Arthur Stanley Eddington: pioneer of stellar structure theory

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Abstract

In 1920, Eddington pointed to the fusion of four hydrogen atoms into a helium atom as the likely energy source supplying the observed stellar luminosity. In his monumental *Internal Constitution of the Stars*, he argued that the luminosity could be predicted from the equations of hydrostatic equilibrium and radiative transfer. By means of a draconian approximation, which required the energy ε liberated per gram to be effectively uniform through the star, he produced his 'standard model', with the luminosity strongly dependent on the mass. Application of his theory to the observed Main Sequence confirmed this, but required also that the central temperature increase only moderately with mass, implying that ε is a strong function of temperature. This was subsequently vindicated by thermonuclear theory, but contradicts his approximation. Later work showed that his Mass-Luminosity relation is really a Mass-Luminosity-Radius relation in disguise, but with only a weak dependence on the radius. Radiative transfer effectively fixes the luminosity, and the energy balance condition fixes the radius. To get agreement with the observed luminosity, stellar material must have a substantial hydrogen content.

Eddington's theory does not apply to the high-density, low-luminosity white dwarf stars. He was delighted at Fowler's application of the Pauli Exclusion Principle to show that even at zero temperature, the effectively free gas of degenerate electrons exerts a pressure able to balance the enormous gravitational force. But he never accepted the Stoner-Anderson-Chandrasekhar relativistic extension, with its prediction of a limiting mass, beyond which no cold body can exist in equilibrium. However, his claim that the Fowler equation of state remained valid at all densities failed to carry conviction: 'relativistic degeneracy' is now an essential part of our picture of stellar evolution, ensuring that we can account for the synthesis of the more massive elements, the occurrence of supernovae, the formation of neutron stars, and collapse into a black hole state.

Keywords: *Eddington; Radiative Transfer; Mass-Luminosity Relation; White Dwarfs; Electron Degeneracy.*

1 INTRODUCTION

On 2004 March 12, the Royal Astronomical Society held a meeting to commemorate, sixty years after his death, the outstanding contributions made by Sir Arthur Eddington (Figure 1) to astronomy, general relativity, and cosmology, and to the popularization of science. A summary of the Proceedings was published in the 2004 June issue of the RAS House Journal *Astronomy & Geophysics*. The present paper is a complete version of one of the contributions.
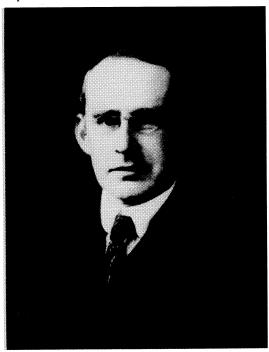

Figure 1: Sir Arthur Eddington (Courtesy: RAS Archives).

2 MAIN SEQUENCE STARS

2.1 Pre-Eddington: Some Landmarks

In the late nineteenth century, William Thomson (Lord Kelvin) and H von Helmholtz suggested that the source of stellar energy is gravitational energy released during contraction.[1] The determining factor was the surface loss, which was not calculated but was taken from observation. Kelvin's estimated cooling time for Earth was of the same order as the K-H contraction time for the Sun—both too short by a factor 100 or so to fit in with geological evidence (Kelvin, 1897).

Kelvin had followed Homer Lane (1869) and A Ritter (1878-89) in picturing stars as being in convective equilibrium. In an *RAS Memoir* titled "On the rotation and mechanical state of the Sun", R A Sampson (1894) wrote: "For a theory of solar rotation, we need a satisfactory theory of the internal temperature distribution." His expressed dissatisfaction with the existing theory of convective equilibrium led him to pioneer the idea of *radiative equilibrium*. And in 1906, Karl Schwarzschild pointed out that the instability leading to convection would occur only if the local temperature gradient is super-adiabatic.

James Jeans (1944) noted that inside a gaseous star, the high internal temperature required for thermal pressure support against self-gravitation must lead to collisional ionization of virtually all the gas. Bremsstrahlung emission and absorption by the electrons moving in ionic Coulomb fields leads to the rapid build-up of a radiation field in local thermodynamic equilibrium. Radiation pressure contributes to equilibrium of the star, and there is a steady leak of radiation down the temperature gradient to the surface.

Polytropic equilibria—self-gravitating gas spheres subject to the phenomenological pressure-density relation $P \propto \rho^{1+1/n}$, with n a constant—were

studied systematically in Robert Emden's monograph *Gaskugeln* (1907).

2.2 Eddington

Already in his 1920 address to the British Association, Eddington (1920) had argued that the "... contraction hypothesis is an unburied corpse...", and that the energy sources maintaining stellar radiation must be subatomic, as urged also by Jeans and others. Following from Aston's demonstration that the helium atom has a mass less than that of four hydrogen atoms, he pointed out that if only 5% of a star's initial mass consists of H, the energy released by H-He fusion will more than suffice, and we need look no further for the source of a star's energy. He ended with the prophetic sentence: "If, indeed, the subatomic energy in the stars is being freely used to maintain their great furnaces, it seems to bring a little nearer to fulfillment our dream of controlling this latent power for the well-being of the human race—or for its suicide." (Ibid:353-355).

