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The 1979 Milne Lecture – Edward Arthur Milne: His part in the Development of Modern Astrophysics

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This is a precious occasion for me. For, here in Oxford, I used to visit Milne frequently during the early and the mid-thirties. I exchanged letters with him regularly for a period of 20 years; and I have always cherished his friendship and his counselling. In view of my long and sustained friendship with Milne, it has seemed to me proper that I devote this Lecture to an assessment of his rôle in laying the foundations of modern theoretical astrophysics. In making this assessment, I shall try my best to be objective; and, if in the process, I dwell, on occasions, on what appears to me his weaknesses and his failures, it is because I admire Milne sufficiently to be a rationalist about him. Besides, it would not seem to me that I shall be serving his memory by giving a partial account or by adopting anything less than the highest standards.

I

Milne entered the arena of astrophysics in 1921. At that time, only the barest beginnings had been made in what have since become the two main pillars of modern astrophysics: the theory of stellar atmospheres and the theory of stellar structure.

In 1920, there was only one extant book which may be considered as treating topics in theoretical astrophysics: and that was Robert Emden's *Gas Kugeln* – or gas spheres – published in 1907. Emden's book gives a surprisingly complete account of gaseous masses in equilibrium under their own gravitation and in which the pressure is proportional to some power of the density. This is the theory of the polytropic gas spheres – a theory which was to play a key rôle in the subsequent investigations of Eddington and of Milne. Emden's book, besides its more well-known parts dealing with polytropic gas spheres, includes a discussion of the physical conditions in the solar atmosphere with an account, in fact, of Karl Schwarzschild's inferences of 1906 that the outer layers of the Sun cannot be in convective equilibrium but rather be in radiative equilibrium. Schwarzschild had drawn his inferences from the finite brightness of the solar limb – a matter to which I shall return presently. Another landmark of this same period is a paper of Arthur Schuster's in 1905 in which a problem in the theory of radiative transfer, relevant to the formation of absorption lines in the solar and in stellar atmospheres, is treated.

The concept of radiative equilibrium was further analysed by Schwarzschild in 1914. And in 1916, Eddington introduced these same concepts in the larger context of the equilibrium of the stars as a whole and had begun the first of his celebrated series of papers dealing with the internal constitution of the stars. Also in 1918, Eddington had formulated his pulsation theory of stellar variability.

Atomic theory was still very much in its infancy; and Saha's papers dealing with the ionization and excitation of atomic species, at the temperatures and pressures to be expected in stellar atmospheres, were yet to appear.

This was the time when Milne entered the arena of astrophysics. Let me say at once that more than the particular advances for which he was responsible, his greater contribution was his attitude and his style. I shall say more about them later.

II

It was fortunate that the problem to which Milne first turned his attention was one that suited his style and his methods admirably. As I shall indicate presently, the results which he derived in these, his first and earliest investigations in astrophysics, have remained essentially unchanged over the years and have provided the basis for certain permanent features of our understanding of the outler layers of the stars. For this reason, I shall consider them in some detail.

The problem which Milne considered was concerned with the interpretation of the variation of the brightness of the Sun across its disc. This is the phenomenon of the darkening of the Sun towards the limb illustrated in Plate I. This variation of the brightness across the solar disc occurs not only in the total brightness (as exhibited in Plate I) but also in the different wavelengths or colours.

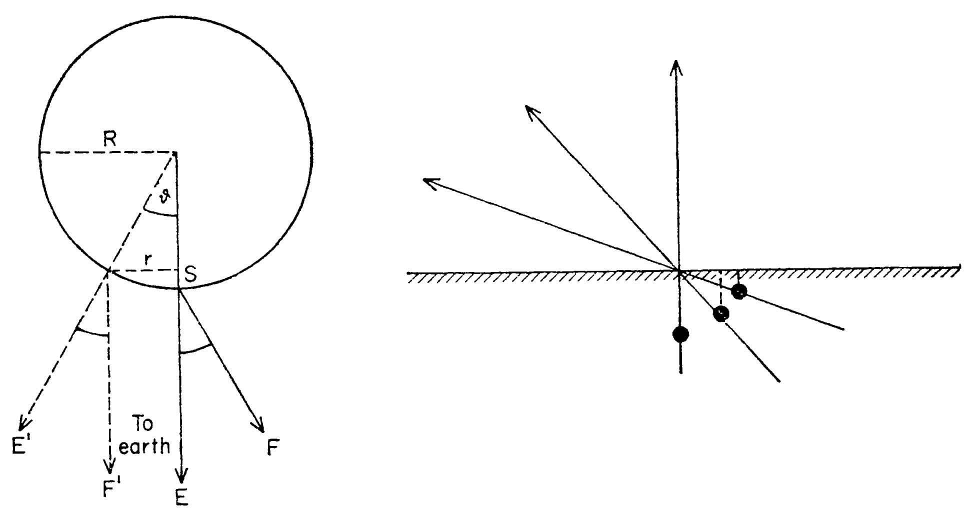

FIG. 1. To illustrate that the darkening results from the angular dependence of the emergent radiation and on the temperature gradient in the atmosphere.

