and working at the same speed, the motion to the one hand will produce *red*, and when reversed, *blue?* Mr. Hannay also seems to imply that the colours of my experiments to be seen well, should be looked at passively or without keen attention. On the contrary, the more light thrown on them, and the keener, fresher, and younger the eyes of the observer, the more brilliant are the colours, and if a boy of ten or twelve years old, who never saw anything of the sort before, be called in, he will describe them better than grown people.

Scientific men have hitherto considered it a sufficient explanation of these experiments to say the effects are "physiological," as if colour were ever anything else. Newton says, speaking of *coloured rays*, that he uses the term merely to suit the understanding of the vulgar, as they are nothing but a certain power and disposition to stir up a sensation of this or that colour. Prof. Ogden Rood again classes these as *subjective* colours, a word which, if it has any meaning at all beyond a very limited one, can have none with reference to colours which remain permanent so long as the machine is in motion.

27, York Place, Manchester, April 22 NAPIER SMITH

How may Clouds consisting of Liquid or of Frozen Water be Distinguished?

IN NATURE, vol. xxv. p. 529, M. de Fonvielle asks my opinion as to what observations may be made in a balloon to discover whether in a cloud whose temperature is below zero the minute particles of water are liquid or solid.

There may be difficulties in the way of deciding by direct observation of the form of the particles, whether they are globules or crystals. But since H. B. de Saussure, a century ago ("Essai sur l'Hygrométrie), by means of a single lens, could distinguish in the air over heated water, globules of condensed water of different size, greater ones which appeared to him full, and smaller ones which he regarded as hollow; and when in more recent times A. Waller (*Philosoph. Transactions*, 1847) could make his "microscopic observations on the so-called vesicular vapour of water as existing in the vapours of steam and in clouds, &c.," with the result that he denied the existence of mist-vesicles, it seems possible that by means of a magnifying instrument the form of the particles suspended in the air can be recognised.

A sure evidence, but obtainable only under favourable circumstances, that the particles forming a cloud are ice-crystals would be the observation of the halos of 22° and 46° radius and of parahelions as produced by the cloud. HERMANN KOPP

Heidelberg, May 3

On the Conservation of Solar Energy

Dr. SIEMENS's theory of the constitution of the sun implies that there is an absorption of solar rays constantly going on in space. If this is so, space cannot be perfectly transparent.

An astronomer of the early part of the present century—I think it was Olbers—came to the same conclusion, though from different reasons. He found that as the space-penetrating powers of the telescope is increased, the number of stars that become visible does not increase so rapidly as it would if they were evenly scattered through space, and if space were perfectly transparent; and he concluded that most probably space is not perfectly transparent. This, however, is by no means conclusive, because it is possible that the reason why the number of stars that become visible does not increase as it ought to do on the supposition, is that the number of stars in the universe is limited.

JOSEPH JOHN MURPHY

Old Forge Dunmurry, co. Antrim, May 3

CYCLONES[1]

II.

IN our former article we dwelt on the deductions arrived at by the author from a consideration of the mechanical theory of cyclones. We will now proceed to examine how far such theoretical relations are corroborated by the results of observation. The results of observation utilised by the author comprise those of the Rev. W Clement Ley, published in his "Laws of the Winds"; those of Prof. Loomis, deduced from a study of the U.S. Signal Service charts; those of Dr. Hildebrandsson, with regard to the upper currents from an examination of the Danish synoptic charts; those of Capt. Toynbee, from a study of the Atlantic storms; and lastly, some contained in a recent work on the hurricanes of the Antilles, by Padre Viñes of Habana. Mr. Ferrel at the outset pointedly remarks that for a mariner to be able to make use of the laws deduced from a study of the theory of cyclones, not only a knowledge of such laws is requisite, "but likewise of the *normal* states of the wind and of the barometric pressure in all parts of the ocean and at all seasons of the year, unaffected by the abnormal disturbances of these progressive cyclones; since with a knowledge of the normal conditions of the winds and of the barometric pressure at any time and place he can perceive the first indications of the abnormal disturbances which are the forerunners of these storms, and so can be on his guard, and then with a knowledge of these storms or cyclones, he can generally avoid at least their most dangerous part."

With regard to the first result from theory, viz. the general incurvature of the winds in a cyclone, which was formerly altogether denied by the cyclonists—so-called—Reid and Piddington (not Redfield), or inordinately magnified in every case by Espy, and other upholders of the radial theory, there seems to be no doubt from the results of observation here given, as well as from others not cited by the author, that the wind deviates to a considerable extent from the tangent to the isobars inwards towards the low centre. Moreover, in accordance with theory, this inclination is greater at inland stations where there is more surface friction than at or near the sea when it is less. Thus Ley found the inclination to be about 29° for inland stations, but only 13° for those on the coast. This difference between the inclination at sea and on land may perhaps account for the tenacity with which sea captains still cling to the notion that the wind blows in circles coincident with the isobars; since it is precisely at sea where the incurvature should theoretically be least. The increase in the inclination corresponding to a decrease in the latitude is likewise borne out by observation. Thus from Ley's observations, which embrace North and South Europe, the mean inclination to the isobar is about 25°, from those of Capt. Toynbee on latitude 50° it is 29°, from those of Loomis nearer the equator in America 47°,[1] from those of Padre Viñes in the Antilles 45°, and we may add from some in the Bay of Bengal, mentioned by Blanford, about 42°.

It is thus pretty evident, as the author remarks, that "though the horn-cards of Piddington based on the strictly circular theory of the winds may still be used at sea in high latitudes without great error, yet nearer the equator they must become more erroneous, and entirely fail at the equator if cyclones could exist there." Mr. Meldrum, of Mauritius, as far back as 1867 drew attention to the disasters which resulted in consequence of vessels having estimated the direction of the centres of cyclones according to the rules of the circular theory, and since the publication by M. Faye of his "Défense de la loi de tempêtes," has strenuously opposed the resuscitation of this exploded doctrine. He has also lately given an admirable proof of the truth of the incurvature, by publicly announcing when the wind, in a cyclone on March 21, 1878, was from the north-east in Mauritius, that the centre of the storm was not to the north-westward according to the circular theory, but to the west-south-westward, which was afterwards found to have been the case.

It is a manifest duty therefore which mankind owes itself, if the dangers of the sea are to be minimised, that the amount of inclination of the wind to the isobar should be determined by observation in different seas and for

[1] "Methods and Results of Meteorological Researches for the use of the Coast Pilot." Part II.—On Cyclones, Waterspouts, and Tornadoes. By William Ferrel. (Washington, 1880.) Continued from p. 12.

[1] Some of these included very gentle winds.