Eddington's researches into stellar structure began in 1917 with study of the pulsation theory of Cepheid variables, and continued in 1918 and 1919. He soon saw that it was necessary first to understand the steady state about which the star oscillates. He brought together his studies in his classical treatise *The Internal Constitution of the Stars* (Eddington, 1926), which from now on will simply be referred to as *I.C.S.* Following Schwarzschild, he emphasized that convection requires special conditions to maintain it, whereas radiative transport always occurs in the presence of a temperature gradient. His aim was to show how the luminosity L of a star in radiative equilibrium could be *predicted* from knowledge of its mass and composition. His remarkable insight led him to distinguish between the two related but distinct questions: (a) What fixes the luminosity L? and (b) What is the source of the energy liberated that maintains the star in a quasi-steady state?

Like others before and after him—Henry Norris Russell, Cecilia Payne-Gaposchkin, Robert Atkinson, F Houtermans and J Perrin—he surmised that the energy generation ε per gram would be temperature-sensitive. A star begins by contracting, as in the Kelvin-Helmholtz picture, until the internal temperatures have become high enough for the rate of liberation of sub-atomic energy to be comparable with the radiated surface loss. The star then stops contracting, settling into a state of thermal equilibrium, with energy liberation in each small volume balancing the net efflux of radiation.

Eddington's literary powers are well illustrated by a famous passage (*I.C.S.*:16) in which he imagines a physicist on a cloud-bound planet, computing from first principles the radiation pressure $p_R \propto T^4$ and the gas pressure $p_G \propto \rho T$ inside self-gravitating globes, and finding the ratio p_R/p_G to grow with steadily increasing mass. He then makes the stronger claim that the physicist would find that the two pressures were of comparable order just in the mass range where the stars "happen".

With hindsight, one feels it would have been better if he had written rather that increasing mass implies higher internal temperature, with indeed a corresponding increase in the ratio p_R/p_G, but also in *the spontaneous heat flow down the temperature gradient towards the surface*. In a non-degenerate gas, conductive heat transport is small, but because the mean-free path of a photon is much greater than that of

a particle, radiative transport down the same gradient can be correspondingly greater, even if the radiation energy density is still well below the kinetic energy density. A successful theory must certainly predict luminosities and radii of the observed magnitude in the observed stellar mass range, but it is not obvious that the associated ratio p_R/p_G will always be near to unity: its value will depend on the composition of the gas (cf. below).

The equations for a star in *radiative equilibrium*, with radiative transport carrying to the surface all the subatomic energy liberated, are as follows:

$$\frac{dP}{dr} = -\frac{GM(r)\rho}{r^2} \tag{1}$$

$$P = p_G + p_R, \tag{2}$$

$$p_G = \frac{\mathcal{R}\rho T}{\mu} \equiv \beta P, \quad p_R = \frac{aT^4}{3} \equiv (1-\beta)P \tag{3}$$

$$\frac{L(r)}{4\pi r^2} = -\frac{4acT^3}{3\kappa\rho}\frac{dT}{dr} \tag{4}$$

$$L(r) = 4\pi \int \varepsilon\rho r^2 dr \tag{5}$$

in standard notation.

To simplify the mathematics, for his 'standard model' Eddington made what today looks a hair-raising approximation: he assumed

$$\eta\kappa = k_0 = \text{constant}; \quad \eta \equiv \frac{L(r)/M(r)}{L/M}. \tag{6}$$

This has some convenient consequences. It makes β a constant—the radiation pressure and the gas pressure are constant fractions of the total pressure throughout the star. The luminosity is given by

$$L = \frac{4\pi cGM(1-\beta)}{k_0}. \tag{7}$$

The $P-\rho$ relation has then the $n=3$ polytropic form $P \propto \rho^{4/3}$, which when fed into (1) yields the density distribution already published by Emden. Eddington then inferred his famous quartic for β:

$$(1-\beta) = CM^2\mu^4\beta^4 \tag{8}$$

where $C \propto G^3a/\mathcal{R}^4 = 3\times10^{-3}/M_\odot^2$. Equations (7) and (8) then yield the 'Mass-Luminosity relation'. At the low-mass end, $\beta \approx 1$, so $L \propto M3$; with increasing M the index declines, until for highest masses with radiation pressure dominant, β is small and we reach the limit $L \propto M$.

A fair approximation to the photoelectric opacity was given by the Kramers law

$$\kappa = \kappa_0\rho/T^{7/2}, \tag{9}$$

so that with the polytropic relation inserted, the assumption (6) yields $\eta \propto T^{1/2}$, implying a similarly weak dependence of ε on T. Eddington's standard model is approximated by models which take ε nearly constant through the star (cf. below).

Again with hindsight, it is clear that Eddington could have emphasized that his $M-L$ relation, derived (with some sleight of hand) from the equation of radiative transport, is really a mass-luminosity-*radius*

relation in disguise, but owing to the fortuitous form of the opacity law, the dependence on R is weak. Eddington is thus able to uncouple the equation determining L from that determining R, which is given by the requirement that the central temperature be high enough to liberate sub-atomic energy at the rate required to balance L. He noted that if his models applied to the Main Sequence, with R estimated for each star from the surface temperature T_e, then whereas L increases strongly with increasing M, for the energy liberation to balance L, one requires the central temperature $\propto \mu\beta GM/\Re R$ to increase only mildly with M, implying a *strong* dependence of the energy generation rate ε on T.

