botanical officer, as well as by the necessity for having a botanical officer at the disposal of the Government of India to accompany military expeditions beyond the frontier.

Arrangements were then made, with the concurrence of the local Governments concerned, under which the following territorial division of India was prescribed for the purposes of botanical survey :—

Under the Superintendent, Royal Botanical Gardens, Calcutta.—The Provinces of Bengal, Assam, and Burma, the Andamans and Nicobars, North-East Frontier Expeditions.

Under the Government Botanist, Madras.—The Presidency of Madras, the State of Hyderabad, the State of Mysore.

Under the Principal, College of Science, Poona.—The Presidency of Bombay, including Sind.

Under the Director, Botanical Department, Northern India.—The North-Western Provinces and Oudh, the Punjab, the Central Provinces, Central India, Rajputana, North-West Frontier Expeditions.

The distribution above stated was reported to Her Majesty's Secretary of State, and his Lordship has been pleased to express his satisfaction with these arrangements.

(3) The Government of India now desire to communicate the following observations as to the central position which, in conformity with the suggestions of the Director of the Royal Botanical Gardens at Kew, the officer at Seebpur will occupy in the scheme for the botanical survey of India, and as to the sphere and nature of duties of each botanical officer, so far as they are connected with botanical survey.

It is desirable that the Seebpur Institution—which, as remarked by Mr. Thiselton Dyer, "though technically Provincial, must, at any rate in external estimation, from its age (it has passed its centenary), from its scientific traditions, and from the splendour of its maintenance, rank as Imperial"—should, without any interference with the Provincial control over the Royal Botanical Gardens, be officially recognized as the acknowledged centre of the Botanical Survey of India, and that to it should be referred the solution of all problems rising out of the practical or scientific study of Indian botany. In view of the important position which the Superintendent of the Royal Botanical Gardens, Calcutta, will thus occupy as the central authority in the Botanical Survey of India, the Government of India have, with the concurrence of the Secretary of State, added to Dr. King's present designation the official title of "Director of the Botanical Survey of India," and it is requested that in all correspondence dealing with subjects relating to general botanical exploration the latter title should be employed. The more effective botanical survey of Burma and Assam has also been intrusted to the Director, who will arrange a definite programme each year for the purpose in communication with the Chief Commissioners of those Provinces. He will also submit a separate Annual Report on the botanical exploration and researches effected during the year. The Government of India record with satisfaction that the local Administrations of Burma and Assam have each contributed an annual grant from Provincial revenues as an addition to the Imperial grant for the botanical survey of their provinces.

The investigation of the flora of the Madras Presidency and of the Hyderabad and Mysore States has been intrusted to Mr. M. A. Lawson, the Government Botanist and Director of Cinchona Plantations, who has expressed his opinion that the whole survey of the territories in question might, if diligently prosecuted, be completed in three or four years.

In Bombay, a scheme involving an annual expenditure of Rs. 4500 per annum on botanical work has been sanctioned, and Dr. Cooke, Principal of the College of Science, Poona, is officially recognized as in charge of botanical research in that Presidency. A herbarium exists at the College of Science, and a botanical collection is in course of formation at the Victoria Gardens, Bombay. The former place is to be the head-quarters of botanical research and collections, and the existing herbarium there is to be developed.

By the transfer of the services of the Superintendent of the Government Botanical Gardens, Saharanpur—who now bears the designation of Director of the Botanical Department, Northern India—the services of this officer are, as already explained, available for scientific investigation in all Provinces and States in Northern and Central India, as well as on expeditions beyond the north-west frontier. Mr. Duthie, the officer now holding the appointment, was thus in 1888, by his deputation to accompany the Black Mountain Expedition, enabled to acquire information concerning the flora of a country which had not hitherto been botanically explored. During the last three years, Mr. Duthie has also been deputed to Simla in the hot weather to assist in the preparation of the "Dictionary of the Economic Products of India," and during the same period he has been engaged in the botanical exploration of Rajputana and of the Central Provinces.

M. FAYE'S THEORY OF CYCLONES.

IN his admirable work on "The Principles of Science," the late Prof. Jevons thus sums up the characteristic mental attributes of the great scientific discoverer :—

"He must be fertile in theories and hypotheses, and yet full of facts and precise results of experience. He must entertain the feeblest analogies and the merest guesses at truth, and yet he must hold them as worthless till they are verified in experiment. Where there are any grounds of probability, he must hold tenaciously to an old opinion, and yet he must be prepared at any moment to relinquish it when a single clearly contradictory fact is encountered."

