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PROFESSOR WILLIAM FERREL.

WILLIAM FERREL was born in Pennsylvania, 1817. In 1856, at the age of thirty-nine, he began a series of studies in meteorology, which, in their more finished form, in later years gave a new aspect to this science, and placed him at the time of his death, Sept. 18, 1891, at the front of American meteorologists. His work was always quietly done, never with any attempt at the conversion of the great public, or almost with indifference to the attitude of the scientific public regarding his beliefs; but with the patient conviction that he was working in the right direction and that his theories would in time receive general acceptance. Towards the close of his life, this happy end was reached, as far as the better informed meteorologists of the world were concerned, and in Europe as well as in this country, Ferrel was regarded as the leader in the methods of mathematical meteorology; not that others who followed in his paths did not exceed him in completeness of demonstrations, but that the methods which he introduced into the science were essentially the same as those by which his successors carried it further. A comprehensive narrative of his life is given in the *American Meteorological Journal* for February, 1888, by Alexander McAdie of the Weather Bureau, and a list of his publications in the same journal for October last; I shall therefore here only touch on what seems to me highly characteristic of his work, and of the revolution that it produced in scientific meteorology.

Unscientific meteorology, such as was current before Ferrel's work reformed it, cannot yet be said to be excluded from popular acceptance. We still find writers who take Maury as their authority, following his antiquated views, quite unaware that they are thirty years behind the times. I do not wish to detract in the least from the deserved reputation gained by Maury for his persevering study of the winds and currents of the ocean; for the great incentive that he gave to ship-masters to become observers and bring home a careful record of their observations. The tabulation of the facts thus gathered formed the basis of wind-charts for the several oceans, first produced in this country, and closely followed by the hydrographers of many foreign nations. It is on this collection of facts that Maury's reputation rests secure; and not on his theories, for they were essentially wrong and are now practically laid aside. Unfortunately for his success in this department of science, Maury seems not to have been well equipped with knowledge of physics and mathematics, and in his ignorance of these subjects he was led into serious errors as to the motions of the winds. Those errors have been considered by various writers, but by none earlier or more effectively than by Ferrel, who, in 1856, published an essay in the Nashville *Journal of Medicine*, an essay prompted by the insufficiency of Maury's theories. It is not necessary to enter here into an exposition of Ferrel's theory; those who wish to study it may find its fullest statement in his latest work, a "Popular Treatise on the Winds," published in 1889. Some statement of these theories may be found in *Science*, ix., 1887, 539; and xv., 1890, 142. But it may be briefly said that the difference between Maury's theory and Ferrel's is as the difference between darkness and light.

Maury thought the return current from the poles was in this hemisphere an east-north-east wind: Ferrel showed that it is a west-north-west wind. Maury was not alone in thinking that the polar return current flowed in our latitudes from the north-east. Dove, the leading German meteorologist of the middle decades of this century, had the same idea, and, I think, at an earlier date than Maury. According to Dove, the alternation of north-east and south-west winds that we feel with the passage of our storms centres is simply the contest of the polar and equatorial currents, of which first one and then the other reach the surface of the earth. This view, embodying the idea of the north-east-south-west course of the polar return current, may be said to have held an accepted place in meteorology at the time when Ferrel prepared his first essay on the subject. But for those who have followed Ferrel's work, the north-east return current has no existence. His reasons for giving this return current a north-west source are simple and ample; and for those who do not share this view, there is a large fact in nature which cannot be explained; namely, the low pressure about the North Pole; a similar arrangement prevailing in the Southern Hemisphere, where the return current comes from the south west.

This seems to be a small matter. It is a slight change to make in words, to say that the return polar current comes from the north-west, not from the north-east: and truly, if this were all that could be said, it would not be a great affair. But if the reader will examine the question carefully, and study the development of our knowledge of the winds, he will soon be convinced that the introduction of Ferrel's idea, as to the course of the polar return current and the explanation of the low pressure that is bound up with it, marks the introduction of rational physical principles into this department of meteorology. This change came at a time when the physical study of meteorology was a rare thing. Look, for example, at Schmid's "Meteorologie" of 1860, a voluminous treatise, well representing the condition of the science then; compare with it Spring's "Lehrbuch" of 1885, in which the science is presented in the manner introduced by Ferrel. The difference is that between statistical, inductive methods, and fully expanded logical methods that utilize all means of inquiry. The science has become a new thing by this change; would that meteorologists had as greatly changed and were not still so content to read instruments and count up totals and means.

