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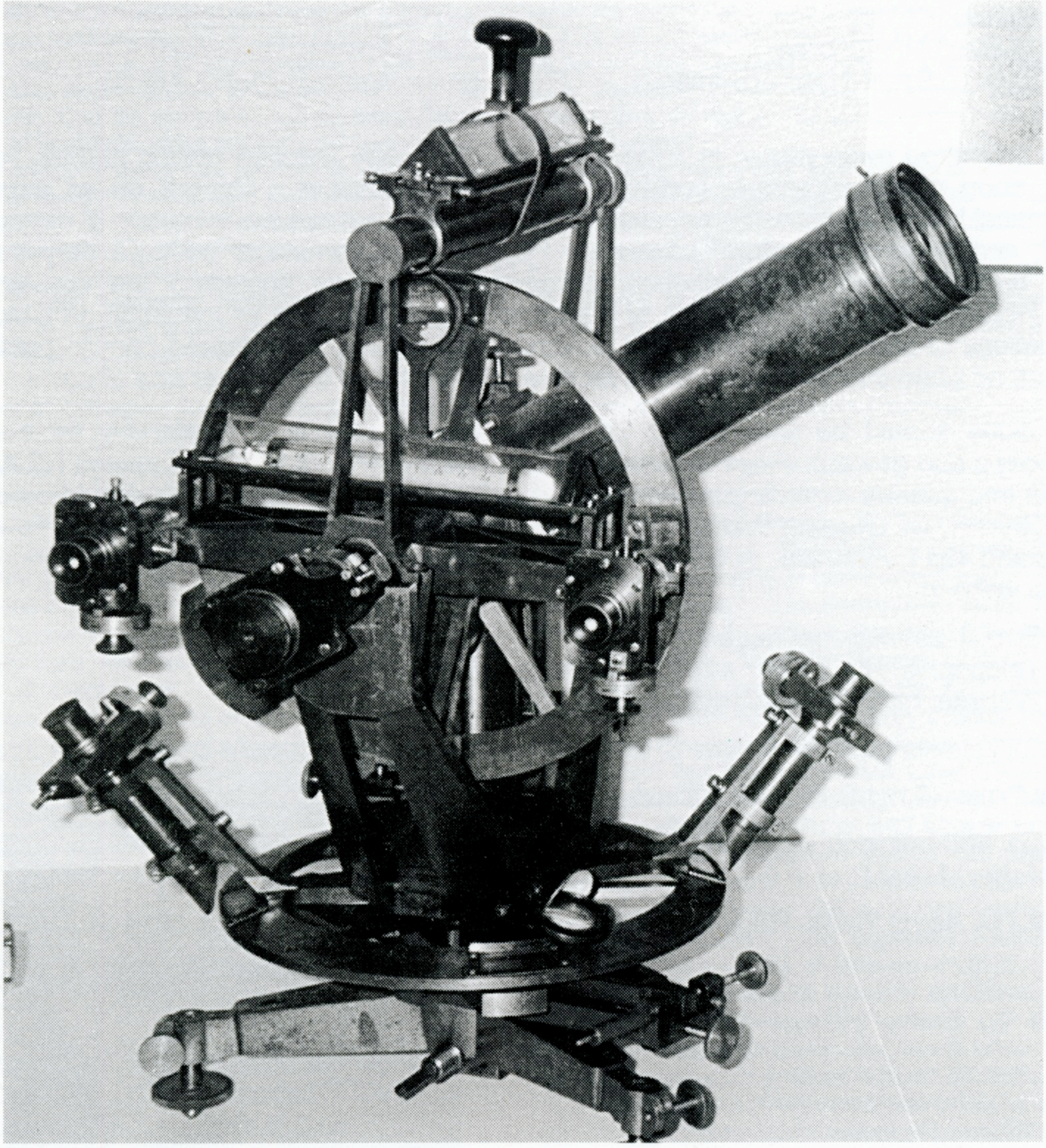

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Cover: A universal instrument for Fridtjof Nansen, 1892. See page98

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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.