In his theory of cyclones, M. Faye has abundantly proved himself to possess those attributes that are defined in the first phrase of each of these sentences, and particularly the final one. Whether, however, in his treatment of this subject, the manifestation of the remaining and qualifying attributes is equally recognizable ; whether he has fairly grasped and duly weighed all the established facts that are relevant and even essential to his hypothesis ; and whether, among those that he has overlooked, there are not some that are "clearly contradictory" to the requirements of his theory, and therefore fatal to it—these are the questions that I propose to inquire into in the present article.

A true theory of cyclonic storms has not merely a scientific interest, it has also practical bearings of very high importance. When a ship is involved in the outer circle of a tropical cyclone, the vital problem which the seaman has to solve is, how to escape the fearful squalls of the inner vortex and the tremendous cross-seas of the central calm. In order to do this he must be able to judge of the bearing of the storm-centre from the actual position of his ship, and, to determine this point with even approximate accuracy, his sole guide is the direction of the wind. It may well be, then, that the safety of his ship, his own life and those of his fellow-seamen, are involved in the right answering of this question, "Does the storm-centre bear at right angles to the local direction of the wind, or is it from two to four points in advance of this position?" M. Faye's theory assumes and inculcates the former ; the latter is consistent only with the hypothesis of an indraught from all sides, and an ascending current over the storm, the existence of which M. Faye persistently denies.

M. Faye's views on the nature of cyclonic storms are

too well known to render necessary any detailed description of them. An account given by Mr. Archibald in vol. xxxviii. of this journal (p. 149) is quoted without disapproval by M. Faye in his latest publication in the *Comptes rendus*, and may therefore be accepted as just. Its essential points are that cyclones are generated as great eddies in the higher regions of the atmosphere, and that there is a downrush of air in the vortex. "Dans ces tourbillons, tout semblables à ceux qui se forment dans les cours d'eau, les spires, d'abord très larges, iront en se rétrécissant par en bas, et leur girations progressivement accélérées, en vertu d'une loi bien connue de mécanique, amènent au contact du sol, et y concentrent sous une aire bien plus étroite que celle de leur embouchure les énergies continuellement renouvellées du fleuve aérien jusqu'à ce que son élargissement croissant aboutisse à la décomposition du cyclone."

Further on, with respect to the descending current in the vortex, he remarks: "L'air envoyé en bas sera en petite quantité mais animé d'une vitesse de rotation énorme."

I leave aside for the present any criticism of the physical and mechanical actions which M. Faye conceives to take place in these unfortunately inaccessible vortices of the higher atmosphere, and which I, for one, am unable to reconcile either with the results of direct observation or with well-established physical laws. For the moment I wish to concentrate attention on the question of fact, whether there is an indraught of air to the cyclone vortex at the earth's surface, and therefore necessarily an ascending current over it, or, on the contrary, an outflow from a descending current. This is the crucial point of the controversy, and by the answer M. Faye's theory must stand or fall. Indeed, M. Faye seems to recognize this, since he says:—

"L'argument le plus solide, celui qu'on m'opposait toujours pour prouver que l'air était ascendant dans les cyclones, à savoir le fait que les isobares étaient partout et toujours coupés sous un angle assez notable par les flèches des vents, de manière à accuser une tendance nettement centripète, &c."

He admits, too, that in certain cases there is really an indraught and ascent of air; only, on his view, these are not cyclones.

In order to forestall any objection on this score, I will take as the subject of inquiry the cyclones of the Bay of Bengal, the typical cyclones to which Mr. Piddington first applied the name, however etymologically incorrect. I trust, by this restriction, to escape ignominious dismissal from court on the plea that my witnesses are impostors —merely "prétendus cyclones"—and that their evidence is consequently irrelevant.

My first experience of a great tropical cyclone was the memorable storm that devastated the port and city of Calcutta on October 5, 1864. Up to that time, my acquaintance with cyclones was, like M. Faye's, "academic"; and under the impression that Reid's and Piddington's description of the winds, as blowing in circles or at right angles to the radius vector of the vortex, was an established scientific fact, on the evening of that day I sketched out, for the information of some friends, the probable course of the storm that was then passing away, having swept the port of its shipping, and left half the houses around us more or less wrecks. Having no other guide at the moment than the changing directions of the hurricane as experienced at Calcutta, on the supposition that the centre lay at right angles to these directions, I inferred that the storm had reached us from the north-east corner of the bay, and had followed a north-west or west-north-west course past Calcutta. What was my surprise, then, when accounts began to come in from other places in Bengal, showing that the course of the storm had been almost due north; and when, further, on plotting down the wind directions reported from other stations according to the hours at which they had been observed, I found that, instead of being at right angles to the radius vector, they were strongly inclined inwards; and such as, after making all allowances for their being only estimated directions and perhaps, therefore, a point or two in error, could be reconciled only with a sharp spiral indraught to and up to the central calm. Later on, when I obtained copies of the logs of ships that had been involved in the storm in its passage up the bay, I found that their wind observations, equally, were compatible only with spiral directions. Unlike M. Faye, I had no theory to support, and I submissively accepted the teaching of the evidence which lay so plainly before me.

