Diffraction is caused by the regular reinforcement and destructive interference of light waves from a small source. You can see this effect when looking at a light source (not the sun!) and bringing your thumb and forefinger together but leaving tiny gaps at the creases in your forefinger. A ring with bright and dark bands across it – the diffraction bands – will be seen if you look carefully. A similar set of regular brighter and darker bands is also created by diffraction around the edges of very small particles, like cloud droplets. On a large scale, this results in a corona. Each wavelength of light will produce slightly different bands of light and dark; in combination this leads to the colouring of the main coronal disc, and the occasional presence of coloured rings beyond the main aureole.

Other diffraction effects are also observed in the atmosphere. One of the most common is the iridescence, or coloured banding, of broken clouds or around the edges of larger clouds (see front cover). Coronae depend on a relatively uniform droplet or ice particle size but if small clouds are evaporating then the droplet size distribution, or the size of small ice crystals, can vary significantly across a small angular distance. Visible diffraction bands will then only occur for parts of the spectrum, giving distinct bands of colour to the cloud. The droplets or tiny ice crystals need to be locally very uniformly distributed for this effect to occur.

The glory is another diffraction effect, but an unusual one (see Fig. 7, p. 247). If you are above a cloud on a mountain or in an aircraft, with your shadow being cast on to the cloud top, your shadow will appear to have a bright outline of similar angular dimension to a corona. This is the glory. It is probably caused by diffraction of light by water droplets, but backward diffraction, involving a reflection, rather than the forward diffraction which produces a corona. A good physical explanation of this phenomenon is still lacking. There are a number of similar diffraction effects which Greenler (1980) and Lynch and Livingston (1995) discuss in some detail.

Conclusion

The regular and careful observation of the sky is very rewarding. Practically every day variations on the optical effects discussed in the two parts of this article can be seen and thinking about the causes of this variability will immeasurably increase your understanding of how the atmosphere works.

Acknowledgements

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Letters to the Editor

Correspondents are requested to follow the undermentioned guidelines when submitting letters to be considered for publication: the material should be typed or clearly written on one side of the paper only, with wide margins at top and bottom as well as at each side of the text, and each line double-spaced.

Very cold easterlies, December 1996/ January 1997

In a recent letter (*Weather*, **52**, pp. 97–98) I discussed the cold, dull and dry weather produced by stable easterly winds in March 1996. Stable easterlies again prevailed from 20 December 1996 until 9 January 1997, but there were interesting differences from March 1996.

Temperature: The surface temperature of the air leaving the Continent was much lower than in March 1996. Around New Year's Day it was so cold that over the area for which the sea track for an east wind is short (*i.e.* the south of England) the temperature remained below freezing all day in many places. On 2 January 1997, Hastings (East Sussex) had a maximum temperature of only −3.3°C.

Sunshine: On 23 and 24 December 1996 the lowest layers of the air leaving the Continent were so dry that the cloud sheet which usually occupies the upper part of the surface convective layer during stable easterlies in winter (as in the dull March of 1996) failed to form where the sea track was quite short. The result was that the south of England enjoyed two consecutive cloudless days. Heathrow measured 7.1 hours of sunshine on 23 December, and Cambridge 6.6 hours on 24 December.

Precipitation: The *Weather Log* summary describes falls of snow as light in December, and mainly light in January, but there was one outstanding exception – a snowfall on 27 December and another on the 29th/30th gave a total depth of 25 cm over the North Downs in east and mid Kent. It is significant that the trend of the Downs in east and mid Kent was nearly perpendicular to the low-level wind direction (east-north-east). I also note that at Hurstmonceaux (East Sussex) between 26 and 27 December the 700 mbar temperature fell by 6degC, and between 29 and 30 December by 9degC. So a plausible explanation is that orographic uplift over the Downs in Kent, reinforced by each arrival of colder air at middle levels, resulted in two periods with frequent moderate or heavy snow showers, especially on windward slopes.

Egham,
Surrey

F. E. Lumb

A not so unusual cloud over the Cambridgeshire fens?

With reference to the letter by Jim Galvin in February (*Weather*, **52**, pp. 58–59), I feel it must be challenged on two counts, both meteorological:

(i) Is the analysis of the tephigram shown in the letter correct?

(ii) Is the Aughton sounding for 1800 GMT representative of the air over Wisbech, where the relevant cloud was photographed?

The answer to both these questions must be 'no'.

It is a shame that Mr Galvin gives no surface parameters for the conditions in the Wisbech area at the time, and also fails to give any indication of the overall synoptic situation. Taking observations for my own station at Wokingham, Berkshire, to be broadly representative of the conditions inland on 5 August 1994, I find a day with a maximum temperature of 26 °C, 9.1 hours of sunshine, and a temperature of 22.8 °C and dew point 13.8 °C at 1900 GMT. The wind was a light north-westerly, highest hourly wind 330° 5 kn, maximum gust 12 kn at 1710 GMT. The synoptic chart for 1200 GMT shows a developing ridge of high pressure located over Ireland, and a weak cold front lying north to south over the North Sea.