It is clear from Fig. 1, that the darkening towards the limb is simply an expression of the angular dependence of the intensity of the emergent radia-

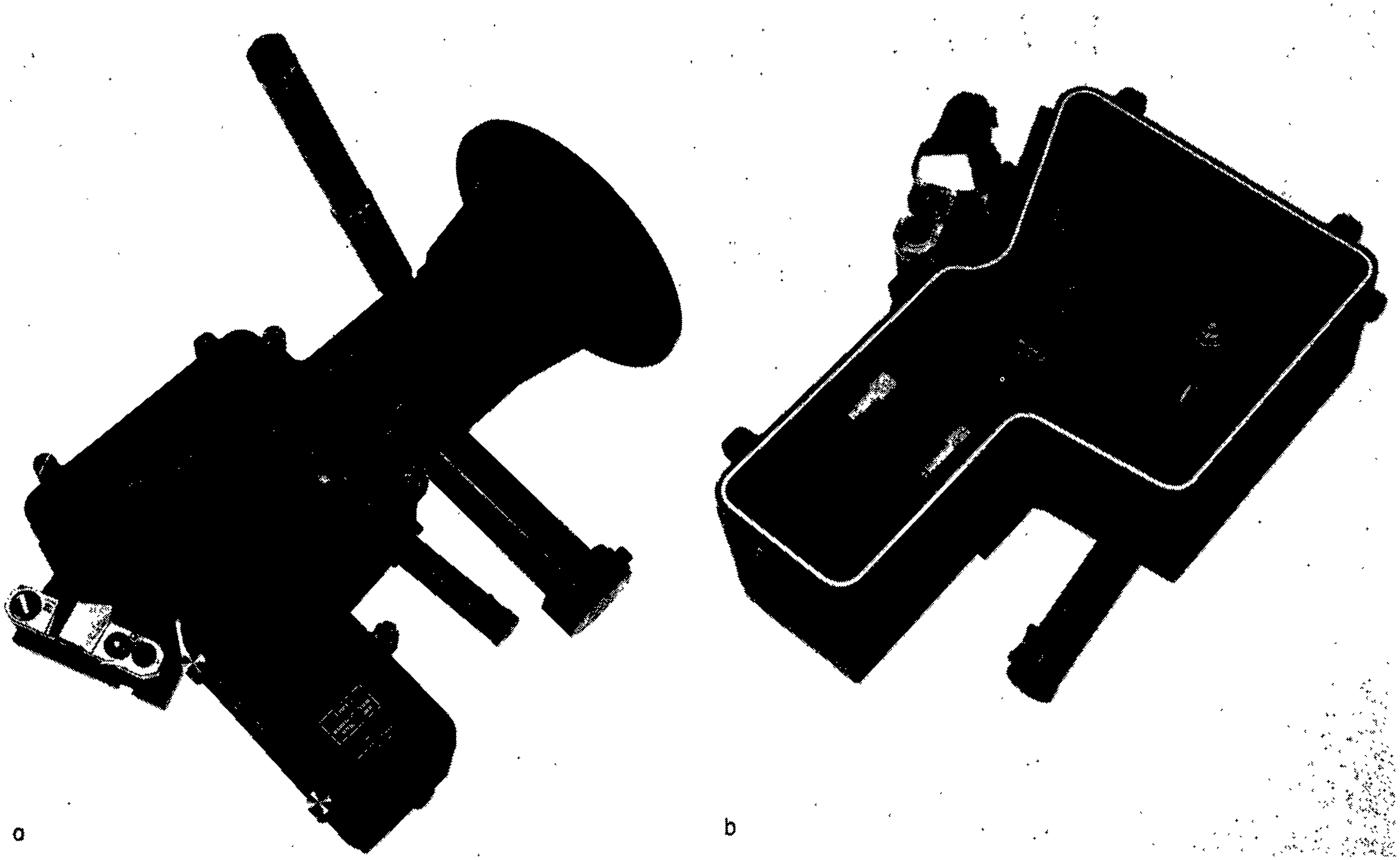

PLATE I. (a) The ULO grating spectrograph. (b) Detail of optical components. The $f/11$ beam is turned 90° by a small flat mirror near the grating; this makes the instrument very compact.

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tion. This problem of the darkening of the Sun towards the limb had been considered by Karl Schwarzschild in 1906; and he had related it to the prevalance of radiative equilibrium and to the resulting variation of temperature in the outer layers of the Sun. The interpretation of the darkening, on the basis of these ideas, is very simple.

The basic fact is that the radiation from all depths contributes to the emergent radiation; only the radiation from the deeper layers is increasingly attenuated by the opacity (i.e. the fogginess) of the overlaying material. On this account, we may say that the radiation which emerges from the surface is characteristic of the radiation prevailing at a certain average depth below the surface. We may, in fact, say that there is a depth to which we effectively see. This depth, measured by the extent to which the overlaying layers attenuate the radiation traversing them, must be the same for all wavelengths and for all angles of emergence. In other words, we effectively see down to an optical depth of unity in all cases. (Radiation traversing material of optical depth unity will be attenuated by a factor of approximately 1/4.)

Since radiation emerging at an angle traverses a path which is slanting through the atmosphere, it is clear that such radiation will be representative of the radiation prevailing at a level not as deep as the level which is representative of the radiation which emerges normally from the surface. Since we should expect the deeper layers to be at higher temperatures, it follows that the radiation emerging at an angle will be characteristic of a temperature lower than the temperature characteristic of the radiation emerging normally. Therefore, the intensity of the radiation emerging at an angle must be less than that emerging normally (see Fig. 1). In other words, there must be a darkening towards the limb.

From the foregoing description, it is clear that the principal problem, which requires solution before we can account for the phenomenon of darkening, is the distribution of the temperature in the outer layers. Once the temperature distribution has been ascertained, the emergent intensity at any given angle can be directly related to the variation of the opacity (i.e. the absorption coefficient or the absorptive power) of the material for light of different wavelengths.

