

SCIENCE.—SUPPLEMENT.

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ADVANCES IN METEOROLOGY.

DURING Mr. William Ferrel's service as a professor in the signal office for the past few years, from which he has recently retired, his chief occupation was the preparation of a work on meteorology that should represent the modern attitude of the science, and serve as a guide in the theoretical questions that continually arise in the prosecution of the practical studies of our weather-bureau. The book, originally intended to be a 'professional paper,' now appears as an appendix to the late chief signal officer's last report, published by authority of the secretary of war. This form of publication involves some inconveniences: the making of the book is not so good as such a book deserves; the current page-heading, 'Report of the chief signal officer,' is an unfortunate example of formality; but the matter of the book is a long way beyond that of any English work on the subject, and it will take and hold the place of a standard authority.

Its mathematical treatment of the subject carries it beyond most readers. A more popular work by the same author would be a boon to teachers and students alike, and would do more than this advanced treatise can, to correct the misconceptions that still prevail in most text-books, and to induce a consideration of deductive, dynamical meteorology as well as of inductive, statistical meteorology, that now takes so large a share of the scholar's time.

The problem of the general circulation of the atmosphere serves particularly well to illustrate the need of this change of view. It is, moreover, a subject in which Professor Ferrel holds a peculiarly high position.

Instead of attempting to review all of the 'Recent advances,' I shall therefore refer only to this great problem, whose successful solution illustrates the high value of our author's methods.

First, some thirty years ago, Ferrel made the initial steps towards its rational solution; and, with a single exception, there has been no one else working in this profitable field until a few of the European mathematical meteorologists lately entered it.

A short acquaintance with the study will suffice to show that the temperature, pressure, and motion of the atmosphere must be closely interde-

pendent. Difference of temperature, as between equator and poles, must bring about difference of pressure; difference of pressure will cause winds; and the winds would soon restore equilibrium, if the difference of temperature were not continually maintained. The equilibrium cannot be reached: the winds will flow in obedience to residual differences of pressure that they cannot reduce to zero as long as the sun shines.

The early attempts at the further solution of this problem generally led to the statements that the warmth of the torrid zone caused the equatorial belt of low pressure; that the cold of the polar regions ought to cause areas of high pressure there, which were somehow reversed into lower pressures than at the equator, especially in the antarctic regions; that the belt of high pressure around the tropics was due to the crowding of the upper winds as they overflowed from the equator north and south along the converging meridians. The trade-winds, and the anti-trades above them, were normal members of this general circulation; but the prevailing west-south-west winds of the north temperate zone, and west-north-west winds of the south temperate zone, were not so easily explained. Dove called them 'equatorial' winds, and saw the compensating return current in the occasional north-east or 'polar' winds, which are now known to be 'accidental' or cyclonic in origin, and quite apart from the general planetary circulation. Maury explained them by supposing a curious crossing of currents at the tropical belts of high pressure. In the torrid zone the equatorial overflow was aloft; but outside of the tropics it came down to sea-level, and the return current ran aloft, — a most arbitrary and unreasonable hypothesis. Views hardly more logical than these still prevail in many text-books. It is indeed now almost universal to ascribe the tropical belts of high pressure to the convergence of the meridians; though why the crowding of the air should disappear in higher latitudes, where the meridians converge faster, is not explained. Sprung calls attention, in his excellent 'Lehrbuch der Meteorologie,' to the firm hold that this unphysical explanation has obtained, and wonders at the very slow awakening of meteorologists to Ferrel's theory. It is unfortunate that a theory so greatly needed has been so obscurely published. The *Nashville journal of medicine and surgery* first concealed it in 1856. *Runkle's mathematical monthly* gave it a more expanded