Analysis of the tephigram shown by Mr Galvin indicates the θ_w (wet-bulb potential temperature, a useful airmass tracer and analysis tool), to be nearly constant at 12.3 °C from 1010 to 915 mbar, falling slightly to 11.9 °C from 915 to 850 mbar, then rising to 13.3 °C by 735 mbar. It should be remembered that an atmospheric sounding is not only representative of the air in the immediate vicinity of the instrument, but that it can also reveal something of the history of the air sampled. The almost constant θ_w up to 850 mbar shows that, despite the current stable layers, the lower atmosphere is well mixed, indicating a recent history of convection, at least up to 850 mbar. The inversion and dry layer from 915 to 850 mbar is typical of air which is, or recently has been, subsiding. This layer marks an effective lid for any existing convection for the surface conditions at Aughton at that time. The weaker stable layer from 975 to 915 mbar is within the boundary layer, and may be associated with small-scale vertical motions on a cloud scale, or perhaps with a sea-breeze circulation. Taken at face value, convective plumes originating at the surface would have remained 'dry', and would have been limited to 975 mbar. However, assuming the weaker stable layer to have been transient, some plumes may have reached 915 mbar, then cloud would have formed near 960 mbar, giving shallow cumulus with stratocumulus up to 915 mbar.

Two factors preclude Mr Galvin's hypothesised development of stratocumulus castellanus above 915 mbar:

(i) The marked descent in the 915 to 850 mbar layer.

(ii) The increase of θ_w with height from 850 to 735 mbar. For castellanus to form, the air in the layer must be potentially unstable, that is θ_w must decrease with increasing height. This requirement was clearly not met in this case.

Concerning the representativeness of the Aughton ascent for the Wisbech area, one only has to consider the location of the former with respect to the ambient flow to see that it was located near the windward coast; thus we would expect the ascent to be characteristic of the air's recent passage over the sea. The near-constant θ_w up to 850 mbar, and its value near 12 °C, would be typical of a cool airmass being slowly modified by convecting to water temperatures near 15 °C. If we imagine this profile transported to an inland location and subjected to a diurnal heating cycle, there would be enough energy in August to raise the surface temperature to above 20 °C. Calculation of the 1000 to 850 mbar thickness for the ascent shown gives a value of 1350 dam, which would support a maximum temperature near 23 °C at the time of year. Such a value would allow convective plumes to reach the inversion at 735 mbar, providing there was enough moisture to form cloud, and provided that there was no further subsidence in the lower layers, which is unlikely given the synoptic situation.

The cloud photographed, then, is unlikely to have been as described by Mr Galvin, but is more likely to have been straightforward cumulus with stratocumulus or altocumulus cumulogenitus, given the inland temperatures quoted earlier, and the probable extensive synoptic-scale subsidence occurring above 700 mbar.

Wokingham, Berkshire **Bernard Burton**

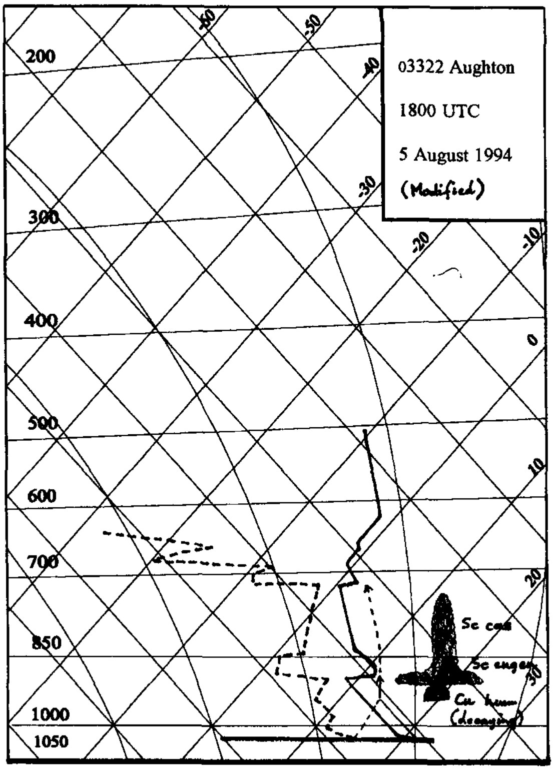

Fig. 1 Modified tephigram for Aughton (Lancashire) at 1800 GMT on 5 August 1994

Jim Galvin replies:

Bernard Burton's comments on my cloud photograph with regard to the synoptic situation on 5 August 1994 are interesting. They show the different ways in which meteorological patterns may be interpreted – indeed, some of the difficulties which must often be faced even in the settled conditions which prevailed at the time. However, his comments, whilst adding to the information I provided, do nothing to invalidate my own description, although I note his comments on the representativeness of the Aughton ascent which, on reflection, I should perhaps have modified somewhat.