This picture was vindicated in the late thirties when the work of Gamow, Bethe and von Weizsäcker did predict that the rate of energy liberation by the fusion of hydrogen into helium would indeed have a strong T-dependence (e.g. Kippenhahn and Weigert, 1990:§18). This was generally interpreted as a triumph for Eddington's theory. However, one should note that this conclusion is at variance with his simplifying assumption (6), which we have seen yields a *weak $\varepsilon(T)$* dependence. In fact in his 1966 *RAS* Presidential Address, that notoriously penetrating critic, Tom Cowling, remarked that with ε nearly independent of T, there is not really a mass-luminosity relation at all, but just one mass which liberates energy at the rate required by the equation of radiative transport. Slightly less strongly, one can say that it is hardly consistent to have ε a weak function of T within a given star, but a strong function when stars of different mass are compared.

The problem was clarified in a paper by Fred Hoyle and Ray Lyttleton (1942). The equations are tackled with simple algebraic formulae for the opacity and for the newly discovered temperature-sensitive ε inserted. Homology relations emerge, showing clearly how the radiative transport equation yields L with a strong M- but a weak R-dependence, while the energy liberation condition effectively fixes R with only a weak modification to the formula for L.

Eddington had in fact studied also an alternative to his standard model, with the energy sources concentrated in a point at the centre. An acceptable version of this (recovered in the Hoyle-Lyttleton paper) is Cowling's (1935) *point-convective model*, in which it is shown from the Schwarzschild stability criterion that an energy source with a T-dependence as strong as the subsequently discovered Bethe-von Weizsäcker C-N cycle yields a model with a central convective core, surrounded by a constant L envelope. The Cowling model is a paradigm for the structure of stars in the upper Main Sequence.

Returning to Eddington's original treatment: there is a chapter in *I.C.S.* devoted to the coefficient of opacity, in which he pointed out that there is a discrepancy of a factor $\simeq$ 23 between the physical value, given by the Kramers theory applied to an Fe gas, and that required to give the observed value of L for given M and R. He proceeded to calibrate his $M - L$ relation by adopting the value $\mu = 2.1$, as was then thought to be spectroscopically acceptable, and then used the star Capella to fix the coefficient κ_0 in (9). He recognized (*I.C.S.*: 244) that the introduction of a fair quantity of H into the stellar gas would resolve the discrepancy, both by lowering the value of μ and by supplying more electrons for the massive ions to capture. But he gave reasons for seeking other ways

out of the dilemma, not least because the μ^4 factor in (8) would "... upset altogether the relation which we have found between the masses of the stars and the critical values of $(1 - \beta)$ ". Like my late friend Martin Schwarzschild, I have always been perplexed by this remark; for as noted above, there is no requirement that Eddington's cloud-bound physicist should find that $p_R/p_G \simeq 1$.

In private, and with some reluctance, Eddington finally accepted Bengt Stromgren's definitive demonstration (1932, 1933) that the opacity discrepancy would disappear if his assumed very low value for the H-content X were changed to $\simeq$ 0.35, yielding $\mu \simeq 1$. (Credit should be given here to Cecilia Payne-Gaposchkin, who in her thesis work on stellar atmospheres had argued for a high H-content, against the opposition of her seniors.) Further, it had been noted by Chandrasekhar and others that the values $\mu \simeq 0.5$, $(1 - X) \ll 1$ would also be consistent with the observed luminosities; and in 1946, Hoyle argued convincingly that this very high X solution was to be preferred on general astrophysical grounds. The same conclusion persists today, but with X replaced by $(X + Y)$ where Y is the primeval He-content, again yielding $p_R/p_G \ll 1$ except for the most massive Main Sequence stars. (The original Cowling point-convective model likewise assumes that p_R may be neglected in (1) and (2).)

The whole basis of Eddington's approach to the problem of stellar structure had been criticized by Jeans (1944) and by E A Milne (1952). Jeans had independently advocated sub-atomic energy sources, but he appears implicitly to have thought that the energy liberation would be by radioactive decay, a process virtually unaffected by the temperature and density of the material. He therefore argued that the luminosity is another independently specifiable quantity, like the mass and composition. Eddington's response was that if a star did have sources liberating energy at a rate greater than could be transported out by radiative transfer, the star would not radiate more, but would gain energy, expand and cool. With the energy arising from thermonuclear reactions rather than spontaneous radioactivity, the star behaves like a household hot water system with a built-in thermostat.

Eddington's reply to Jeans is undoubtedly correct. However, one should note that it does depend on the star's not becoming convective, something that can indeed be confirmed for the essentially homogeneous, Main Sequence models under study (cf. below).