Suppose $\bar{\kappa}$ is some mean absorption coefficient and let τ be the optical depths measured in terms of $\bar{\kappa}$. Let T_τ be the temperature that prevails at depth τ. Then at the depth τ, the spectral distribution of the radiation will be determined by the Planck distribution

$$B_\nu(T_\tau) = \frac{2h\nu^3}{c^2} \frac{1}{\exp(h\nu/kT_\tau) - 1}, \tag{1}$$

where ν, c and h denote the frequency, the velocity of light and Planck's constant, respectively. Accordingly, the intensity of the emergent radiation, at an angle θ to the normal and with a frequency ν, will be given by

$$I_\nu(\theta) = \int_0^\infty d\tau\, B_\nu(T_\tau) \left(\frac{\kappa_\nu}{\bar{\kappa}}\right) \sec\theta \exp\left[-(\kappa_\nu/\bar{\kappa})\,\tau\sec\theta\right], \tag{2}$$

where κ_ν is the absorption coefficient at the particular frequency considered.

It is clear that from a comparison of the observed intensities with those which would follow from equation (2), we can deduce the variation of the absorption coefficient with wavelength (as determined by $\kappa_\nu/\bar{\kappa}$); and this variation will clearly determine something significant about the constitution of the solar atmosphere.

The problem, which I have outlined, was solved by Milne with exceptional thoroughness in his early papers; and he deduced from the solar observations the variation of the solar continuous absorption-coefficient with the wavelength. His results are shown in Fig. 2.

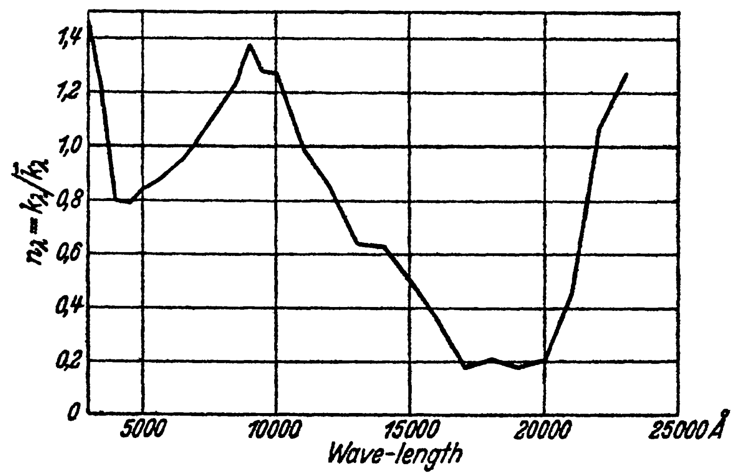

FIG. 2. Milne's deduced variation of the solar continuous absorption coefficient with wavelength.

Milne emphasized two features of the deduced variation: *first*, that the absorption coefficient increases gradually over the entire visual part of the spectrum and attains a very well-defined maximum at about 8000 Å; and *second*, that beyond 8000 Å it decreases to a very deep minimum at about 16 000 Å.

Milne's analysis was repeated by others, in other forms, during the following decades; and they all confirmed his major deductions. I shall consider one such confirmation taken from an investigation of Chalonge & Kowiganoff in 1946 (some 25 years after Milne).

Consider a level at some assigned temperature T and ask for the opacity of the overlaying layers in various wavelengths. Clearly, this question can be answered with the aid of Milne's basic theory. The results of the analysis of Chalonge & Kourganoff are exhibited in Fig. 3. The basic deductions of Milne are clearly confirmed.

The simplicity of the analysis leading to the deduced variation of the continuous absorption coefficient with wavelength signifies, unequivocally, that a fundamental constituent of the solar atmosphere is here involved. As to what this constituent may be finally emerged only during the forties with the definite isolation of what could then be truly described as a new fundamental constituent of the solar atmosphere.