In this case, the ascent, as shown, was that most applicable to the cloud observed. Nevertheless, it cannot represent small-scale differences which are likely to have been present, especially near the surface. At the time the photograph was taken, I contend that the air temperature was probably about 21 °C, although this will be hard to verify for any one location given that cloud amounts had been very variable during the day. Given that the Aughton ascent may be considered to be broadly representative, a dew point of 14 °C would have been just sufficient to form cumulus clouds with bases at about 2800 ft (about 935 mbar in this case), comfortably below the inversion base at 915 mbar. However, this temperature was not sufficient, for the most part, for continued uplift above 915 mbar, thus causing the stratocumulus layer to form. By trained eye, the stratocumulus base was at a height probably above about 3000 ft. This is again consistent with the Aughton temperature profile. Despite the near-constant wet-bulb potential temperature in the near-surface layers, only isolated cumulus clouds could have reached the 850 mbar level, given these temperatures. I have redrawn my Fig. 2 (see Fig. 1) showing both a revised version of the Aughton temperature profile and the likely temperature of a rising bubble of convective air.

Although mass descent, as indicated by the profile, is typical of anticyclonic weather, it is not in itself sufficient to prevent cloud formation, which is convective and, therefore, adiabatic – that is (theoretically, at least) independent of the local conditions as long as the bubble of air which produced the cumulus cloud remains warmer than its surroundings. The behaviour of the bubble of air once it reached the stable layer is much more difficult to be sure of. However, given a surface temperature of 21 °C, convection is likely to have been sufficiently vigorous locally to allow some clouds to convect above the stable layer, as described by Bernard, although most would simply spread out, forming the stratocumulus cumulogenitus shown. Observation of the cloud development included unstable tops to the layer of stratocumulus after the cumulus clouds had started to decay. Thus I have assumed that a portion of the saturated air did rise into the base of the layer above 875 mbar. This air, with a wet-bulb potential temperature of 17 °C, could, once again, rise adiabatically. Being saturated, its temperature would have been higher than that of the surroundings until it reached the 735 mbar level. I accept that this situation is not the 'normal' seen in cases of castellanus cloud development, falling wet-bulb potential temperature with height generally required for widespread castellanus cloud development, as Bernard has stated. Crucially, it was the decaying cumulus which prompted me to name the continued development of the stratocumulus 'castellanus'. Regretfully, I feel this has led to an argument over semantics.

I have been careful to stress that assumptions were made in my analysis, although its interpretation will remain somewhat subjective. I am happy to leave each reader to decide whether my own analysis is correct on the given evidence, although I maintain that my description fits well with the observations I made at the time and during the next day, when I returned to work and could attempt to discover the reason for the unusual cloud formation I had observed and photographed.

The Coriolis force and the veering of the sea-breeze

Following the current debate on veering winds and sailing (*Weather*, **51**, pp. 115–116, pp. 320–322 and **52**, p. 100), I have been surprised that nobody has made any reference to the importance of the Coriolis force as the mechanism for the veering of the sea-breeze from an onshore direction to almost parallel to the coast. Simpson (1985) lists the Coriolis force as one of the six factors that affect the sea-breeze circulation, in particular determining its horizontal dimension and producing the clockwise rotation with time. Although this consequence of the Coriolis force is well known among forecasters, it is hardly mentioned in any standard textbook on meteorology. According to an interesting article by Neumann (1984), the effect of the earth's rotation on the sea-breeze was recognised at the end of the nineteenth century, but this understanding was confused by a paper by Jeffreys (1922) who claimed, from a mathematical-scale analysis of the equations of motion, that the Coriolis force has a negligible effect for mesoscale features like the sea-breeze.

In the discussion at the Royal Meteorological Society following Jeffreys' presentation, a Mr Whipple, referring to Aberdeen records, showed that the earth's rotation indeed affects veering of the sea-breeze and that Jeffreys' analysis "obscured this interesting fact". Jeffreys admitted that "a closer approximation, including the rotational terms" might account for Whipple's observations.

Jeffreys does not seem to have realised the time element of the process – what is lost in space is gained in time. As long as the air remains in the sea-breeze circulation, the Coriolis force affects the direction and gradually rotates it (Atkinson 1981, pp. 150, 164). This resembles the mechanism behind the Foucault pendulum where the swinging ball accumulates small deviations over a longer time period. Together with the Foucault pendulum, the sea-breeze circulation is indeed one of the few physical systems where the turning of the earth can be clearly visualised.

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