If we were to search Ferrel's writings for the most important principle introduced by him into the study of meteorology, it would be found in the deflective force arising from the earth's rotation, by which all bodies moving on the earth's surface tend to turn to the right in this hemisphere, but to the left in the southern. It is curious in reading over the general run of meteorological essays to notice how inadequately the action of this force is considered. In the first place, it is too commonly said to act only on meridional motions; that is, to make a poleward motion run ahead of its initial meridian, or an equatorward motion fall behind; but to have no effect on a motion to the east or west. This is incorrect, for, as Ferrel shows, the deflective force is independent of the azimuth of motion, and varies only with the velocity of motion and the sine of the latitude. In this he was preceded by others, who discussed the mathematical aspects of the question; but if we except the overlooked article of Tracy, no one before Ferrel correctly introduced the action of the deflective force into meteorology. It is not simply that a wind tends to turn aside from the gradient, as may be seem by the most elementary inpection of our weather maps; but that, in thus turning aside, it reacts on the distribution of pressure by which its motion is caused, and produces a very significant re-arrangement of pressures in some cases. This was first demonstrated by Ferrel; and if the student wishes to appreciate the conditions under which the winds move, he should follow this subject out to its end. The most conspicuous effect of the re-arrangement of pressures in this manner is the reduction of the polar high pressures, such as would exist if determined by low temperature alone, into low pressures: for, on account of the earth's rotation, the whole system of terrestrial winds in temperate and frigid latitudes runs in a great whirl around the poles from west to east; and the centrifugal force thus developed in excess of that characterizing the rotation of the earth itself, suffices to withhold so much air from the polar regions that the anticipated high pressure due to low temperature cannot occur there: the air thus withheld from the polar regions forms a broad belt of high pressure around the tropics. The importance of this even in elementary teaching must be apparent; for when a teacher tells his class that the general winds flow because the difference of temperature between the equator and the poles establishes a convectional circulation, the class has a right to ask why the region of low temperature is not the region of high pressure, as it should be in a convectional circulation. No sufficient answer to this significant question is to be found in any text-book in our language, except Ferrel's "Popular Treatise." Not only so; some of the most eminent meteorologists give no particular attention to this aspect of the question. For example, in the recent "Report on the Meteorology of the Challenger Expedition," the most beautifully illustrated of any meteorological work ever published, Buchan passes over the matter without alluding to Ferrel's explanation of it, and without giving any adequate explanation of it himself. In Germany there is a much better appreciation of the nature of the case, as far as it is represented by the investigations of mathematicians and the discussions in recent text-books. The contrast between the attitude of the conservative British and the progressive German schools may indeed be taken as indicating the difference between the older and the more modern status of meteorology; the division between the two being on the lines marked out by Ferrel. Certainly, when we find that the general distribution of atmospheric pressure, the general direction of the greater part of the atmospheric circulation and the general velocity of its motion all depend on the deflective forces arising from the earth's rotation, it is not unfair to claim for them and for the investigator who first properly introduced them a large share of credit in the recent advances of meteorology. It is the same with cyclones; those of the torrid regions, where the deflective force is small, present illustrations of distributions of pressure and circulation of wind dependent chiefly on differences of temperature and local centrifugal force; but in temperate latitudes, where the sine of the latitude is of a considerable value, the low pressure of the central part of the cyclonic storms is in great part the product of outward deflective force that accompanies the motion of the winds Finally, even in the small vorticular whirls of tornadoes, the deflective force has its effect; not directly, as in the case with cyclones proper, but indirectly: the tornado whirls around because it is developed in a whirling cyclone, and the cyclone turns because it is developed on a rotating earth. Indeed, in following through Ferrel's admirable theory of tornadoes, the only theory of tornadoes worthy of a name, it is made clear that if the deflective force of the earth's rotation were not, indirectly at least, communicated to the tornado, its violence would be greatly reduced, perhaps to the degree of rendering it nearly harmless.