This evidence is set forth on Plates I. and II. of the Report drawn up by Colonel Gastrell and myself, which was widely distributed at the time to scientific bodies, so that, in all probability, a copy must exist in the library of the Académie des Sciences.

Since then, many other storms in the Bay of Bengal have been carefully investigated, and their full details embodied in Reports drawn up by Messrs. Wilson, Eliot, Pedler, and myself. Without a single exception, the evidence thus accumulated has been to the same effect as that of the cyclone of 1864, and these gentlemen have all arrived at conclusions similar to mine. Thus, Mr. Wilson says[1]:—"The following rule may be used to determine the approximate bearing of the centre with as much accuracy as it seems to be possible to arrive at: *In the northern hemisphere, with the face to the wind, the direction of the centre is from ten to eleven points to the right-hand side*"; and, to quote only one of Mr. Eliot's numerous references to this subject,[2] "The air is drawn into the centre [of a cyclone], but is not drawn directly to it. The particles move by a kind of spiral path to the centre." And he gives a diagram, followed by charts of the Balasore cyclone of May 1886 and the Madras cyclone of November of the same year, as illustrative examples. And Mr. Pedler, in summing up the evidence of the False Point cyclone of September 1885, says[3]:—

"It is therefore clear, from these autographic records, that there was a very strong indraught towards the storm-centre, and that for a considerable portion of the time, even when the storm-centre was comparatively close to Hazaribagh, the winds were part of a well-defined spiral system. In fact, for a large part of the time they subtended an angle of less than 45° with the radius of the storm. . . . The records of five anemographs within the influence of the storm . . . show that the theory of the circular movement of winds in a cyclone, which was advanced by Reid and Piddington, and has been supported by some later writers, is utterly untenable. At considerable distances from the storm-centre the winds approach more to the radial direction of indraught towards the centre, as advocated by Espy, than to any circular movement. As the centre of the storm is approached, the circulation appears to become more defined; but even just outside the storm-centre there is no evidence to show that the direction is tangential."

The reports here quoted and many others, all leading to the same conclusions, have been communicated officially to a large number of scientific bodies in Europe and elsewhere, and taken together they probably furnish the most copious and complete body of existing evidence relative to the cyclones of a tropical sea. Not long since I examined the whole of the charts given in these reports, in order to verify Mr. Wilson's rule (quoted above) for ascertaining the bearing of the storm-centre when the

[1] "Report on the Midnapore and Burdwan Cyclone of October 15 and 16, 1874," p. 86. The italics are as in the original Report.
[2] "Hand-book of Cyclonic Storms in the Bay of Bengal," p. 14 (1890).
[3] "Indian Meteorological Memoirs," vol. iv., Part 2, p. 127. The barometric reading recorded when the centre of this storm was passing False Point Lighthouse is the lowest that has ever been observed at the sea-level.

local wind direction is the only datum available, and I found that in the north of the Bay of Bengal, as the mean result of 132 measurements, the angle included between the wind arrow and the radius vector of the vortex was 122° (or 32° greater than a right angle), and that of twelve positions within 50 miles of the storm-centre, that is to say, in the inner circle of the hurricane, 123°. In the south of the bay it was 7° greater. Prof. Loomis, taking into account the land as well as the marine observations, and all barometric depressions, whether storms or otherwise, obtained an angle 25° greater, and differing only by 33° from the radial direction. It is hardly necessary to refer to Prof. Loomis's results of his examination of the Manilla cyclone of October 1882, which gave an angle of 118°, or to Mr. Meldrum's work on the cyclones of the South Indian Ocean, which has already been quoted by Mr. Archibald in his article in NATURE, mentioned above. All testify uniformly and in the strongest manner to the sharp spiral indraught of the winds in tropical cyclones, so that, as Prof. Loomis has truly remarked, "we thus see that tropical storms are spouts and not cyclones, and it is unfortunate that the term cyclone should have been ever adopted." In this view I fully agree, and I make M. Faye a present of the admission, that in an etymological sense, if in no other, Mr. Piddington's typical cyclones are not cyclones at all.