Another essential part of Eddington's approach is that the stellar photosphere responds passively to the energy outflow, adjusting its temperature so that it radiates at the correct rate. Milne's principal contributions to stellar astrophysics were in the study (parallel to Eddington) of stellar atmospheres. In standard notation, L, R and the effective temperature T_e are related by

$$L = \pi ac R^2 T_e^4. \tag{19}$$

Below R, the photon mean-free-path is much less than the macroscopic scale-height, and radiative transfer is well described by the diffusion approximation (4). Above R, the mean-free-path $\rightarrow \infty$, and the integro-differential equation of radiative transfer must be used. In the simplest case of a 'grey' atmosphere, the Milne-Eddington treatment yields the condition on the photospheric optical depth:

$$\tau = \int_R^\infty \kappa\rho\mathrm{d}r \simeq 2/3. \qquad (11)$$

Combined with the local form of the equation of support, we arrive at the physically correct surface boundary conditions

$$T \to T_e, \qquad \rho \to \frac{2\mu GM}{3\kappa\mathcal{R}T_e R^2}. \qquad (12)$$

This is the definition of the photosphere—the condition that the radiation can escape. For an essentially homogeneous, Main Sequence stellar model, constructed according to Eddington's prescription, the 'mathematical' boundary conditions $(\rho, T) \to 0$ are adequate, yielding a radius little different from that given by (12).

Jeans's other criticisms of Eddington's work emerged from his studies of stellar stability—again a problem pioneered by Eddington, with an eye on the Cepheid variable stars. Jeans's approximate treatment suggested that all gaseous Main Sequence stars would be vibrationally unstable even if ε were no more than a weakly increasing function of T. This deeply embarrassing conclusion led Jeans on a wild-goose chase—the study of *liquid* stars, with near incompressibility replacing the normal gaseous equation of state. However, a rigorous treatment of the vibration problem by Cowling (1934) showed that the properly constructed eigen-functions yield a large dissipative contribution in the outer regions, due to radiative conduction, so vitiating Jeans's alarming conclusion.

In the last (1944) edition of his popular book *The Universe Around Us*, presumably strongly influenced by the thermonuclear results, Jeans effectively accepted Eddington's theory—gaseous stars, with both luminosity and radius not arbitrary but determined by the mass and composition. Milne (1952), however, in his posthumously published biography of Jeans, failed to pick this up, and even attempted to defend Jeans's pursuit of the liquid star will-o'-the-wisp (even though the stability problems had long since disappeared), on the spurious grounds that Biermann, Cowling, Unsöld and others had shown that stars are partially convective.

In Eddington's treatment of homogeneous stars, it is the gross structure that fixes both L and R. In current parlance, the bulk and surface solutions are well-matched asymptotic approximations, with the star as a whole being the 'dog' wagging the atmospheric 'tail'. Things are different in the 1955 Hoyle-Schwarzschild giant models, for which the expected physical evolution has led naturally to an inhomogeneous structure with the central regions consisting of a burnt-out core, degenerate in the Pauli-Fermi-Dirac sense, surrounded by an energy-generating shell, and with the rest of the star a still unprocessed envelope through which the energy liberated in the centre has to be propagated, to be radiated from the photosphere. Eddington's prescription applies to the central regions: the stellar luminosity L and the radius and temperature of the energy-generating shell are fixed by the prescribed mass in the core, but are hardly affected by the value of the photospheric radius R. The surface condition fixes R and T_e so that the star radiates the luminosity L supplied from below. The surface opacity is due largely to the negative H-ion, which requires that T_e cannot fall too low. As L increases with growth of the burnt-out core, the radius R grows—the star evolves into the giant domain of the H-R Diagram. Further, it is found that the assumption of purely radiative transport through the envelope yields a photospheric density that is too low: to satisfy the surface condition (12), the envelope outside the energy-generating domain must be largely convective. So for these highly evolved stars, the surface layers are not purely passive, but react strongly on the gross structure.

In his controversy with Eddington during the 20's, Milne had argued that for homogeneous Main Sequence stars, there were solutions alternative to Eddington's, with degenerate cores, and with a structure that depends sensitively on the surface conditions. Studies by several authors (Cowling, Chandrasekhar, Russell, Stromgren) failed to support him. It is indeed ironic that Milne's picture does turn out to be pertinent, but to *in*homogeneous stars that have evolved into the giant domain, very different in structure from the homogeneous, non-convective Main Sequence models that he, Eddington and Jeans were studying.

In a homogeneous, contracting pre-Main Sequence star, the surface loss fixes the release of gravitational energy and so also the rate of contraction. Again, the surface opacity fixes a lower limit to T_e. In the early phase discussed by Hayashi (1961), the contracting star has a super-Eddington luminosity, is largely or fully convective, and moves nearly vertically in the H-R Diagram. For stars of about a solar mass or more, this phase ends when the Hayashi stack meets the nearly horizontal Henyey track: the star completes its contraction towards the Main Sequence with most of the mass in radiative equilibrium, described well by Eddington's prescription.

As stated, Eddington's basic hypothesis was that in typical Main Sequence stars, radiative transfer would be the norm. Subsequent work by Unsöld, Biermann, Cowling and others showed that there would be local domains in which the Schwarzschild criterion predicts convective instability, and that usually the estimated highly efficient convective heat transport would keep the temperature gradient very close to the critical (locally adiabatic) value. Biermann (1935) showed that the sub-photospheric solar convection zone, driven by the reduction of the adiabatic exponent γ to near unity by H-ionization, would in fact extend well below the radius at which H and He ionization is complete and γ has returned to the 'monatomic' value 5/3. Biermann's estimate has been confirmed by recent helioseismological measurements.