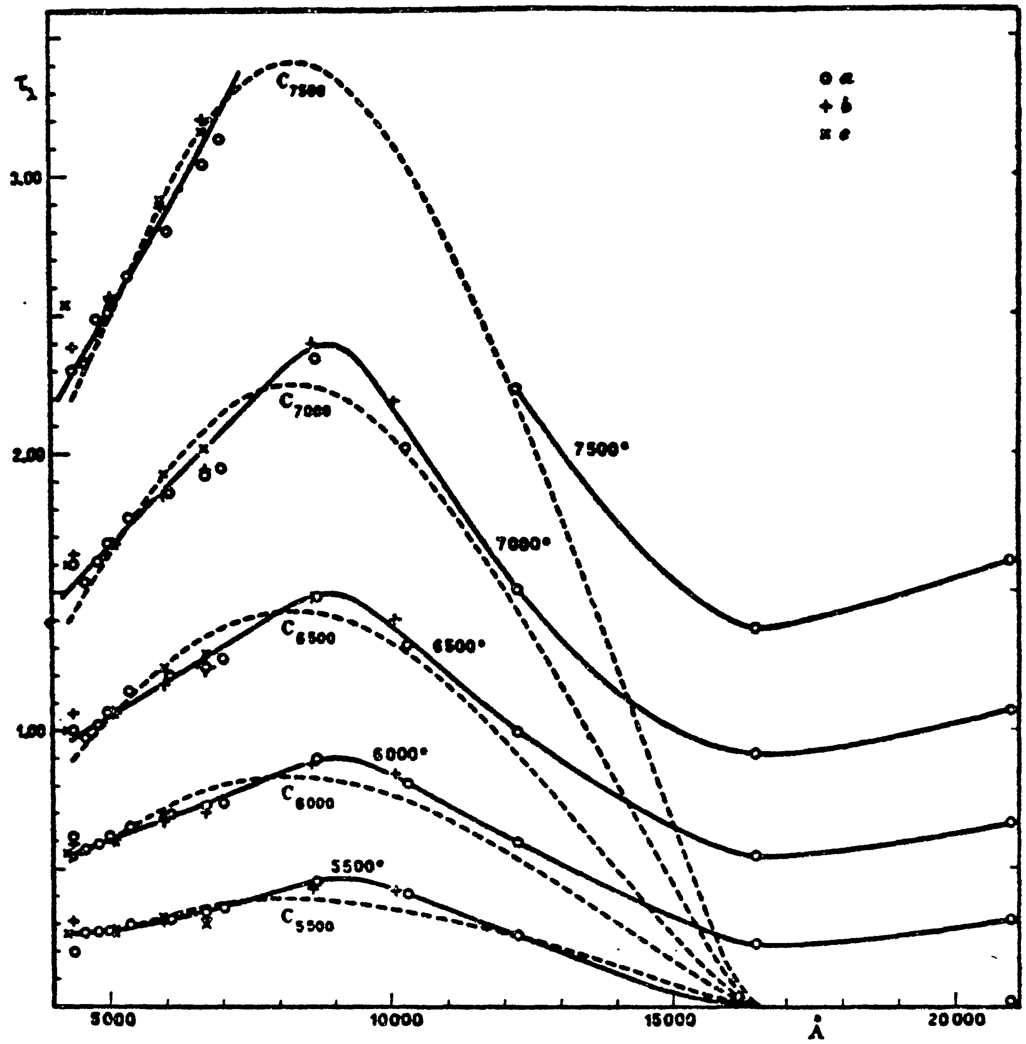

FIG. 3. Optical depth of photospheric layers at different wavelengths (Chalonge & Kourganoff 1946). Dotted lines are H^- absorption; right part of graph explained by free–free transitions.

Let me briefly trace the history of these developments since it represents the fruition of Milne's basic and earliest researches.

By an application of the variational method by which one can set upper bounds to the ground-state energies of atomic systems, Hylleraas and Bethe had, independently, shown in 1930 that a hydrogen atom can stably bind itself to an electron to form a negative ion with a binding energy exceeding 3/4 eV. But it was only in 1938 that Rupert Wildt, returning to the fundamental problem that had been posed by Milne 16 years earlier and which had been side-stepped during the intervening years, pointed out that the negative ions of hydrogen must be present in substantial concentration in the solar atmosphere if hydrogen is indeed as abundant as other evidences had indicated. This was a most fruitful suggestion: it represented a key discovery which made possible all later developments in the theory of stellar atmospheres. But several difficulties had to be overcome before a definitive

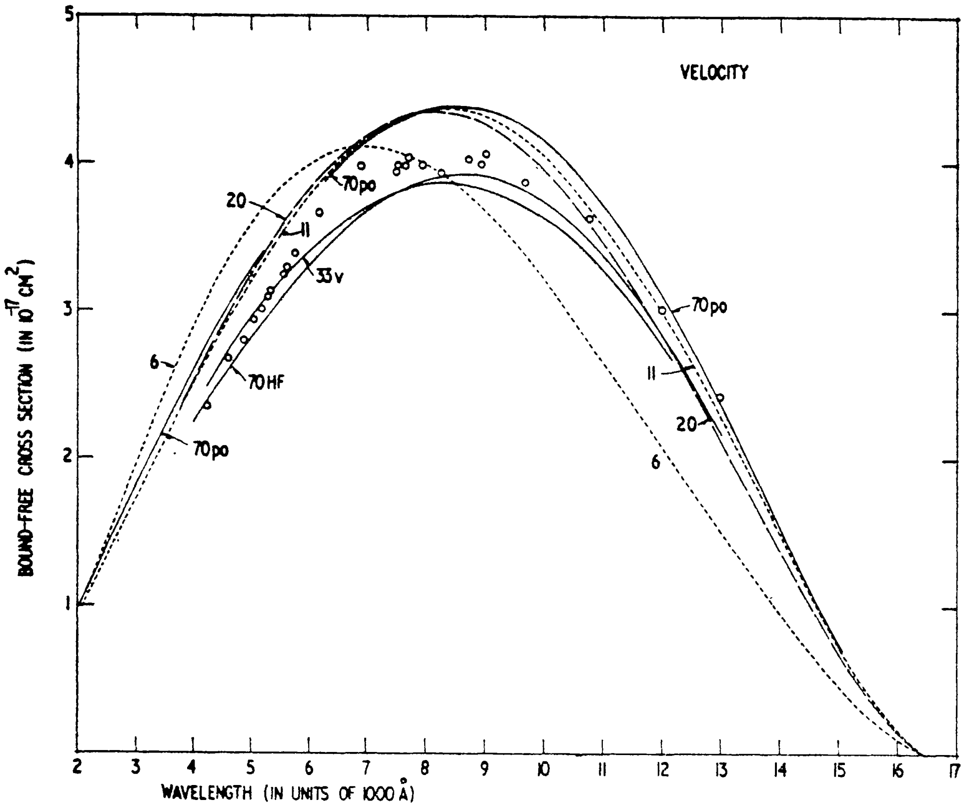

FIG. 4. Photo-detachment cross-section of H^-. Experimental values due to Smith & Burch (1959). Number of variational parameters in bound state wave function is indicated on each curve. Broken lines used if ejected electron is represented by plane wave: 6 (Williamson 1943), 11 (Henrich 1944), 20 (Chandrasekhar 1953). Full lines used if ejected electron is represented by more refined approximation: 70 po, polarized orbital, Bell & Kingston (1967); 70 HF, Hartree–Fock expansion, Doughty *et al.* (1967); 33v, variational, determined by simplified Kohn–Feshbach method, together with Rotenberg–Stein bound function, Ajmera & Chung (1975). All calculations based on the matrix element of the velocity (Chandrasekhar 1945) – from D.R.Bates, 1978, *Other Men's Flowers, Physics Reports*, **35**, 306.

identification of the negative ion of hydrogen, as the source of the continuous absorption in the solar atmosphere could be made.