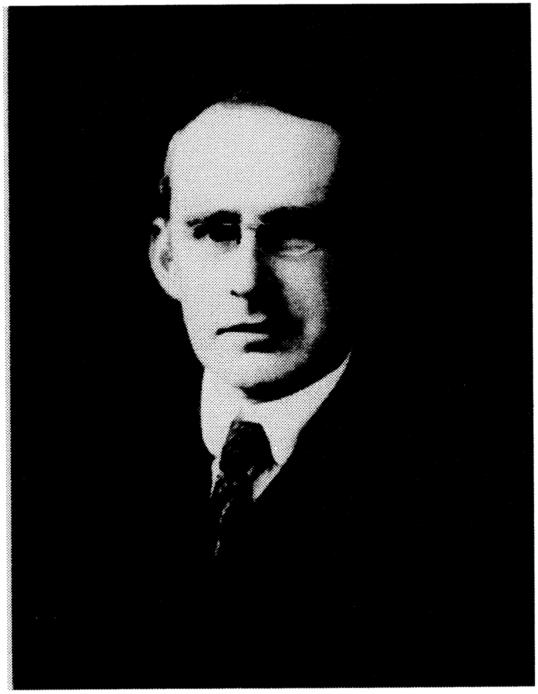

Figure 1: Sir Arthur Eddington (Courtesy: RAS Archives).

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:

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

$$P = p_G + p_R, \qquad (2)$$

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

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

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

in standard notation.

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

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

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

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

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:

$$(1-\beta) = CM^2\mu^4\beta^4 \qquad (8)$$

where $C \propto G^3 a/\mathcal{R}^4 = 3\times 10^{-3}/M_\odot^2$. 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

$$\kappa = \kappa_0 \rho / T^{7/2}, \qquad (9)$$

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 $\mu \simeq 0.5$, $(1 - X) << 1$ 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 $p_R/p_G << 1$ 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

$$L = \pi acR^2T_e^4. \qquad (10)$$

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 → ∞, 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, Strømgren) 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 nonnegligible 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 ≈ 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.

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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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Astrogeodetic study of the orientation of ancient and Byzantine monuments: methodology and first results

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Abstract
This work presents a method for the thorough research of the orientation of monuments, based on state-of-the-art geodetic and astronomical measurements. These measurements permit the production of an astronomically-oriented, digital plan of the monument, a digital diagram of the perceptible horizon around the monument, and a digital reconstruction of the apparent path of the Sun, as it rises above the horizon at characteristic dates. The data reduction procedures are rigorous and lead to an accurate determination of the orientation of the monument. The orientation is then interpreted in terms of other, mostly cultural, information about the scope of the monument and its time of construction. In retrospect, therefore, the method provides an independent determination of the time of construction of the monument within a narrow chronological range. The effectiveness and accuracy of the method is demonstrated by its application to the late Byzantine church of the Assumption of the Virgin Mary in the Greek town of Kalabaka. The orientation of the main axis of the church is geometrically determined to an accuracy of 0.6 arcminutes. Combining all geometric data with cultural and historical information, we determine the time of construction of the church (AD 1000 ± 13 years).

Keywords: *Archaeoastronomy, orientation of monuments, chronology of monuments*

1 INTRODUCTION

Monuments belong to the cultural heritage of humanity. Scientists study them because they give information about human traces on this planet. With regard to buildings, their placement, orientation, shape, material, and the time of decoration, all give us important information about the cultural activities of a civilization (Hoskin, 2001).

In this context, religious monuments (temples) are especially valuable because their construction follows certain rules and traditions and is often connected to calendrical purposes. Religions have contributed to the development of calendars, since the positions of the Sun, the Moon, and the planets have been good time indicators (Theodossiou and Danezis, 1996). The astronomical orientation of the main axis of the temples has usually been a robust tool that has helped priests in their observations and in their efforts to achieve higher accuracy of their calendars.

Archaeoastronomy has delivered insights about the orientation of prehistoric and historical tombs, temples, and other buildings world wide. The orientation of a building towards a certain direction often reflects the way of thinking of a culture about social or religious items. Several celestial bodies were used for the orientation of buildings in many cultures, the one mostly preferred among them being the Sun. According to Malville *et al.* (1998), this rule was already followed around 6,000 BC. Hundreds of Neolithic tombs on the Iberian Peninsula, in southern France, and in the western Mediterranean dating between 7,000 and 2,000 BC clearly show a preferred orientation (see Hoskin, 2001).