statement, but only carried it before a limited circle of readers, from 1858 to 1860. A briefer and more popular account appeared in the *American journal of science* in 1861, when school-masters might have seen it more generally, had not their attention been distracted by the war. Other brief articles have appeared in the same journal and in *Nature*. About ten years ago, an extended memoir, entitled 'Meteorological researches,' appeared as appendices to the coast-survey reports for 1877 and 1878, where they were said to be 'for the use of the coast pilot.' Like the earlier articles, these researches were too advanced and too little known to reach the school-master directly; but a review of them in *Nature* by Archibald has brought them before British meteorologists, where they were truly as much needed as with us. Still, it is only in Germany that they have had much effect on recent text-books, and it is to be feared that even the present work may not reach the readers who ought to have it: hence the hope expressed already, that Professor Ferrel may write a more popular book. We may hope, further, that it may find a way into our schools through some regular book-publishers rather than through dealers in second-hand government reports. Reflecting on this, how different was the immediate conquest of popular interest by Maury's famous 'Physical geography of the sea' from the long obscurity of Ferrel's 'Essay on the winds,' and how different the brief life of Maury's theories from the continually increasing vitality of Ferrel's! Perhaps, after all, the *Nashville journal* is a good medium of publication for the young scientist.

Ferrel showed in his first article, that, in consequence of the earth's rotation, all the atmosphere outside of the tropical belts of high pressure must have a general motion from west to east, and this disposed of Dove's north-east, 'polar' wind as a member of the planetary circulation. He also showed, that, as a consequence of the general eastward motion, the atmosphere would be drawn from the poles and thrown toward the tropics, thus causing the tropical belts of high pressure, and reversing the polar high pressure, that would be caused by the cold of the frigid zone, into polar low pressures. But, in order to explain the oblique recession of the surface winds in temperate latitudes from the tropics towards the poles, Ferrel then reversed the whole circulation of the winds at the tropics, placing the return current from the pole at the top, while it is at the bottom in the torrid zone.

The correction of this inversion appeared unconsciously and independently in a brief paper 'On the grand currents of atmospheric circula-

tion,' by Prof. James Thomson, read before the British association at Dublin in 1857. It occupies but little more than a page in the report of the meeting, and has never been expanded in the more complete form that it fully deserved. Thomson, like Ferrel, saw that the general motion of the atmosphere must be eastward, except in the trade-wind belt, and that the centrifugal force of the great polar whirls thus generated would decrease the pressure at the vortices or poles; but Thomson perceived also that the lowest part of the oblique return current, losing velocity by friction with the earth, tends to flow towards the pole, to supply the partial void in the central parts of the vortex. He states explicitly, "that, in temperate latitudes, there are three currents at different heights; that the uppermost moves towards the pole, and is part of a grand primary circulation between equatorial and polar regions; that the lowermost moves also towards the pole, but is only a thin stratum forming part of a secondary circulation; that the middle current moves from the pole, and constitutes the return current for both the preceding; and that all these three currents have a prevailing motion from west to east in advance of the earth." This was a most significant addition to Ferrel's first paper, but it lacked quantitative completeness. Ferrel's second paper modifies his first statements and diagrams, introducing the three-current system, and referring to Thomson's paper in a final paragraph, from which we may infer that the correction had occurred independently to our author. Be this as it may, Thomson's suggestion deserves more recognition than it has generally received. A second modification of the plan of general circulation appeared in the 'Researches' of 1877, in which the north-east winds of the arctic regions are omitted from the scheme of winds that would appear on a homogeneous earth, and thus by implication referred to a class that may be called continental, as depending directly and indirectly on the diversity of land and water surface on the globe: they are not known to occur around the south pole, where the surface is so largely water. Thus simplified, the scheme appears in the present work, where it demands the closest attention.