The principal difficulty was the theoretical determination of the continuous absorption coefficient of the negative ion of hydrogen. This is neither the place nor the occasion to go into the history of the solution of this problem. For those who might be interested, I may refer to a comprehensive account that has recently been published by Sir David Bates in *Physics Reports*. Fig. 4 (taken from Bates' report) suffices to show the years of effort that were required to resolve this problem. It is manifest from this figure that a quantally reliable evaluation of the continuous absorption coefficient of the negative hydrogen-ion reproduces the essential features of the curve deduced by Milne in 1922 (see also Fig. 3). And it should not be overlooked that at the time this identification was made, the negative ion of hydrogen was a theoretically predicted atomic species and the confirmation of the theoretically predicted continuum by laboratory experiments was still 10 years in the future.

And so we come to the end of one major chapter in the history of modern theoretical astrophysics which began with Milne's researches.

III

I now turn to a second chapter. At about the time Milne was working on the problem of the continuous spectrum of the Sun, R.H.Fowler and C.G.Darwin were developing their new approach to statistical mechanics; and Saha's first successful quantitative application of the theory of statistical (or rather, in his case, thermodynamic) equilibrium of stellar reversing layers were appearing in a series of papers in the *Philosophical Magazine* and in the *Proceedings of the Royal Society*.

Saha's theory was based on the following observation. An atmosphere absorbs a different optical spectrum for each stationary stage of ionization, and in fact a different set of lines for each stationary state belonging to each stage; and therefore the relative intensities of the absorption lines of its successive spectra in the spectrum of any star must give some indication of the relative numbers of atoms in the various stages of ionization in the reversing layer and therefore of the temperature and the pressure. Saha's early application of this idea was based on the points of first and last 'marginal' appearances of particular spectral lines. At such points, Saha had argued that the fraction of the atoms in the reversing layer capable of absorbing the line must be very small. And if the corresponding pressure could be estimated the temperature can be deduced.

The precision of these early calculations was questionable owing to the difficulty of formulating conditions for the marginal appearance of particular lines: we do not know how small the 'very small' fraction of atoms must be at marginal appearance; also, the point of marginal appearance will depend on the relative abundance of the element responsible for the line. Fowler & Milne in a series of papers published during 1923–1925 reformulated the basic problem as follows:

> Other things being equal, the intensity of a given absorption line in a stellar spectrum varies always in the same sense as the concentration of the atoms in the reversing layer capable of absorbing the line.

The difficulties with the concept of marginal appearance are avoided in this formulation; and Fowler & Milne first devoted their attention to the place in the stellar sequence at which a given line attains its *maximum intensity*. On the stated premise the maximum intensity will be attained at the maximum concentration of the atoms capable of absorbing the line; and the conditions for this to happen will involve only the temperature and the pressure. In other words, the temperature at which, for a given pressure, a given line attains its maximum is simply deducible from the properties of the equilibrium state. This was the first satisfactory way of applying Saha's theory quantitatively to fix stellar temperatures and pressures. In this manner, Fowler & Milne established a theoretical temperature-scale for the grand sequence of the Harvard spectral types for the first time: a true landmark in theoretical astrophysics.

In subsequent papers, Milne showed that the concepts of mean pressure and mean temperature that are at the base of his studies with Fowler, in turn require refinements in at least two directions. First, we must make precise the meaning we are to attach to a phrase such as 'the intensity of an absorption line'. And *second*, we must allow for the variation of the temperature, the pressure, and the various attendant physical parameters through the layers in which the absorption line is formed. We must, in fact, construct model stellar atmospheres. While Milne formulated some of the basic considerations which must be incorporated in such refinements, he did not pursue them in any great depth or detail. They were left for Pannekoek, Unsold, Minnaert and a host of others to analyse and to complete. The construction of model stellar atmospheres has now become a large industry; but it had its beginnings in the heroic efforts of Saha, Fowler and Milne towards a basic physical understanding.

IV

I now turn to Milne's work bearing on stellar structure.

Already during the years Milne was occupied with problems in the theory of stellar atmospheres, he was turning his attention to problems in the theory of stellar structure. Thus, in a paper published in 1923, he considered the effect of a slow rotation on Eddington's standard model for the stars and on his mass–luminosity relation. This is an altogether exemplary paper in which the mathematics and the physics are scored in counterpoint. (Perhaps, I may be allowed to state here parenthetically that it was this paper of Milne's which stimulated me to develop a complete theory of distorted polytropes some 10 years later.)

However, it was only in 1929 that Milne seriously turned his attention to problems in the theory of stellar structure. But it was begun inauspiciously under the pressure of a bitter controversy with Eddington.

The controversy with Eddington was an unhappy episode which, at least in my judgment, had tragic repercussions on Milne's subsequent work. I shall not say anything more about this episode on this occasion; but it is not possible to avoid overtones of it in any account of Milne's work after 1929.

