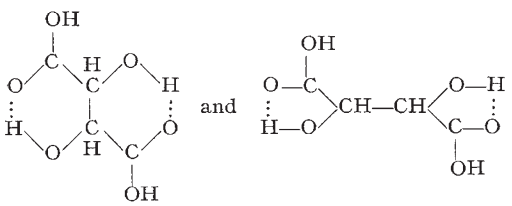
vindication of his claim to have separated the lævo-rotatory component completely from the dextro-rotatory component which is formed from it in solution.

In his original paper, Astbury ascribed the anomalous dispersion of tartaric acid to "strings of molecules, connected together at the hydrogen junctions" of the hydroxyl-groups, which he represents as points of contact between contiguous molecules in the crystals, thus —OH HO . CO—. When it is realised that the X-ray analysis of organic compounds deals only with the orientation and spacing of *molecules*, and gives no information as to the positions of the individual *atoms*, it may be doubted whether there is any experimental foundation whatever for the wholly improbable view that the molecules of tartaric acid are held together *by the mutual attraction of two protons*. The idea that two hydroxyl-groups attract one another is indeed an old one; but since Bragg established the crystal-structure of ice, it has become evident that this action depends on a mutual attraction between the hydrogen of one molecule and the oxygen of another, a view that has recently been developed under the title of "Bivalent Hydrogen" by G. N. Lewis in America, and in Great Britain under the title of "The Co-ordination of Hydrogen" by one of the undersigned. Considerations of this kind have led us to conclude that the anomalous rotatory dispersion of tartaric acid and its esters probably depends on an interaction between the hydroxylic *hydrogen* and the carboxylic *oxygen*, giving rise to internal co-ordination-compounds such as



This conclusion is in harmony with the fact that the anomalous dispersion of tartaric acid persists in its esters, *in spite of the elimination of the carboxylic hydrogens*, which are essential to Astbury's hypothesis; it also agrees with the fact that all the anomalies disappear in the methylene ether (Austin and Carpenter, 1924) in which the hydroxylic hydrogens (but not the carboxylic hydrogens) have been eliminated.

If, as may be gathered from his letter in NATURE of July 26, Astbury is anxious to lay stress on the fact that the forces which produce anomalous rotatory dispersion in solutions of tartaric acid are identical with "the forces which bind the molecules into the crystalline structure," we should be in complete agreement with him; but we should regard these forces as depending on the simultaneous attraction of a proton for two oxygens, rather than of two protons for one another.

T. M. LOWRY.
P. C. AUSTIN.

Spiral and Structureless Nebulæ.

IN "Our Astronomical Column" of NATURE of August 2, reference is made to Prof. Perrine's letter on Prof. Lindemann's spiral nebula hypothesis which appeared in the *Observatory* for July. As Prof. Lindemann's hypothesis was based to some extent on photographic observations of my own, perhaps I may be permitted to make a few comments on the matter generally.

Prof. Perrine's criticisms on the supposed reflection of light from the Galaxy are not new, and are practically covered by my paper on Prof. Lindemann's theory which appeared in the Monthly Notices of the R.A.S. for May last year. The observational facts are entirely against this part of the hypothesis, and I think I am not misrepresenting Prof. Lindemann when I say that he does not attach so much importance to the *source* of reflection as to the probability of reflection being the cause of the colour distribution. There are considerably more grounds for assigning the source of reflection (if any) to the bright central nucleus of the spiral itself. I found, for example, that the curve of light distribution in the nucleus of the Andromeda nebula (N.G.C. 224) was a near approach to the curve $(x+1)^2y = \text{constant}$; and Hubble has recently derived much the same result for a series of bright amorphous ellipsoidal nebulæ of non-galactic type, which have been photographed with the 100-inch reflector at Mt. Wilson. This applies to the amorphous or uncondensed type of spiral nuclei, but it does not apply to the condensed type of spirals where numerous condensations appear and the nucleus is weakened by continued outflow of matter along the spiral arms. In the latter case the peak of the curve is considerably flattened, and the non-luminous matter at the periphery of the nebula is taken up into the condensations.

It may be inferred from this that reflection as a cause of illumination is a possibility in the amorphous type but is definitely ruled out in the condensed type. This has led me to doubt the validity of the photographic observations on which Prof. Lindemann's hypothesis was based. The photographs which Prof. Seares obtained at Mt. Wilson with the 60-inch reflector were comparative—one with an unscreened ordinary plate and the other with a yellow screen and an isochromatic plate. These showed quite definitely for the condensed type a stronger image of the nucleus in the isochromatic than in the ordinary plate compared with the outer regions of the spiral, the inference being that outer regions of the spirals are deficient in the longer wave-lengths. The same results were found in my photographs of the uncondensed type which were measured with a photo-micrometer, and were also carried down to the red region of the spectrum with panchromatic plates and suitable screens. Some photographs were also obtained of the nucleus of the Andromeda nebula with ordinary plates and a nickel oxide screen to cut out the visual part of the spectrum and transmit the ultra-violet. It is evident, however, that we are dealing with plates having contrast factors which differ with the wave-length, and the variation found may be owing to a combination between the range of spectrum and the plate employed.

This question has been dealt with at some length by Ross (*Astrophysical Journal*, vol. 52, p. 86), who finds that the variation of γ (the contrast factor) along the spectrum is greatest for panchromatic plates and least for orthochromatic plates. He points out also that the depth of image in the ultra-violet is not so great as in the longer wave-lengths, which penetrate further into the emulsion. At the same time, I should not wish to condemn the observations out of hand as untrustworthy, but rather I would advocate the repetition of them under more rigorous conditions, which is quite possible, and would give us definite information on this important point.

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Low Wood, Harborne,
August 14.