For stars with $M < M_\odot$, this outer convection zone deepens, and in fact for $M \approx M_\odot/4$, Biermann found that the star has become fully convective below the photosphere. This is confirmed by computations which show that for low masses, the Hayashi track of fully convective pre-Main Sequence stars intersects the Main Sequence (e.g. Iben, 1965). Thus for low-mass, homogeneous, Main Sequence stars, we are back to the pre-Eddington picture! However, as pointed out by Cowling (1938), the luminosity is still not arbitrary, but is again fixed by radiative transport through the stellar atmosphere, summed up in (11) and (12), and the polytropic stellar structure law for the structure of the star. The surface layers act as a bottleneck, limiting the energy loss to the value allowed by the opacity, and the efficient convection adjusts the superadiabatic temperature gradient to carry this. The surface condition yields an $L - M - R$ relation, but now

this does not reduce approximately to an $L - M$ relation, as found by Eddington for an essentially radiative star with Kramers-type opacity: for these very low mass, fully convective stars, to get the $L - M$ and so also the $R - M$ relation, one needs to know the law of nuclear energy generation $\varepsilon(\rho\, T)$, so that $L = L(R)$ may be constructed from (5).

Hermann Bondi has remarked that it is more important to be lucky than clever. Eddington was both. His guess (6), leading to his standard model with constant β and the $n = 3$ polytropic solution, was indeed a cook, and Cowling's (1935) point-convective model is certainly a far better paradigm for the early-type Main Sequence stars. Yet after his laborious numerical work (using only log-tables, not having even a Brunsviga calculator), Cowling found (I suspect to his chagrin) that the density distribution was not so very different from that in Eddington's polytrope. Likewise, Ludwig Biermann (1935) remarked on the robustness of the Mass-Luminosity relation.

I have dwelt mainly on Eddington's contributions to our understanding of the internal constitution of the stars, where—even with some reservations shown up by hindsight—he stands out as the paramount figure. Mention should be made again of his work on stellar atmospheres, where perhaps the honours should be divided evenly with Milne.

Eddington's remarkable ability to simplify the analysis while also retaining the essentials of the physics is shown by his treatment of the integral equation of radiative transfer. In the 'Eddington approximation', he anticipates that J—the mean over a sphere of the direction-dependent intensity of radiation I—should not be very sensitive to the detailed form of I. He computes J from the simplest possible form for I, using it then to construct first and second approximations to I. The resulting estimates, e.g. for limb darkening, agree with a fully accurate treatment to two significant figures.

The pioneering Schuster-Schwarzschild model for the formation of an absorption line introduced a sharp boundary between the continuum-radiating photosphere and a surrounding *reversing layer*, transparent to all frequencies except those within the absorption line. The later, rather more realistic Milne-Eddington model treats the strong line and the weaker continuous absorption as occurring at all levels in the atmosphere. The same Eddington approximation technique (1929) is again successful in its prediction of equivalent widths.

In *I.C.S.* on pages 101-103, Eddington gives a memorable digression on methodology, emphasizing that for physicists, 'proof' is a tool and 'insight' the finished product, whereas for mathematicians, insight is a tool and proof the finished product. He could point to the success of the most important parts of his work on homogeneous stars as a vindication of his philosophy: for example, his intuition that the luminosity would not be very sensitive to the variation of the postulated nuclear sources through the star. However, as seen, extrapolation to the *in*homogeneous models arising naturally through stellar evolution would be foolhardy, and in fact wrong. I think Eddington would have agreed that the correct 'insight' emerges once *provisional* insight has stood the test of proof.

3 WHITE DWARFS
In his 1922 RAS Centenary address, Eddington

referred—again prophetically—to the few white dwarfs then known: "Strange objects, which persist in showing a type of spectrum entirely out of keeping with their luminosity, may ultimately teach us more than a host which radiate according to rule". The mass of the companion to Sirius—'Sirius B'—was well determined from its orbit to be close to $1M_\odot$, but its luminosity is $L_\odot/300$. Its faintness would occasion no surprise if it were a red star, but WS Adams found its spectrum to be that of a white star, not very different from that of Sirius A. The corresponding $T_e \sim 8000$ K combines with its low luminosity to give a radius of 18,800 km; this body with a stellar mass but a planetary radius must have a density of "a ton to the cubic inch", leading most astronomers at that time to add "which is absurd".

Just before the publication of *I.C.S.*, Adams had published his measurements of the Einstein gravitational red-shift—proportional to M/R—on Sirius B. His results appeared both to verify the third of the early predictions of general relativity, and to confirm the high mean density of the star. Contamination by the light of Sirius A makes these measurements particularly difficult, and subsequent observations yielded conflicting results. The first reliable measure of the Einstein shift was on the white dwarf 40 Eridani B (Popper, 1954)—cf. Trimble and Greenstein (1972).

