

resembling the folds of a curtain passed rapidly across the northern sky. The light was colourless, with occasional flashes of crimson.

H. MELLISH

Hodsock Priory, October 9

Newton, Wollaston, and Fraunhofer's Lines

IN most of the current treatises on spectrum analysis, on the spectroscopy, and on optics generally (Lloyd's works being exceptions), injustice is done to Newton's care, and scientific insight in his optical experiments, when Wollaston's discovery of the dark lines in the solar spectrum is alluded to, by most positive statements to the effect that Newton never used the slit, or that Wollaston was the first who ever made observations on the pure spectrum.

That the statements are erroneous may be seen by a comparison of the following extracts from Wollaston's paper in the *Philosophical Transactions* for 1802, p. 378, and Newton's *Opticks* edition of 1704.

Wollaston concludes from his experiments that "the colours into which a beam of white light is separable by refraction, appear to me neither 7, as they usually are seen in the rainbow, nor reducible by any means (that I can find) to 3, as some persons have conceived, but that by employing a very narrow pencil of light, four primary divisions of the prismatic spectrum may be seen with a degree of distinctness, that, I believe, has not been described nor observed before." He describes the experiment as follows:—

"If a beam of daylight be admitted into a dark room by a crevice 1-20th of an inch broad, and received by the eye at a distance of 10 or 12 feet, through a prism of flint glass free from veins [italicised by Wollaston], held near the eye, the beam is seen to be separated into the four following colours only, red, yellowish, green, blue, and violet." He then describes four lines marking these divisions, together with two others for which he does not offer any explanation.

Compare with this Prop. 4 of Book I. of the "Opticks," which is "To separate from one another the heterogeneous rays of compound light." Newton, after showing at some length why he uses a lens to "diminish the mixture of the rays," describes Experiment 11, p. 47, as follows:—

"In the sun's light let into my darkened chamber through a small round hole in my window-shutter, at about ten or twelve feet from the window, I placed a lens, by which the image of the hole might be distinctly cast upon a sheet of white paper. . . . Then immediately after the lens I placed a prism, by which the trajected light might be refracted either upwards or sideways." The "oblong image" thus formed he received upon paper placed "at the just distance where the rectilinear sides of the image became most distinct." By varying the size of the hole, he made "the mixture of the rays in the image to be as much or as little as I desired." For this purpose he caused the breadth of the image to be sometimes sixty or seventy times less than its length.

"Yet instead of the circular hole, 'tis better to substitute an oblong hole shaped like a long parallelogram, with its length parallel to the prism. For if this hole be an inch or two long, and but a tenth or twentieth part of an inch broad or narrower; the light of the image will be as simple as before, or simpler, and the image will become much broader, and therefore more fit to have experiments tried in its light than before."

For the purpose of comparing the simpler light with the more compound, he used also a hole of the shape of an isosceles triangle, whose base was "about the tenth part of an inch, and its height an inch or more" (the width of which, therefore, at a quarter of an inch from the vertex, would be one-fortieth of an inch). The refracting edge of the prism was parallel to the perpendicular of the triangle. The images would therefore be "equicrural triangles," "a little intermingled at their bases, but not at their vertices."

He is very emphatic as to the precautions in making the experiments. He was always careful to have the image in the position of minimum deviation—all foreign light must be carefully excluded from the chamber. The lens must be good—the prism being made of "glass free from bubbles and veins," with its sides truly plane and its polish elaborate." "The edges also of the prism and lens, so far as they make any irregular refraction, must be covered with a black paper glued on." "It's difficult to get glass prisms fit for this purpose."

He did not, as is sometimes supposed, always receive the

images on paper, for in Expt. 4, Prop. ii., p. 22, he says: "I looked through the prism upon the hole."

That with good prisms, and the great variety of experiments which he must have tried, he did not see the dark lines by looking through the prisms, seems remarkable. It may possibly be explained by the fact that in the very class of experiments in which he was most likely to discover the lines (and in which Wollaston actually discovered them), he found himself obliged to rely on the observation of an assistant. This is mentioned on p. 92 in Prop. iii. of the second part of Book I. The proposition is "To define the refrangibility of the several sorts of homogeneous light, answering to the several colours." In this he says: "I delineated therefore in a paper the perimeter of the spectrum" . . . "and held the paper so that the spectrum might fall upon this delineated figure, and agree with it exactly, whilst an assistant, whose eyes for distinguishing colours were more critical than mine, did by right lines drawn across the spectrum note the confines of the colours."

ALEXANDER JOHNSON

McGill College, Montreal, Canada, September 19

The Spectroscope and Weather Forecasting

I MUCH regret that Prof. Smyth should have taken the word prognostic, applied to the rain-band as a depreciatory epithet, when it was only intended as a term of classification. In common parlance any particular "look" of the sky is called a prognostic, and it is a natural extension of the idea to call the "look" of the sky absorption a spectrum prognostic also.