In 1926, R.H.Fowler had shown in a fundamental paper that the state of matter in the interiors of the white-dwarf stars, such as the comparison of Sirius, cannot be a perfect gas governed by the equation of state, $p = \mathcal{R} \rho T$ (where p denotes the pressure, ρ the density, T the temperature and $\mathcal{R}$ the gas constant); and that it should be governed by the equation of state provided by the then new statistical mechanics of Fermi and Dirac and, indeed, in its limiting form when all the energy levels of the free electrons below a certain threshold are occupied and none above it. In other words, matter must be degenerate, as one says.

Fowler's discussion convincingly demonstrated that Eddington's assumption that the stars are wholly gaseous, with the normal equation of state, cannot be universally valid: the white-dwarfs are examples to the contrary. In the white-dwarfs, the matter is degenerate and the relation between the pressure and the density is, in a good approximation, independent of the temperature. It is therefore legitimate and proper to inquire when and under what circumstances degeneracy can develop in the interior of the stars. But Milne's inquiry was not so directed. He started with the premise – at least, he took it as a foregone conclusion – that all stars *must* have domains of degeneracy and that they *must* belong to one or other of two classes which he called centrally-condensed configurations and collapsed configurations, the distinction between them consisting mainly in the extent of the domains of degeneracy.

In his first detailed paper on the subject, published in 1931 January, Milne developed some powerful analytical tools for constructing composite stellar configurations in which different relations, between pressure and density, obtain in different parts. Besides, Milne stimulated his long-time friend R.H.Fowler to undertake a systematic study of *all* the solutions of Emden's differential equation governing polytropic distributions.

(Parenthetically, may I quote here some remarks of G.H.Hardy's at a meeting of the Royal Astronomical Society in 1931 January. Hardy, as some of you may remember, was the Savillian Professor of Geometry here in Oxford during the twenties. He said (with his tongue in his cheek, as he confessed to me later):

> 'As a mathematician, I don't care two straws what the stars are really like.... But I am particularly interested in Mr Fowler's paper.... His paper is probably the only one of the collection which is of lasting value, for he is certainly right, whereas it is extremely likely that everyone else will be shown to be wrong ...'.

I am afraid that what Hardy prophesied has mostly come to pass.)

To return to Milne's investigations: It was pointed out to him, in fact even before he had communicated his first paper to the Royal Astronomical Society, that the mass of a wholly degenerate star cannot exceed a certain limiting value; and that this fact in turn places an upper limit to the mass that can be contained in the degenerate cores of stars; and finally, that in view of the increasing importance of the radiation pressure in massive stars, sufficiently massive stars cannot possibly develop domains of degeneracy. But Milne would not accept these conclusions. Instead, he wrote:

> 'If the consequences of quantum mechanics contradict very obvious much more immediate considerations, then something must be wrong either with the principles underlying the equation-of-state-derivation or with the aforementioned general principles. Kelvin's gravitational-age-of-the-Sun calculation was perfectly sound; but it contradicted other considerations which had not then been realized. To me it is clear that matter cannot behave as you predict.... Your marshalling of authorities such as Bohr, Pauli, Fowler, Wilson, etc., very impressive as it is, leaves me cold.'

From the vantage point of today, it is clear that Milne's negative attitude prevented him from realizing that the incorporation, positively, of the consequences of Fermi degeneracy, leads one directly to conclude that massive stars, after they have exhausted their sources of energy must collapse to black holes – a conclusion which Eddington drew but which neither Eddington nor Milne would accept. This failure on their part illustrates the danger of perceiving Nature in the images of one's personal beliefs and faiths.

As I said earlier, in the course of his analysis, Milne developed powerful analytical methods for treating composite stellar models. His methods were ideally suited for exploring stellar models with degenerate cores of the kind that stars can have consistently with their allowed upper limit. Milne could easily have carried out such explorations. That he did not was unfortunate both for Milne and for astrophysics.

V

Before I turn to Milne's last and largest phase of his work, namely, kinematic relativity and cosmology, I should like to make a reference – if only a brief one – to a beautiful analysis in stellar kinematics which he published in 1935. In this paper, Milne analysed the differential motions that can occur in a stellar system, in the manner Stokes had analysed hydrodynamic fluid-motion into three parts: a rotation, a sheer and an expansion From the point of view of this analysis, the occurrence of the so-called double-sine wave, in the variation, with the galactic longitude, of the radial velocities of stars with an amplitude proportional to the distance of the stars, becomes self-evident. Milne's analysis provided the base for much dynamical discussion carried out subsequently.

VI

I now come to a phase of Milne's work which he undoubtedly considered as his most important scientific contribution. Thus, referring to his theory of the explanding Universe, he wrote to me in a letter dated 1943 July 6:

> 'I do not know whether I have ever opened my heart to you on that theory. I only know that the texture of the argumentation in it is something utterly and surprisingly different from usual mathematical physics, and that when it comes to be recognized, it will be regarded as revolutionary. It is not usual to crack up one's own work in this way; but it is all very near my heart . . .'.

Perhaps it is not fair that I quote what Milne clearly meant only for me. But to the extent that in my assessment, I am unable to give to his theory the same exalted place, it is necessary that I acknowledge it with equal frankness.

In developing his kinematic theory of relativity, Milne took the strong position that a theory of gravitation can do very well without the general theory of relativity. Indeed, *Gravitation without Relativity* is the title of a contribution which he wrote for a collection of essays that was presented to Einstein in 1949 and included in volume 7 of the *Library of Living Philosophies – Albert Einstein, Philosopher–Scientist* (edited by Paul A.

Schilpp). Einstein's reaction to Milne's contribution, in his concluding essay in the volume, was:

> 'Concerning Milne's ingenious reflections, I can only say that I find their theoretical basis too narrow. From my point of view one cannot arrive, by way of theory, at any at least somewhat reliable results in the field of cosmology, if one makes no use of the principle of general relativity.'