Eddington's theory of Main Sequence stars is clearly inapplicable to a white dwarf—it would yield a luminosity of the same order as the Sun's. "The radiation of the white dwarfs is one of those paradoxes which arise from time to time when imperfect theoretical knowledge is brought to bear on observation" (I.C.S.:171-172). He pointed out what he clearly considered to be the principal difficulty. "I do not see how a star which has got into this compressed state is ever going to get out of it. So far as we know, the close packing of matter is only possible so long as the temperature is great enough to ionize the material. When the star cools down and regains the normal density ordinarily associated with solids, it must expand and do work against gravity. *The star will need energy in order to cool* ... Imagine a body continually losing heat but with insufficient energy to grow cold!".

To Eddington's delight, the basic problem of white dwarf equilibrium was resolved by Ralph Fowler's application (1926) of the Pauli Exclusion Principle. In later parlance: at white dwarf densities, the mean inter-particle distance is less than the Fermi-Thomas 'mean radius' d of an isolated neutral atom, so the atoms suffer 'pressure-ionization': the electrons form a nearly uniform gas with a huge zero-point 'exclusion energy' enforced by the Pauli Principle, and so exerting a pressure $\propto \rho^{5/3}$. Thus *at zero temperature* the star is a 'black dwarf', with the pressure of the 'fully degenerate' electron gas able to balance gravity. In a white dwarf—with a finite temperature—there is a small thermal contribution to the pressure which increases the radius slightly. As the star cools slowly towards the black dwarf state, the gravitational energy released is nearly all absorbed by a corresponding slight increase in the exclusion energy (Mestel, 1952; Mestel and Ruderman, 1967).

Work by Auluck and Mathur (1959), Salpeter (1961) and Hamada and Salpeter (1961) had shown that the basic model is modified slightly through the ions' forming a nearly rigid lattice, yielding a non-

negligible negative Coulomb correction to the zero-point energy, and with its thermal energy being given by the Debye theory of solids. For a review of the link-up between observation and theory, see Trimble and Greenstein (1972).

In 1930, Eddington published a Second Impression to *I.C.S.*, with some updating, but, surprisingly, without referring to Fowler's 1926 paper. However, the seeds of a new controversy were sown in papers by E C Stoner (1930) and W Anderson (1929a, 1929b) and independently by S Chandrasekhar. At densities high enough for the energy at the Fermi surface to be comparable with the electron rest energy, one should surely make use of the special relativistic relation between kinetic energy and momentum. The new, more complicated $p - \rho$ relation goes over from Fowler's $\rho^{5/3}$ form, valid for small masses, to $\rho^{4/3}$ as M approaches a limiting mass M_{Ch}, beyond which there is no cold body in pressure-gravitational equilibrium.

At the famous—not to say notorious—1935 RAS meeting, summarized on pages 125-126 of Wali (1991), at which Chandrasekhar presented the culmination of his years of research on the problem, Eddington explicitly made the correct deduction that a super-critical mass would not be able to cool down but would revert to a modified Kelvin-Helmholtz contraction: "*The star has to go on radiating and radiating and contracting and contracting until, I suppose, it gets to a few km. radius, when gravity becomes strong enough to hold in the radiation, and the star can at last find peace.*" However, he then went on to say: "I felt driven to the conclusion that this is a reductio ad absurdum of the relativistic degeneracy formula. *Various accidents may intervene to save a star, but I want more protection than that. I think there should be a law of nature to prevent a star from behaving in this absurd way!*... The formula is based on a combination of relativity mechanics and nonrelativity quantum theory, *and I do not regard the offspring of such a union as born in lawful wedlock.* I feel satisfied ... that if the theory is made complete the relativity corrections are compensated, so that we come back to the 'ordinary' formula."

The late Sir William McCrea always resisted strongly the widespread judgement that Eddington had acted unethically on that occasion. He was sure that Eddington felt it kinder to let Chandrasekhar deliver his paper without the knowledge that it would be followed by Eddington's onslaught. Be that as it may, there is no doubt that the memory of that day had a devastating effect on Chandrasekhar. He was anxious to get backing from leading physicists if only because astronomers were overawed by Eddington's reputation. Bohr, Rosenfeld, Dirac, Peierls, Pauli and Fowler all supported Chandrasekhar against Eddington, at least in private, though there seems to have been some reluctance to stand up and be counted. Thus in his treatise on statistical mechanics, Fowler remarks in a footnote that Eddington *says* that the relativistic degeneracy formula is wrong, but he does not come out and say that he disagrees with Eddington. Among astronomers, even Chandrasekhar's friend Milne forgot his long-standing controversy with Eddington over conditions in the interior of a star. Without probing into Eddington's arguments, he was naturally happy with conclusions that fitted in with his own ideas: he told Chandrasekhar that because Eddington's work had shown that the limiting mass was incorrect, his own idea that every star had a degenerate core must be

valid. And later, he wrote to Chandrasekhar, apropos of the support of Bohr, Pauli, Fowler *et al.*: "If the consequences of quantum mechanics contradict very obvious, much more immediate, considerations, then something must be wrong either with the principles underlying the equations of state derivation or with the aforementioned general principles" (Wali, 1991: 132).