The question at issue is this. Assuming that the rain-band is a quantitative measure of the amount of vapour in a section of the atmosphere, why is it of more use in forecasting than the numerous sky and other prognostics which also indicate an excessive amount of vapour, as, for instance, sweating walls, or a soft sky. Like them, it precedes rain in certain cases, and for the same reason; like them, it fails in numerous cases where rain falls without being preceded by excessive vapour quantity; and, like them, it cannot compare in forecasting value with synoptic observations over a large area, which correlate moist currents with isobaric lines.

But there is one case in which the rain-band may give valuable information—when we have a vapour-laden upper current over a dry surface wind. This often occurs in winter, with a warm south-west current over an area of frost and an east wind. In practice this almost invariably makes itself visible by the long converging stripes of cirrus which so often precede a rainy thaw, but still cases may occur when no cirrus is formed, or it is otherwise invisible. Here is a case in point. One spring morning in London there was a thick fog, with a south-west wind. About 1 a.m. the wind shifted to east, the pavement remaining white and dry; when, to my surprise, the ordinary spectrum of a fog was crossed by a strong rain-band. Two hours afterwards a few big drops of rain fell, which soon ceased, and the wind returned to the south-west.

The 8 a.m. chart showed London to be then on the northern edge of an anticyclone, with a small secondary cyclone over Devonshire; this moved on during the day, bringing rain with it, which soon passed.

We can now estimate the different values of the several indications. Cirrus, if there is any, tells that a moist south-west upper current has set in, but not if it is specially vapour-laden.

The rain-band tells us simply that there is an abnormal amount of vapour somewhere, and roughly measures it; by inference, from the dryness of the ground, we know that the vapour must be overhead; in very rare cases the band would speak before the cirrus, and in any case would show unusual vapour, which the cirrus could only suggest by looking softer than usual. On the other hand, the spectroscope would be silent in numberless cases where cirrus would indicate rain correctly, and neither could tell their story till the vapour-bearing current had set in.

The forecaster, who used synoptic charts, would know that a damp south-west wind always blew over the north-east wind in front of a cyclone, and in a case like this could say that as the secondary approached, the moist upper current would set in some hours before it arrived, and would be so far ahead of any prognostic; but he would have no means of saying if the current was extra vapour-laden or not.

But in most cases a knowledge of the fact would be but of little service to him. Suppose that in this case there was rain at Plymouth, cirrus or rain-band at Portsmouth, and blue sky over

fog in London, the knowledge of this rain-band would not help him much, for he knows by his chart that rain has already set in.

What he does want to know is whether the cyclone will move northwards, eastwards, or southwards. This no prognostic can tell him; the only known clue to a cyclone path is got from a knowledge of the movements of isobaric lines. In this instance the rain in London was, I think, correctly forecast, but unfortunately such a simple case rarely occurs in this country.

Thus we see that a knowledge of the amount of moisture in any current is of only secondary importance to synoptic forecasting, so that if we may welcome the rainband as an addition to our old stock of prognostics, there is little ground for hoping that it will be of further service than them.

All that Prof. Smyth claims for the spectroscope is to act as a gauge of pure vapour quantity, but it seems probable that its employment may be still further extended. There are strong grounds for believing that an air spectrum may vary not only with the amount of pure vapour, but also with the size, aggregation, and physical condition of the condensed vapour suspended in it. For instance, take the so-called rain-lines. These may appear either alone, or with a rain-band of any intensity; so that if the band is due to pure vapour only, the lines must depend on some other condition. Again in sunset tints we have a natural spectroscope whose colours certainly are the product of both the quantity and quality of the total moisture suspended in the air. I have made a large number of observations on the lurid, coppery, yellow, green, and red skies, which form such a large portion of all weather lore, but without decisive results; for sunset spectra are too complicated and too fleeting to be unravelled by a small instrument. They certainly seem to differ, but their spectra are not so marked as their appearance to the naked eye.

But even supposing that this idea is completely verified, and that the spectroscope can be used as a new weapon of research to discover the still unknown nature of clouds, and that we are ever able to say that such and such an absorption spectrum belongs to such and such a kind of sky, there are no grounds for believing that we can ever regard these spectra otherwise than as a new set of sky prognostics, or that as such they will be of more use in forecasting than those already known.

What the use of any prognostics is in forecasting, and how they are related to synoptic charts, and how isobaric lines map out the shape of rain areas, are other sides of the great problem of weather forecasting, which cannot be discussed here.

Some may differ from Prof. Smyth as to the forecasting value of the rain-band, but all will appreciate the singular skill with which he has surmounted the practical difficulties in the way of making it a quantitative measure of atmospheric vapour.

21, Chapel Street, S.W. October 2 RALPH ABERCROMBY

The Comet

WHEN observing the comet this morning, with $7\frac{1}{2}$ inch aperture and powers of 70 and 150, I at once noticed that the nucleus was far from circular, the length being carefully estimated at $45''$ and the breadth at $15''$, while the measured "Position" of the maj. axis was (96° – 276°); this was also the supposed direction of the tail, which had ceased to be visible in the increasing twilight.