In juxtaposition with this view of Einstein's, let me place Milne's view of the general theory of relativity:

> 'Einstein's law of gravitation is by no means an inevitable consequence of the conceptual basis given by describing phenomena by means of a Riemannian metric. I have never been convinced of its necessity. . . . General relativity is like a garden where flowers and weeds grow together. The useless weeds are cut with the desired flowers and separated later!'

And Milne goes on to say

> 'In our garden we grow only flowers.'

To be complete, may I be allowed to state my own view. General relativity proceeds on the assumption that a theory of gravitation must reduce to all of the Newtonian laws as they operate in the 'small' as, for example, in determining the motions as they obtain in the solar system; and that only a theory constructed, consistently with the other laws of physics (as incorporated in the principle of equivalence) can, with confidence, be extended to the larger context of the Universe. Milne's procedure is exactly the converse of this. He proceeds on the assumption that gravitation can be understood by first constructing a theory of the universe in the large and then descending to phenomena manifested in the small. Apart from the fact that Milne did not succeed in completing his program, it is probable that the program is an inherently impossible one.

Well! There you have three views of varying authority!

Having stated my over-all negative view of this phase of Milne's work, may I say at once that there are some key aspects of his work which are refreshingly original. Thus, Milne's analysis of Lorentz transformations in terms of light-signals exchanged by observers is a model of precision and economy of thought. It deserves much wider knowledge than it enjoys. As Bondi has written:

> 'I feel that not nearly enough has been said about the deep debt of gratitude that we owe to Milne, who in his work on cosmology, introduced the notion of the radar method of measuring distance.'

I now turn to the ideas which Milne contributed to cosmology and which have secured for themselves permanent places in the current literature.

During the late twenties and early thirties, the facts which were perceived as basic for a theory of the Universe as a whole were the following.

1. In a first approximation, the distribution of the extragalactic nebulae is locally homogeneous and isotropic.

And

2. The galaxies are receding from us and from one another with velocities which are proportional to their mutual distances, as codified in Hubble's law.

The discussion of these facts in the framework of the relativistic cosmological models of Friedmann and Lemaitre, as popularized particularly by Eddington, gave one the impression – intended or not – that general relativity is necessary to incorporate them into a coherent theory. But this perception exaggerated the rôle of general relativity. And Milne was certainly correct in pointing out that the facts considered have a simple interpretation which requires no special appeal to any particular theory.

The observed expansion of the Universe and the Hubble relation imply, only, that all the nebulae we now observe must have been, at one time, close together in a small volume of space.

Suppose, then, that at some initial time, t_0, the nebulae were all confined to a small volume (see Fig. 5) and that they all had the same speed V but in random directions. Then, after a sufficient length of time $(t-t_0)$, these same nebulae will have moved outward and will be confined to a relatively thin spherical shell of radius $V(t-t_0)$. Now suppose that, in addition to the nebulae with the velocity V, the same initial small volume had also contained nebulae with half the velocity V. Then, after a lapse of time $(t-t_0)$, these nebulae will be confined to a thin spherical shell of half the radius $\frac{1}{2}V(t-t_0)$. More generally, it is clear that if the original volume had contained nebulae of all velocities, then after a sufficient length of time, the nebulae, with differing velocities, will become segregated; they will, in fact, arrange themselves at distances from the centre which are proportional to their distances in conformity with Hubble's law. Or, as Milne stated: 'the birds of a feather flock together'.

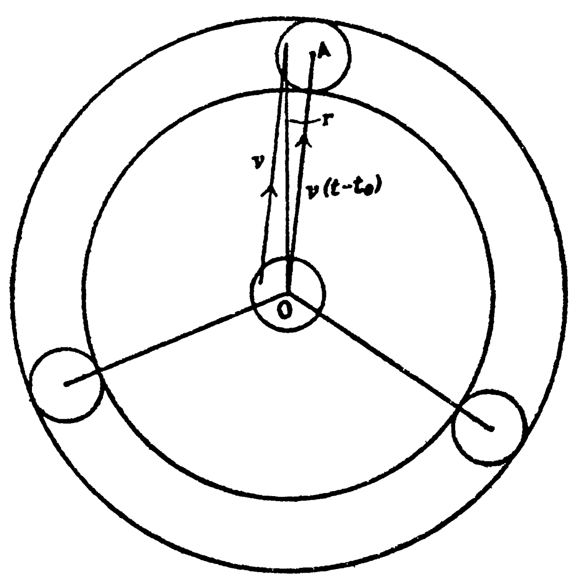

FIG. 5. For illustrating the emergence of a Hubble relation for a system initially confined to a small volume.

This simple model we have described suggests that the observed expansion of the Universe is simply the result of an early beginning of high mean density – a conclusion no one denies at the present time.

Again, as Milne emphasized, a homogeneous swarm of particles receding from a chosen particle in it with velocities proportional to their distances from it, has a remarkable property. As a simple application of the parallelogram of velocities shows (see Fig. 6) the description of the motions will be the same with respect to any other particle in the swarm provided one does not go too near the boundary. In such a system, each particle in the swarm can consider itself as at the centre of the swarm with the other particles receding from it radially with velocities proportional to their distances from it, with the same constant of proportionality. In other words, a Universe which is homogeneous and isotropic and in which the motions satisfy a Hubble relation are related facts which derive their common origin in the requirement that the description of the Universe is the same as viewed from all galaxies. This last requirement was formulated by Milne as a *cosmological principle.* Milne considered this principle as inviolable: it is the centre-piece of his kinematical theory of relativity.