Returning for a moment to *I.C.S.*; on page 6, in discussing giant stars like Betelgeuse, Eddington emphasizes that these stars have enormous radii because of their having low density rather than high mass. Betelgeuse's mass is probably between 10 and 100 $M_\odot$, its volume 5×10^7 that of the Sun, so its density is less than the Sun's by $\approx 10^{-6}$. He then quotes Einstein (and Laplace) to note that if the density were that of the Sun, then the star would have become what we nowadays call a black hole: light would be unable to escape, the spectrum would be red-shifted out of existence, and "... the mass would produce so much curvature of the space-time metric that space would close up round the star, leaving us outside (*i.e. nowhere*)"—my italics. I think he is here ramming home the point that giant stars have low mean density; there is not a hint that at the time of writing he envisaged collapse into a black hole as something to be taken seriously.

In the following years, there was an ongoing, ding-dong battle between Eddington on the one hand and Chandrasekhar and his supporters on the other: Moller and Chandrasekhar (1935); Eddington (1935a, 1935b, 1935c); Peierls (1936); Eddington (1939); Dirac, Peierls and Pryce (1942), Eddington (1942). There was again open controversy at the 1939 Paris meeting on "Novae and White Dwarfs". On page 263 of the published proceedings (Shaler, 1941), Eddington initially states his objections to the *astronomical* consequences a little less forcibly than at the 1935 RAS meeting: "If the star is symmetrical and not in rotation, it would contract to a diameter of a few kilometers, until according to the theory of relativity, gravitation becomes too great for the radiation to escape." He now interpolates "*This is not a fatal difficulty ...*" (my italics), but then continues: "... but it is nevertheless surprising; and being somewhat shocked by the conclusion, I was led to re-examine the physical theory".

His "re-examination" led him, however, to the most extreme statements:

Page 250: "The Stoner-Anderson modification is fallacious ... [and] a rigorous treatment leads to the original (Fowler) equation of state."

Page 267: "The Stoner-Anderson formula does not exist."

and later: "Observation can decide between rival hypotheses but not between rival conclusions which profess to represent the same hypothesis."

Eddington retained his rejection of relativistic degeneracy to the end. In his posthumously published *Fundamental Theory* (1947:89), he refers to the Stoner-Anderson formula as continuing to "... work devastation in astronomy".

It should be stated that Eddington's reluctance to accept collapse into a black hole state was shared by others, most or all of whom I believe did not agree with Eddington's critique of Chandrasekhar's analysis. I recall that Einstein himself thought that "... the black hole solution was a blemish to be removed from the

theory by a better mathematical formulation." (Rees, 2000:110). Lev Landau (1932) independently derived essentially the same formula for the critical mass M_0 as Chandrasekhar had done earlier. But he goes on to say: "For $M > M_0$, there exists in the whole quantum theory no cause preventing the system from collapsing to a point. As in reality such masses exist quietly as stars and do not show any such ridiculous tendencies, we must conclude that all stars heavier than 1.5 $M_\odot$ certainly possess regions in which *the laws of quantum mechanics are violated* ... all stars in great probability possess such pathological regions" (Wali, 1991:122, quoting from Alan Lightman).[2]

In the seminal paper by Fred Hoyle (1946) on nucleogenesis by the 'E-process', Chandrasekhar's relativistic $p(\rho)$ formula is accepted, so that when the energy sources in a super-critical mass star are exhausted, at least in the central regions, the star contracts but cannot cool down. Instead, it reaches $\rho \simeq 10^7$ gm/cm^3 and $T \simeq 6 - 8 \times 10^9$K—the "hotter place" demanded by Eddington himself in a famous riposte—so that nucleogenesis can proceed. However, Hoyle is concerned that the nuclei built up in the hot dense interior should be ejected into the interstellar medium *rapidly*, so that they retain their high atomic numbers. In this paper he appeals to rotational instability to cause a continuous loss of mass, which both distributes the processed matter but also enables the star's mass to fall below the Chandrasekhar limit, so that it can cool. However, I recall that in a later work, doubt was cast on the assumption of inevitable reduction of the mass below M_{Ch}, so that the consequences of approach to the Schwarzschild radius must be faced.

Eddington's opposition was to a large extent part and parcel of his disagreement with the generally-accepted treatment of relativistic quantum theory (Tayler, 1996). As emphasized by Norman Dombey (private communication), some of his criticisms were penetrating: for example, that the customary artifice of imposing periodic boundary conditions led to violation of the uncertainty principle. He said correctly that where spatial dimensions were localised so that Δx is finite, then periodic boundary conditions imply that a wave function can be chosen to be the eigenstate of momentum so that $\Delta p = 0$. The solution to this problem is now well-known (Carruthers and Nieto, 1968) but it was not in 1935. He objected particularly to the generalization of the Lorentz-invariant Dirac equation to a two-body (or many-body) system involving potentials $V(\mathbf{r}_1 - \mathbf{r}_2) = V(|\mathbf{r}_1 - \mathbf{r}_2|)$. Again he was correct in principle since a covariant potential would be a function of $x_1^\mu - x_2^\mu$ and therefore of the time-difference $t_1 - t_2$ as well as of $|\mathbf{r}_1 - \mathbf{r}_2|$. It is indeed very difficult to solve the two-body problem in the Dirac equation. However, I know of no-one who could follow and accept his argument that one should use the expression $E = mc^2 + \mathbf{p}^2/2m$ for the electron kinetic energy, whatever the magnitude of $|\mathbf{p}|$, rather than $(m^2c^4 + c^2\mathbf{p}^2)^{1/2}$, so arriving at the Fowler rather than Stoner-Anderson-Chandrasekhar equation of state.