At 6h. om. G.M.T. the place of the comet was

$$\begin{aligned} \text{R.A.} &= 10\text{h. } 27\text{m. } 3 \pm 5 \text{ secs.} \\ \text{N.P.D.} &= 100^\circ 36' 30'' \pm 10''. \end{aligned}$$

These places, taken with the equatorial, were confirmed by measures of the not far distant star α Leonis. They differ considerably from the calculated place given in the Dunect Circular, No. 60.

WENTWORTH ERCK

Sherrington House, Bray, Co. Wicklow, October 9

"Note on the History of Optical Glass"

THE writer of the article in a recent number of your journal, entitled "Note on the History of Optical Glass," has fallen into some historical blunders and anachronisms, which are the ground of my addressing you. My grandfather was born in 1738, and would therefore have been but twenty years of age at the date when he is said to have made the acquaintance of the elder Guinand, then sixteen, in Switzerland. It is almost certain that he never was there, at any rate not as an "illustrious savant," engaged in telescopic experiments. His sister's memoirs present

a blank at this exact date, but it is evident that if he travelled at the time when he withdrew from the Hanoverian military service, it was in the character of an obscure young musician. It is just barely possible that there may be some foundation for the story now given—and if so, I should be glad to learn it—but a totally mistaken colour has been given to it by drawing on the future. What follows is still more erroneous. Dollond (the elder) was at this period at the zenith of his fame as an optician; Faraday was not born, and Herschel was an ex-bandsman; yet we are told that he "returned the following year with Dollond and Faraday." It is probably something more than a mere coincidence that about sixty years later the son of that Herschel, the son of that Dollond, and Faraday, were associated in treating with the son of that Guinand for the glasses manufactured by the latter. Apart from this, I submit that hardly anything new is contributed in the "Note." All, and more than all, which it contains will be found in the *Biographie Universelle*, under the name GUINAND, where also is mentioned the Swiss rencontre, but with the name Dros in lieu of "Herschel and Utzschneider." According to the *Biographie*, Guinand was born "about 1745," and died in 1825. It was in 1821 that the Astronomical Society was instigated to make inquiries (conducted by my father) regarding Guinand's optical glasses.

J. HERSCHEL

A "Natural" Experiment in Complementary Colours

ABOUT two miles above Ormeim, in the Romsdal (Norway) is the well-known Slettafos, an imposing cascade formed by the impetuous Rauma, which here plunges through a deep rocky ravine. Fascinated by the scene, I stood watching the foaming water for some time, and all at once noticed a most beautiful and delicate rosy pink tint colouring the foam and spray in the ravine. The water, where not broken up, was of a green colour, and the pink tint was at once explained as its complementary. But the point of special interest to me was that this pink colour was not visible except on those parts of the spray and foam which were in the shade of the gorge. In the full light these appeared, as usual, white. The result above described is an excellent illustration, afforded by nature herself, of the advantage of toning down the brightness of the white surface, upon which we wish to evoke a complementary tint, until it no longer exceeds that of the exciting colour—the green in this case.

CHAS. T. WHITMELL

H.M. Inspector of Schools

9, Beech Grove, Harrogate, September 11

Animal Intelligence

IN the article on animal intelligence (NATURE, vol. xxvi, p. 523), Mr. Morgan seems to me to have inverted the real process in the case of what he calls "isolation," for he says: "I believe such abstract ideas to be impossible for the brute. I believe them to be the outcome of the use of language." The process of abstraction here alluded to is the conception of a quality apart from the things that possess that quality, as whiteness or edibility.

I watched a little child just able to walk alone, on a railway platform. It went up to a square box, and after staring at it for a few seconds, slowly passed its hand over the top, front and sides, and then along the edge, clearly testing the sense of sight by that of touch. It then did the same with the small wheel of a luggage barrow. It was obviously too young to be able to speak, but I think we may safely assume that it got a notion of what we call "square" and "round." Now a dog can readily acquire a somewhat similar experience, by finding a barrel to be less easy to stand upon than a square box. So far they are much alike; the child, however, certainly exhibited a greater inquisitiveness than a dog is likely to do.

It is obvious that a dog can receive just the same impressions as a child through the senses, so that automatic appreciation of the difference between roundness and squareness is common to both; but, whereas, the dog, as I believe with Mr. Morgan can never get beyond that stage, a child, if not an infant, can make the difference an *object of thought* or a mental abstraction, even without having a word to express it; just as an adult experiencing a new but uncomfortable sensation, can think of it, and coin a term to express it, say, "all-overish-ness;" or again, as one can *feel* indignant or benevolent, and at the same time think about such states without necessarily giving expression to them. If words were necessary, as Mr. Morgan seems to think, then a deaf-