The introduction of a general principle into a science, whereby a variety of apparently independent facts are found to be bound together by a comparatively simple relation, is in itself a great contribution to knowledge. The grand views of the correlations that connect all the winds of the world that are gained through Ferrel's essays repay the effort needed to study them out to the point of clear understanding; not that the essays are obscure or unnecessarily complex, but that their reading involves a rather clear knowledge of physics and mechanics, not to speak of mathematics, and a careful following of close reasoning from premises to conclusions.

No just appreciation of Ferrel's simple life and broad scientific work can be given in a brief article. His work in meteorology is much more varied than may be inferred from the emphasis here given to a single one of the leading principles that he followed. The others will be found by the faithful students of his books. His studies in other subjects than meteorology are of sufficient importance to deserve a separate notice. He was far enough advanced in astronomy, while employed in our Nautical Almanac office, to give new understanding to one of the puzzles of the sky; an unaccounted acceleration of the moon's motion was explained by him as a result of a retardation of the earth's rotation, caused by the action of the tides. The interaction of the lunar and terrestrial tides was also perceived, and when in the Coast Survey office in Washington, the calculation of tide-tables at our Atlantic ports was a subject of advanced study. A tide-predicting machine was then devised, by which the labor of thirty or forty men is now saved. Later in the Signal Office, Ferrel prepared his report on "Recent Advances in Meteorology," and gave lectures to the lieutenants on duty there, the substance of these lectures being now published in the "Popular Treatise on the Winds," referred to above.

Ferrel's simple manner of living kept him apart from the world about him; he had warm friends, but they were comparatively few. These few unite in feeling that it was a privilege to know such a man; modest, unassuming, even humble in his ways; yet with an insight into the truths of nature that goes only with rare genius. He was one of the small number of men in the world who not only advance the limits of knowledge, but who turn the search for it into new courses. It is safe to say that while he must already be regarded as the most eminent meteorologist of our country, the true measure of his eminence will be better recognized when those who follow the science that he enlarged come to appreciate more fully what he did for it.

W. M. D.

PROFESSOR JOSEPH LEIDY: HIS LABORS IN THE FIELD OF VERTEBRATE ANATOMY.[1]

We hear it said that at no time have the conditions for intellectual attainment been so favorable as in the days of Athenian supremacy. This may be true for communities, but not for individuals. Surely the atmosphere of Philadelphia from 1823 to 1891 favored greatness in science, else there is no connection between the man and his environment. Is it not a truth that it only needs the man to come forward to claim favoring conditions, to insist upon them as his own, to have another like Joseph Leidy to be bred among us? A man to whom questions of birth and of patronage were as nothing; one with a common school education and without the subsequent advantages of training under distinguished masters; one to whom all things required for his well-being appeared to come like the beneficent forces of nature until we are apt to lose sight of the will and of the steadfast purpose that directed them. He was never

> "limited and vexed
> By a divided and delusive aim,"

but, fixed and invariable in his methods, he completed a unique career.

He dedicated himself early to anatomy, and it is about this science as a central stem that all his labors cluster.

Signs of immaturity are evident in the early labors of most men. But this was not the case with Leidy. His first paper, entitled, "Notes on the White Pond in New Jersey" (Proc. Phil. Acad. Nat. Sci., 1847) exhibited the same clear observation and lucidity of statement which characterize his subsequent writings. The earliest of his anatomical papers ("On the Fossil Horse of America," Proc. Phil. Acad. Nat. Sci., 1847, 262) was in no respect inferior to any of his numerous records in the literature of paleontology of North America. The word growth used in respect to him is inappropriate. In the best sense of the word he never grew. Rather, like Bichat, he simply unfolded the native resources which lay innate within him.