Let me quote from Ed Salpeter's appraisal (1996), which I think shows appreciation of Eddington's motivation, while emphatically rejecting his conclusions. In 1946, Salpeter was a graduate student, with Peierls as his thesis advisor. Referring to two of Eddington's early papers (1935a, 1935b):

There were two aspects to these papers: (i) they pointed out genuine difficulties that would be faced if one wanted to carry out very rigorous and very accurate calculations, and (ii) an explicit calculation of the equation of state for relativistic electrons as Fermi-Dirac particles which not only gave the wrong result but consisted of sheer nonsense or double-talk or both! An example of (i) was how to treat Dirac electrons under high pressure, when they are not free particles but are confined by a strong gravitational field. Peierls (1936) had solved this problem, although it was not a trivially simple calculation. And I have worried off and on over the last 50 years about (ii). Eddington was a great man and on some level of consciousness he must have known that he had written nonsense—how could he live with himself and how could two respectable journals publish such papers? I have felt that much of the answer stems from the genuine problems in (i) obscuring the treatment in (ii). I consider the juxtaposition of macroscopic and several microscopic complications in one problem a particularly exciting challenge for a theorist.

Some of the questions raised in the two Eddington papers had to do with interactions between particles, directly and through Coulomb forces, i.e. forerunner questions for the combination of plasma physics and quantum mechanics. I have worked on this combination off and on since then, stimulated not only by the negative influence of the two Eddington papers, but also by the positive influence of Chandrasekhar's numerous papers in the 1930s on the equation of state and white dwarf star structure.

And indeed, Ed Salpeter's (1961) systematic discussion of white dwarf microstructure is particularly illuminating, for example in his pointing out that the Fermi-Thomas distance d is the quantum analogue of the Debye shielding length in normal plasma theory (see also Chapter 4 of Evry Schatzman's (1948) book *White Dwarfs*).

It should be clear that there are two aspects to the controversy over relativistic degeneracy. There is the argument about the correct equation of state; and there is the prediction of a limiting mass, beyond which there does not exist a cold body with pressure balancing gravity. What is surprising is that Eddington, who was such a strong advocate of Einstein's theory of gravitation, failed to accept that even with the Fowler equation of state, the Oppenheimer-Volkoff equation, which incorporates the general relativistic non-linearities into the equation of hydrostatic support, inevitably leads to a limiting mass (e.g. Zeldovich and Novikov, 1971:257). Hermann Bondi (1964) found a rigorous upper limit to the surface Newtonian potential GM/R and so to the red-shift on radiation coming from a sphere in hydrostatic equilibrium, whatever the equation of state.

Looking back on the controversy, Chandrasekhar made the following comment (Wali, 1991:142-143):

Eddington failed to see the far-reaching consequences of a straightforward application of relativity. Suppose he had said "Yes, clearly the limiting mass does occur in the Newtonian theory in which it is a point mass. However, general relativity does not permit a point mass. How does general relativity take care of that?" If he had asked this question, he would have realized that the first problem to solve is to study radial oscillations of the star in the framework of general relativity, a problem

solved in Chandrasekhar and Tooper (1964), but which Eddington could have done in the mid-1930s. He would have found that the white dwarf configuration constructed on the Newtonian model became unstable before the limiting mass was reached. He would have found that there is no *reductio ad absurdum*, no stellar buffoonery. *He would have found that stars become unstable before they reached the limit and that a black hole would ensue.* It was entirely within his ability, entirely within the philosophy which underlies his work on the internal constitution of the stars ... He would then have predicted and talked about collapsed stars in a completely and totally relativistic fashion. It had to wait thirty years.

Chandrasekhar's 'relativistic degeneracy' (summed up in his 1939 monograph) is now an essential feature of our picture of stellar evolution, ensuring that stars above the limit contract to states of such high density and temperature that one can account for the synthesis of the more massive elements, the occurrence of supernovae, and the formation of neutron stars, observed as radio and X-ray pulsars. And most of us today are ready to accept the probable existence of black holes, or at least make a "willing suspension of disbelief".

4 CODA
Unlike that between Eddington and Jeans outlined above, the controversy between Eddington and Chandrasekhar, sadly, was resolved not by the conversion but by the death of one participant. Nevertheless, Chandrasekhar's deep regard for Eddington emerges clearly (1983) from his Eddington centenary lectures.

In conclusion, I would remark that it is almost the *raison d'être* of the historian to be wise after the event. Recognition of where Eddington's pioneering work needs emending in no way diminishes one's admiration for a truly towering figure.

5 ACKNOWLEDGEMENTS

6 NOTES
1. The same suggestion had been made earlier in two independent papers (both unpublished), respectively by J Mayer and J Waterston (Professor Virginia Trimble, private communication).
2. I find Landau's (1932) argument puzzling—what bodies of mass $> M_\odot$, having burnt out their nuclear sources, "exist quietly as stars"?

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