papers of the Signal Service, No. XII, Washington, 1882.

Review of this abstract in "Naturforscher," 1881, XIV, 345–348.

56. Methods and results.

Meteorological Researches for the use of the Coast Pilot. Part III: Barometric hypsometry and reduction to sea-level. (No date, evidently 1881.) Washington, 1882.

Also published as U. S. Coast and Geodetic Survey Report for 1881, Append. No. 10, 225–269, Washington, 1883.

See report of progress on this work, C. and G. Survey Rep. for 1880, p. 2, and for 1881, p. 2.

Review by Hartl, Zeits. Oest. Gesell. Met., 1884, XIX, 466–468.

57. On the conditions determining temperature. (No date.)

Bull. Phil. Soc., Washington, January 28 and February 25, 1882, V, 90–97.

Review in Zeits. Oest. Gesell. Met., 1884, XIX, 386–388, 500–501.

58. Discussion of the tides of the Pacific coast of the United States. (Dated Washington, June 1, 1882.)

U. S. Coast and Geodetic Survey Report for 1882, Append. No. 17, 437–450, Washington, 1883.

59. Wind pressure. (Dated Washington, June 20, 1882.)

Van Nostrand's Engineering Magazine, August, 1882, XXVII, 140–143.

60. The relative temperatures of the two hemispheres of the earth. (No date.)

Am. Jour. Sci., August, 1882 (3), XXIV, 89–92.

61. Maxima and minima tide-predicting machine. Exhibition of the machine. (Title only.) Read before the National Academy of Sciences, Washington, April, 1883.

Rep. Nat. Acad. Sci. for 1883, p. 56.

62. Temperature of the atmosphere and the earth's surface. (Dated Washington, June 30, 1883.)

Professional Papers of the Signal Service, No. XIII, 4to, 69 pp., Washington, 1884.

Review in Zeits. Oest. Gesell. Met., 1884, XIX, 500-501.

63. Report on the harmonic analysis of the tides at Sandy Hook. (Dated Washington, July 31, 1883.)

U. S. Coast and Geodetic Survey Report, 1883, Append. No. 9, 247-252, Washington, 1884.

64. Description of a maxima and minima tide-predicting machine. (Dated Washington, November 10, 1883.)

U. S. Coast and Geodetic Survey Report for 1883, Append. No. 10, 253-273.

[See references to the designing and constructing, Rep. C. and G. Survey, 1880, p. 3; 1882, p. 61; and to its satisfactory completion, Rep. C. and G. Survey, 1883, p. 93.]

65. Maxima and minima tide-predicting machine. (No date.) [Abstract by the author of the preceding memoir.]

Science, April 14, 1884, III, 408-410.

66. On the harmonic analysis of the tides at Governor's island, New York harbor. (Dated Washington, June 30, 1884.)

U. S. Coast and Geodetic Survey Report for 1885, Append. 13, 489-493. [See also remarks by the Superintendent C. and G. Survey on page 7.]

67. The gyration of a vibrating pendulum. (No date.) [Letter correcting a typographical error in "Motion of fluids and solids."]

Science, July 18, 1884, IV, 53.

68. Priorität des Buys-Ballot'schen Gesetzes. (Letter dated Washington, March 25, 1885.)

Zeits. Oest. Gesell. Met., 1885, XX, 187.

69. Recent advances in meteorology. [Original dated March 2, 1885; revised in reading proof-sheets, July to September, 1886.]

Report of Chief Signal Officer, 1885, part II, Appendix No. 71, 440 pp.

Review by W. M. Davis, Science, June 3, 1887, IX, 539-541 translated in Met. Zeit., 1887, IV, p. [57]-[59].

Review by G. E. Curtis, Am. Met. Jour., August, 1887.

70. Psychrometry. [Abstract of preliminary results, see No. 73.]

Proc. A. A. A. S., Ann Arbor, August, 1882, p. 80.

71. Vapor tension, dew-point, and relative humidity tables, adapted to the whirled or sling-psychrometer.

Published by the Signal Office as official: signed by Thomas M. Woodruff, November 27, 1885. 12 pp., 8vo.

[These were officially adopted by general orders No. 1, January 1, 1886, and agree with the tables appended to No. 73 of this bibliography.]

72. The solar thermometer. (No date.)
American Meteorological Journal, November, 1885, II, 303–306.

73. Report on psychrometric tables for use in the Signal Service. (Dated December 10, 1885.)
Report of Chief Signal Officer, 1886, Append. 24, 233–259.

74. The Arago-Davy actinometer. (Dated Washington, ...)
American Meteorological Journal, December, 1885, II, 350–354, 395–400.

75. Temperature of the moon. (First letter, no date.)
Science, December 18, 1885, VI, 541-542.

76. Temperature of the moon. (Second letter, Washington, January 4, 1886.)
Science, January 8, 1886, VII, 32.

77. Sea-level and ocean currents. (First letter, dated Washington, January 18, 1886.)
Science, January 22, VII, 75–77.

78. Temperature of the moon. (Third letter, dated Washington, January 28, 1886.)
Science, February 5, 1886, VII, 122–123.

79. A course of forty lectures to signal officers on "Dynamic meteorology" during February and March, 1886.
[Not printed as yet, but made the basis of the subsequent "Popular treatise on the winds."]