For his graduating thesis in medicine he treated of the eye in vertebrate animals. This essay has not been published. In his twenty-second year, namely, July 29, 1845, he was elected a member of the academy, and from this date to that of his election to the chair of anatomy in the University of Pennsylvania, eight years later, his communications were in the main devoted to the structure and properties of the vertebrates. In this interval his industry was great, for he was actively engaged at the same time in teaching, and in assisting Professor W. E. Hoone in his anatomical work, and Professor George B. Wood in dissecting and mounting pathological specimens. He described the retention of the intermaxillary suture in the skull of a New Hollander (Proc. Phil. Acad. Nat. Sci., 1847), also one on the same bodies in the boa constrictor resembling the Pacinian corpuscles (Proc. Phil. Acad. Nat. Sci., 1848, 27). He wrote a paper on the existence of the intermaxillary bone in the embryo of the human subject of the tenth week (Proc. Phil. Acad. Nat. Sci., 1848, 45).

[1] Read at a special meeting of the Philadelphia Academy of Natural Sciences, May 5, 1891, by Harrison Allen, M.D.

Remarkable instances of preservation of organized animal matter were reported by him in 1847 (Proc. Phil. Acad. Nat. Sci., 313) on the films and cartilaginous structures in the extinct genera *Basilosaurus* and *Megalonyx*, the former a reptile of the rocene and the latter a mammal of the pliocene age. The vertebræ of *Basilosaurus* retained tissue which when burnt gave out animal odor. Fibrous membranes taken from one of the bones of *Megalonyx* exhibited many of the characteristics of recent membrane; in the articular cartilages the corpuscles were well preserved and distinct. It was held that under favoring conditions the cartilaginous and fibrous tissue might be preserved for an indefinite period.

In 1848 (Proc. Phil. Acad. Sci., 116) Dr. Leidy read remarks on the development of the Purkinjean corpuscles in bone; on the intimate structure of articular cartilage, and on the arrangement of aveolar sheath of muscular fascicute and its relation to tendon.

Cartilage was found to possess numbers of fine, transparent filaments, nearly uniform in thickness, having an average measurement of $\frac{1}{25000}$ of an inch. Hunter had claimed this fibrilation, but without the aid of the microscope it cannot be demonstrated. This cannot be said to be a prior claim. Professor George A. Piersol has kindly informed me that Dr. Leidy was the first to make the announcement of a fact now accepted. Kolliler was inclined to regard the appearance as pathological. The fibrillar nature of the matrix of all dense connective tissue, including cartilage and bone, is now universally recognized. The comments upon the arrangement of the aveolar sheath of muscular fascicute were to the effect that "the filaments of fibrous tissue cross each other diagonally around the muscular fascicute, forming a double spiral extensive sheath. When the filaments reach the rounded extremities of the fascicute they become straight and in this manner conjoin with the tendinous filaments originating at the extremities of the muscular fibres. The importance of this arrangement can be readily understood, from the diagonally crossing of the aveolar filaments, comparatively inelastic in themselves, the sheath is rendered elastic, thus permitting the muscle fibres freely to move without their action being interfered with."

Dr. Leidy was in the habit of introducing these comments in his lectures when speaking of the function of fibres depending upon their position to each other rather than upon differences in composition.

In 1849 (Am. Journ. of the Med. Sci.) Dr. Leidy announced a plan of the construction of the liver. He assumed that the follicul form of the liver in insects represented the plan of the primitive liver of the human embryo. The subsequent changes which lead up to the complex system of interlacing of tubules with their linings of biliary cells was the result of the blind end of the follicle undergoing subdivision by branching, each of the branches being lined with the cells and the mouths of the now open tubules, freely communicating with each other. This scheme was the most philosophical of any hypothesis previously proposed to account for the intricacy of the minute anatomy of the liver; it was accepted at once by the scientific world, and is itself an answer to the criticism sometimes made upon Dr. Leidy's labors, that they are purely descriptive. The evolution of the system of glands appended to the alimentary canal was distinctly set forth by Leidy in this paper. Since the relations of the liver as a blood-making and an excretory organ have been better defined, other hypotheses than that of Leidy have been proposed to elucidate its morphology.