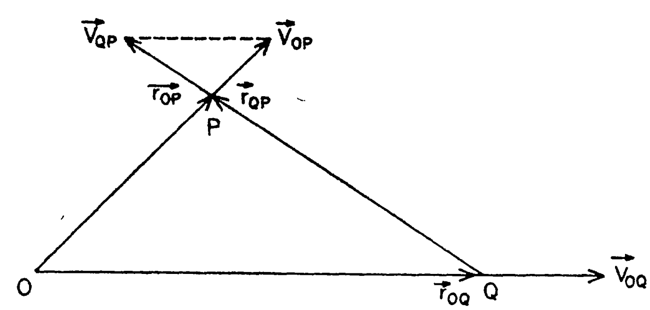

FIG. 6. Illustration of the fact that a cosmological principle is satisfied in a system in which velocity is proportional to distance. Particles P and Q are receding from point O at velocities proportional to their distance from O, but to an observer moving with particle Q, the only apparent motion of particle P is a recession from Q with a velocity proportional to the distance between P and Q.

For reasons I have already stated, I shall not go into any detailed assessment of Milne's kinematical theory of relativity. I shall however, indicate, following Milne's ideas, how the cosmological principle together with Newtonian laws can be used to derive a description of the Universe locally adequate and which is in agreement with the relativistic models of Friedmann.

It is clear that the cosmological principle requires that the world view of any observer, relative to himself, must have spherical symmetry about himself. Then according to a theorem, which is valid equally in the frameworks of the Newtonian theory and the theory of general relativity, a particle at the boundary of a sphere, in a distribution of matter having spherical symmetry about the origin, will be gravitationally acted upon only by the matter interior to the sphere. Consequently, so long as the velocities of expansion are small compared to the velocity of light and the contribution of the pressure to the inertia can be neglected, we can restrict ourselves to the

Newtonian laws of gravitation and to Newtonian concepts in analysing the dynamics of the motions in the system. And we should expect that the results so derived will be valid, also in the wider framework of general relativity, within the stated limitations. Indeed as Milne, and more fully, Milne & McCrea, showed, the equations which follow from the Newtonian analysis are in agreement with those which follow from a relativistic analysis, again, within the stated limitations. But one must go to the general theory of relativity if the limitations imposed, by ignoring the inertial effects of the pressure and by disallowing velocities comparable to that of light, are to be avoided.

I am doubtful if Milne would have approved of my presentation of his ideas and have allowed the concession to general relativity that I have made. Nevertheless, the theory as I have described it, following in main Milne's ideas, is a part of what every student of cosmology now learns.

VII

Let me conclude by describing in general terms what manner of a scientist Milne was.

Milne's special strength was in reducing a complex problem to its elements and analysing each element as to its content and as to its meaning. The crisp and the vigorous style of his writings are manifestations of his keen analytic intellect. He once told me that often his pen could hardly keep pace with the flow of his ideas. Besides, he took delight in solving his analytical problems with grace and elegance. These admirable traits are discernible in all of his writings though in much of his later writings they are shrouded, to a larger or smaller extent, by elements of self-defence and controversy. But when the air was free and his thoughts were untrammelled, his obvious enjoyment, in the flow of his ideas and in the course and texture of his arguments, transports the reader to an equal measure of enjoyment. Nowhere is this transmission of joy more sustained than in his marvellous book on *Vectorial Mechanics*. One can also experience the same delight in some of his papers which were not in the main stream of his scientific concerns; and these gems, in many ways, reveal Milne at his best.

If I have to select a paper of Milne's which illustrates his originality, his style, and, above all, his sheer delight in what he is doing, I should select his paper on 'The Energetics of Non-Steady States, with Applications to Cepheid Variation' published in the *Oxford Quarterly* in 1933. (But even this paper is marred by some unnecessary elements of controversy.) Let me say a few words about this paper.

Milne formulated the ideas contained in this paper during the course of a conversation we were having in my rooms in Cambridge in 1933. Milne was wondering how the phenomenon of Cepheid variability could be grasped in a general theoretical framework without any reference to specific internal parameters such as pressure, temperature, etc. He said that a Cepheid variable is after all a heat engine; and recalling what he had learned from H.F.Newall about Griffith's Heat Engine, he rapidly developed a theoretical framework which led to the functional equation

$$\kappa\phi(t)+\phi(t+b+\phi(t)) = 0, \tag{3}$$

for the time derivative of the relative amplitude of the light variation. (In equation (3), κ and b are certain constants.)

Equation (3) has many remarkable properties. Thus, if ϕ takes the value zero at some instant of time, then it must take the value zero at an infinite succession of instants at intervals of time b apart. Further, if κ were unity, the solutions are periodic with a period $2b$; and a host of other intriguing properties.

At a later time, when Milne gave an account of this work at a meeting of the Royal Astronomical Society he stated, with undisguised delight, that since the functional equation (3)

> 'gave periodic solutions reproducing some of the features of Cepheid light-curves . . . it should not be beyond the wit of man to devise an analysis [of Cepheid variability] which led to it!'

In making an overall assessment of Milne, we have to remember that his early years in Cambridge were interrupted by World War I; that he contracted a fell disease in 1923 which was eventually to prove fatal in 1950; that there were personal tragedies of great magnitude in his life; that his scientific work was interrupted for long years by both World Wars; that during the last several years of his life, he was a sick man; and that, over and above all of these, there was his controversy with Eddington which embittered much of his scientific experience. When we remember all of these and remember also his many solid accomplishments, then we may in truth say, as his long-time friend and colleague Harry Plaskett said

'. . . he died, as he lived, undefeated'.