80. Sea-level and ocean currents. (Second letter, dated Washington, February 18, 1886.)
Science, February 26, 1886, VII, 187–189.

81. Review of Rudolph Spitaler's "Wärme-vertheilung, &c." (no date.)
Am. Met. Jour., March, 1886, II, 520–523.

82. Review of Dr. A. Sprung's "Lehrbuch der Meteorologie" (no date.)
Am. Met. Jour., April, 1886, II, 553–565.

83. Nocturnal cooling of bodies. (No date.)
Science, April 2, 1886, VII, 329–330.

84. Sea-level and ocean currents. (Third letter, dated Washington, July 18, 1886.)
Science, July 30, 1886, VIII, 99–101.

85. Tables for reduction of the barometer to sea-level. [Ordered to be adopted July 10, 1886.]
Published by the Signal Service, July 27, 1886, and Rep. C. S. O., 1886, 226–232.

86. Report on reduction of barometric pressure to sea-level and standard gravity. (Report dated August 31, 1886.)
Rep. of C. S. O., 1886, Append. 23, pp. 221–232.

87. Review of R. T. Smith's "Results of solar radiation observations." (Dated Washington, September 4, 1886.)
Am. Met. Jour., September, 1886, III, 223–226.

88. Sensitiveness of the wind-vane (dated Kansas City, Mo., January 12, 1887.)
Am. Met. Jour., February, 1887, III, 452-454.

89. Relation of the pressure to the velocity of the wind. (Dated Kansas City, Mo., June 5, 1887.)
Am. Met. Jour., August, 1887, IV, 173–177.

90. Theoretical meteorology. (Letter dated Kansas City, Mo., July 13, 1887.)
Science, July 22, 1887, X, 48.

91. Reply to critical review by G. E. Curtis of "Recent advances in meteorology". (Dated Kansas City, Mo., August 14, 1887.)
Am. Met. Jour., September, 1887, IV, 211–213.

92. Note on the influence of forests upon rainfall. (No date.)
Am. Met. Jour., February, 1889, V, 433–435.

93. Note on the wind-pressure constant. (Letter dated Kansas City, Mo., February 20, 1889.)
Science, March 1, 1889, XIII, 171.

94. The wind-pressure constant. (Letter dated Kansas City, Mo., March 5, 1889, correcting a typographical error.)
Science, March 15, 1889, XIII, 205.

95. Note on the Robinson anemometer constant. (Letter, no date.)
Science, March 15, 1889, XIII, 204–205.

96. A popular treatise on the winds, comprising the general motions of the atmosphere, monsoons, cyclones, tornadoes, waterspouts, hailstorms, &c. (dated Kansas City, Mo., April, 1889.)
John Wiley & Sons, New York, 1889, pp. vii, 505.
Reviewed by W. M. Davis, Science, February 28, 1890, XV, 142–143.

97. Laplace's solution of the tidal equations. (Dated Washington, May 22, 1889).
Gould's Astronomical Jour., July, 1889, IX, 41–44.

98. The law of thermal radiation. (No date.)
American Journal of Science, July, 1889 (3), XXXVIII, 3–29.

99. Decrease of temperature with increase of altitude. (No date.)
Am. Met. Jour., August, 1889, VI, 145–150.

100. Comments on Mr. Arthur Searle's "Atmospheric economy of solar radiation." (No date.)
Am. Met. Jour., August, 1889, VI, 177–179.

101. Weber's law of thermal radiation (no date).
Am. Jour. Sci., February, 1890 (3), XXXIX, 137–145.

102. Professors Hazen's and Espy's experiments. (Letter dated Martinsburg, West Virginia, September 24, 1890.)
Science, October 3, 1890, XVI, 192–193.

103. Espy's experiments on storm generation. (Dated Martinsburg, West Virginia, September 25, 1890.)
Am. Met. Jour., November, 1890, VII, 337–340.

104. Laplace's solution of the tidal equations. (Dated Martinsburg, West Virginia, December 3, 1890.)
Gould's Astronomical Journal, December, 1890, X, 121–125.

105. Dr. Hann's studies on cyclones and anticyclones. (Dated Martinsburg, West Virginia, December 12, 1890.)
Science, December 19, 1890, XVI, 344–347.

106. The subtropical zones of high barometric pressure. (Dated Martinsburg, West Virginia, December 22, 1890.) [Reply to Hann and others.]
Science, January 2, 1891, XVII, 8–10.

107. Cyclones and areas of high pressure (dated Martinsburg, West Virginia, January 10, 1891).
Science, January 16, 1891, XVII, 38–40.
[A reply to W. M. Davis in Science, May 30, 1890, XV, p. 332, and Science, January 2, 1891, XVII, p. 4; and to H. C. Clayton in Science, January 2, 1891, XVII, pp. 10 and 11.]

108. The high-pressure area of November, 1889, in central Europe, with remarks on high-pressure areas in general. [A reply to Dr. J. Hann.] (Dated Martinsburg, West Virginia, probably March, 1891.)
Nature, April, 1891, XLIII, 466–471.

109. Autobiographical sketch. (Written in January, 1888, for Mr. A. McAdie.)
[Published herewith as an appendix to this biographical memoir.]