With all these results of a quarter of a century's experience present to my mind, when a gentleman holding the high position of M. Faye reiterates the assertion that the winds of tropical cyclones blow in circles, and that if ever they are found to blow spirally inwards such instances are not true cyclones (in the ordinarily accepted, *i.e.* denotative, meaning of the term), the impression I receive is somewhat such as M. Faye would probably experience were some equally eminent scientific authority to assert in his presence that the Ptolemaic system truly represents the relative movements of the sun and planets, and that the heliocentric scheme of Copernicus is a "prétendu système." If, indeed, M. Faye prefers to avail himself of the admission made above, to relegate Mr. Piddington's typical cyclones to the category of "prétendus cyclones," and therefore to exclude them from his theory, my present argument falls to the ground; but in that case his cyclone becomes the mere abstract definition of a term, and it remains to be shown that there is anything corresponding to it in Nature. That, however, in his latest communication to the *Comptes rendus*, he intended his assertions to apply to these tropical cyclones is abundantly apparent.

Can it be that M. Faye is unacquainted with the mass of original evidence embodied in the Indian cyclone reports, in Mr. Meldrum's writings on the cyclones of the South Indian Ocean, and with Prof. Loomis's work, in which these and many others are discussed? It would indeed seem so, since in none of his writings have I ever seen any reference to any other Indian author than Mr. Piddington, and even in his case it is difficult to believe that M. Faye has done more than simply accept Mr. Piddington's conclusions, without attempting to verify them by an examination of the original data. But if this be really the case—if he has taken so little pains to ascertain the fundamental facts, and to test the soundness of his speculations by an appeal to the evidence of the last twenty-five years—it is indeed strange that he can put forward confident assertions on a matter with which his acquaintance is so imperfect, and that he can disseminate statements that are demonstrably erroneous, and may be fraught with danger to the lives and property of those who accept him as their guide, backed with the high authority that must necessarily attach to his name.

It is a far from edifying spectacle to see such a man, in his latest communications to the *Comptes rendus*, quoting with complacency any isolated passage in the writings of leading meteorologists which seems to promise some support to his tottering theory, and ignoring all that would tell against it. That such cyclones as originate beyond the tropics are, in the first instance, movements of the higher atmosphere, has been rendered very probable by Dr. Hann's demonstration of the temperature relations of cyclones and anticyclones; but nothing that Dr. Hann has ever written has shown that he is in the least inclined to accept M. Faye's strange hypothesis of a descending current as the leading feature of cyclones and tornadoes. That the clearing of the skies in the central calm of a tropical cyclone may be due to the descent of a certain amount of air, although not decisively proved, is yet not improbable; but what would be thought of a man who, standing on a river bank, and seeing an upward current in the back-water immediately below him, should shut his eyes to the broad stream beyond, and assert, on the strength of his observation, that rivers flow from the sea to the mountains? Yet such, and no other, is the relation of this descending current to the great body of the cyclone. All may admit, with Prof. von Bezold, that there is much in the views hitherto prevalent as to the origin of cyclones and anticyclones that requires modification, and it may yet be long before these phenomena are fully and satisfactorily explained. There are many points of difference between the storms of the tropics and those of the temperate zone which seem to show that the forces that are principally active in the former play but a secondary part in the latter. But certainly there is no apparent tendency on the part of the leading meteorologists of Europe and America to accept M. Faye's *idolon specus* as a true theory of cyclones and tornadoes, nor is it in the least likely that such will ever be witnessed.

HENRY F. BLANFORD.

NOTES.

THE arrangements for the meeting of the British Association are now nearly complete. In a former note we referred among other matters to the excursions. We now learn that among them the organization of the pedestrian excursions to the Black Mountains is so far advanced that the detailed programme is now ready, and can be obtained by application to the Local Secretaries.

THE Royal Archæological Institute of Great Britain and Ireland opened their annual meeting in Edinburgh on Tuesday. At noon there was a reception of the members in the National Portrait Gallery by the President and Council of the Society of Antiquaries of Scotland. The inaugural meeting took place in the lecture-hall of the Royal Geographical Society. Sir Herbert Maxwell, on taking the chair, remarked that the closing years of a century naturally suggested the process of stock-taking, and as they had arrived at the last decade of a century which claimed to have witnessed beyond all precedent the accumulation of scientific knowledge, it was not unnatural that they should direct inquiry into the standing obtained by that particular branch of science in which they were all concerned. After a brief summary he stated that one of the problems which was pressing upon antiquaries at the present time was that relating to those mysterious rock sculptures which from time to time were found in increasing numbers all over Scotland. They bore a striking resemblance to similar rock sculptures found not only in Scandinavia and Central Europe, but in such remote parts of the earth as Asia, and Northern, Central, and Southern America. They could hazard no guess even at the race by whom they were made, still less at the object of their authors. All they could do was to record the discovery of them with careful drawings, and wait till perhaps light would flash upon them from the habit of some uncivilized tribe or from a passage in some hitherto unnoticed writer.