Another great generalization is that which connects cyclonic storms with the general circulation. It may be summarized as follows: a cyclone, or revolving storm, that appears in most symmetrical development in the tropical regions, has a centre of decidedly low pressure, surrounded by a ring of slightly higher pressure than the normal; outside of the ring, the surface winds move away from the centre and turn to the right of a radius; inside of the ring, the surface winds cir-

culate around the centre, blowing obliquely along an inward, ascending, left-handed spiral with increasing velocity, until they turn to an outward spiral aloft. The central low pressure in this case is primarily due to its high mean temperature, and secondarily to the centrifugal force of the whirl and the deflective force of the earth's rotation. This is a cyclone with a warm centre. The general winds of the northern hemisphere constitute a cyclone with a cold centre: their centre of low pressure is at the pole, and their ring of high pressure is around the tropic of Cancer, and, except for the lower member of surface winds, the currents approach the centre aloft, along a left-handed, inward, descending spiral, and turn to an outward spiral below. In this case, the pressure at the centre would be high, owing to the cold, were it not lowered by the centrifugal force of the whirl. In warm-centred cyclones, the steepest gradients and highest velocities are near the surface: in cyclones of cold centres, they are in the lofty regions. The enormous progress marked by such a generalization may be appreciated by reading the vague and vain theories of other authors. Ferrel's theory of tornadoes is another monument of deductive study, checked by a fulness of knowledge of fact, as far as observations and records allow.

It is not desired to imply by this reference to deductive methods that meteorological observations and their statistical study should in any way decrease: they are, of course, the essential foundation for further study. But it is a matter of regret that so few willing and interested observers go beyond this foundation-work far enough to discover the intense interest of the broader, physical study of meteorological phenomena. We may take pride in recognizing Espy and Ferrel as leaders in modern meteorology, but we must take care also that they have followers.

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HYPNOTISM IN FRANCE.¹

THE voluntary production of those abnormal conditions of the nerves which to-day are denoted by the term 'hypnotic researches' has manifested itself in all ages and among most of the nations that are known to us. Within modern times these phenomena were first reduced to a system by Mesmer, and, on this account, for the future deserve the attention of the scientific world. The historical description of this department, if one intends to give a connected account of its development, and not a series of isolated facts, must begin with a notice of Mesmer's personality, and

we must not confound the more recent development of our subject with its past history.

The period of mesmerism is sufficiently understood from the numerous writings on the subject, but it would be a mistake to suppose that in Braid's 'Exposition of hypnotism' the end of this subject had been reached. In a later work I hope to show that the fundamental ideas of biomagnetism have not only had in all periods of this century capable and enthusiastic advocates, but that even in our day they have been subjected to tests by French and English investigators from which they have issued triumphant.

The second division of this historical development is carried on by Braid, whose most important service was emphasizing the subjectivity of the phenomena. Without any connection with him, and yet by following out almost exactly the same experiments, Professor Heidenhain reached his physiological explanations. A third division is based upon the discovery of the hypnotic condition in animals, and connects itself to the *experimentum mirabile*. In 1872 the first writings on this subject appear from the pen of the physiologist Czermak; and since then the investigations have been continued, particularly by Professor Preyer.

While England and Germany were led quite independently to the study of the same phenomena, France experienced a strange development, which shows, as nothing else could, how truth everywhere comes to the surface, and from small beginnings swells to a flood which carries irresistibly all opposition with it. This fourth division of the history of hypnotism is the more important, because it forms the foundation of a transcendental psychology, and will exert a great influence upon our future culture; and it is this division to which we wish to turn our attention. We have intentionally limited ourselves to a chronological arrangement, since a systematic account would necessarily fall into the study of single phenomena, and would far exceed the space offered to us.

James Braid's writings, although they were discussed in detail in Littré and Robin's 'Lexicon,' were not at all the cause of Dr. Philips' first books, who therefore came more independently to the study of the same phenomena. Braid's theories became known to him later by the observations made upon them in Béraud's 'Elements of physiology,' and in Littré's notes in the translation of Müller's 'Handbook of physiology;' and he then wrote a second brochure, in which he gave in his allegiance to Braidism. His principal effort was directed to withdrawing the veil of mystery from the occurrences, and by a natural

¹ Translated for Science from *Der Spinaz*.