As early as 1891, Sir Norman Lockyer measured the orientation of several Egyptian temples and found that some of them were oriented towards the rising Sun on the day of the celebration of the God to which they were dedicated (Papathanassiou, 1994). He found four temples oriented towards the winter solstice and two towards the summer solstice, seven to α CMa (Sirius), twelve to α Col, nine to α Cen, three to α UMa, seven to γ Dra, four to α Car, five to α Aur, and two to α Vir (see Theodossiou and Danezis, 1996). There is also no doubt that Egyptians used their astronomical knowledge for the orientation of their pyramids (Haack, 1984). The Great Pyramid at Ghiza is oriented towards the astronomical North Pole (or, equivalently, to the east) with an accuracy of better than 3 arcminutes (Spence, 2000), which enabled the determination of the year of its foundation with an accuracy of 5 years.

Papathanassiou (1994) remarks that Lockyer and Penrose investigated the orientation of ancient temples in Greece and *Magna Graecia* in 1895. She points that Penrose, in 1897, and Nissen, in 1907, determined the constellations to which many Greek temples were oriented. They also determined the dates of their foundation with low accuracy (up to eight hundred years, as proven later). The tombs of the late Minoan cemetery of Armenoi, near Rethymnon in Crete, are oriented to the arc defined by the local winter solstice, the east and the summer solstice (Papathanassiou *et al.*, 1992). Similarly, the archaic temple at Cardaki and the temple of Artemis, one kilometre to the north-west of the first one on the island of Corfou, are oriented towards the equinox (Papathanassiou, 1994). According to Dinsmoor (1939), the Parthenon of Athens is oriented towards the point where the Sun rises on the day of the celebration of the goddess Athena. It is generally accepted that ancient temples had their main entrances to the east and the statues of the deities were on the west side, looking towards the east. The axis of the temple was often defined in such a way that, on the date of the celebration of the deity, the light of the rising Sun passed through the open entrance and illuminated the statue.

Liritzis (2000) reports that rooms and sections of the palaces of the Minoan and Mycenean civilizations had their entrances preferably oriented towards the east. According to Koeberl (1983), the proper orientation of buildings was common practice in Roman Empire and early Christian times, as reported by Clement of Alexandria and Origenes.

In the Christian world, the main entrance of churches was transferred to the west and the altar to the east (*ex oriente lux*). According to the *Patrologia Graeca* (fourth century AD), churches have to be oriented to the east (Migne, 1863 and Sotiriou, 1978). Sotiriou (1978) also reports additional traditions of the Greek Orthodox Church, according to which churches have to be oriented "to the east".

The Byzantine era was one of the longest in European history lasting roughly 1,100 years. The capital of the empire, Constantinople, has often been associated with luxury and prosperity. For centuries, Byzantine architecture has demonstrated quality of construction combined with elegance, originality, and stylistic creativity. Nevertheless, the orientation of the Byzantine monuments has not been adequately investigated.

In this work, a methodology for the study of the orientation of monuments, especially Byzantine churches, is presented, along with the results of applying this method to the Byzantine church of the Assumption of the Virgin Mary, in the town of Kalabaka. The Meteora monasteries and the area of Kalabaka (Central Greece) have churches and chapels representing the cultural achievements of the last three centuries of the Byzantine State. In these centuries, the political power and influence of Byzantium was declining but, on the other hand, cultural life and scientific activities were prosperous. This is evidenced by the renaissance of the Palaiologos dynasty, which paved the way for the Renaissance in Italy a couple of centuries later.

2 METHODOLOGY

The use of modern, digital geodetic instruments for geodetic and astronomical measurements, combined with rigorous data reduction procedures, leads to the astronomical orientation of ancient or Byzantine monuments with unprecedented accuracy. This is a valuable tool for the extraction of accurate archaeoastronomical information about these monuments.

The method proposed in this work is based on the combination of four different procedures:

1) The geometric determination of the main axis or of any other special direction of the monument, which is achieved by establishing an accurate geodetic network around the monument and creating its plan.

2) The determination of the astronomical azimuth of a base of the network using observations of Polaris and transferring it, by geodetic methods, to the main axis or any other direction of the monument.

3) The geometric determination of the boundary line (silhouette) of the perceptible horizon, as seen from a specific position of the monument, through geodetic measurements.

4) The reconstruction of the apparent diurnal path of the Sun, or any other star, for characteristic dates (e.g. the celebration day of the divinity to whom the temple is dedicated, solstice, equinox etc.) related to the time of construction.

The details of the proposed method can be best understood through the following discussion of its application to the Byzantine Church of the Assumption of the Virgin Mary in the town of Kalabaka.

3 APPLICATION

3.1 The Byzantine church of the Assumption of the Virgin Mary

The church of the Assumption of the Virgin Mary is the cathedral of the town of Kalabaka. This town lies at the border of the Meteora region, by the River Peneios in central Greece. It is the gateway to the Meteora Monastic Community, since the unique paved road to Meteora passes through the town. Kalabaka appears in history in the tenth century under the name 'Stagoi' as the seat of the homonymous diocese.

According to historical sources (Sofianos, 1990, and Sotiriou, 1978), the church was founded between the eighth and the twelfth century and was dedicated to the Assumption of the Virgin Mary. It is well preserved, has two vestibules (internal and external), and in the centre a very interesting and rare pulpit of marble. On the south wall there are frescos dating from the twelfth century, while the monk Neofytos painted frescos on the church hall in 1573.

3.2 Surveying and documenting the monument

The topographic survey of the monument started with the establishment of a trigonometric network in a large area around all the churches and chapels of Meteora and Kalabaka. This network was measured by the Global Positioning System (GPS) with an accuracy of about 2 mm for the horizontal coordinates X, Y, and about 5 mm for the vertical coordinate, H. Table 1 presents the coordinates X, Y, and H of the established network, which appear in Figure 1. The coordinate H is measured in reference to mean sea level.

Table 1. Coordinates of the GPS network.

COORDINATE ADJUSTMENT SUMMARY
NETWORK = Meteora / Datum = WGS-84
Coordinate System = User-Defined Transverse
Mercator ($\lambda_0 = 24°$)

Point	X (m)	Y (m)	H (m)
T0	297533.230	4398135.220	597.210
T1	296731.017	4399771.912	555.335
T2	296892.107	4399264.929	481.846
T3	296275.671	4399395.707	335.436
T4	297290.185	4397972.926	538.334
T5	296487.504	4397938.425	279.015

The establishment of such a large geodetic network is not necessary for every monument but, in this case, it was done because we wanted to place all the churches of Meteora and Kalabaka in the same official reference system.

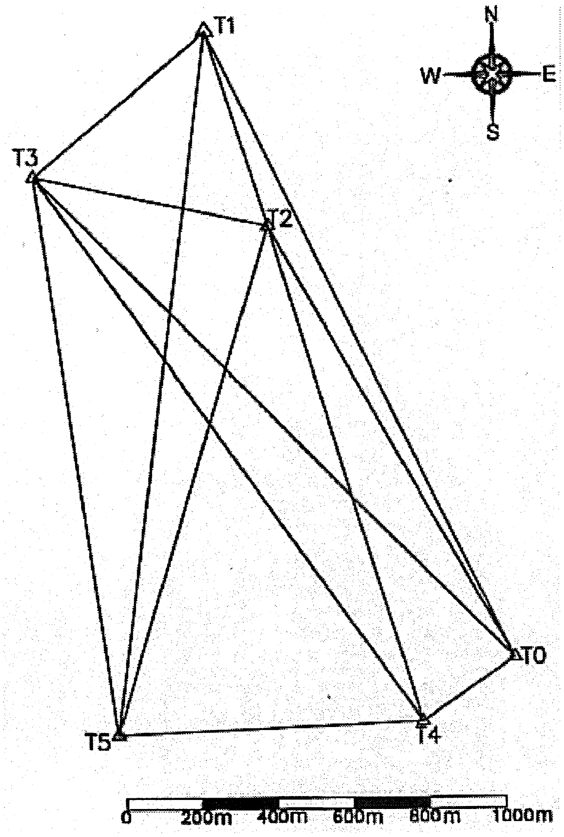


Figure 1. The GPS network.

A polygonometric network of ten points (see Figure 2) was established inside and outside the church, in order to have a view to all surfaces of the monument for the measurement of the detail points. Its elements were measured using the method of the 'three tripods', in order to eliminate the errors of centring and levelling of the instruments.

The measurements of the detail points of the church were made using an integrated Total Station that measures without a retroreflector and has a laser pointer, in order to mark each point accurately. The X and Y coordinates of each point of the monument were determined with an accuracy of ±3 mm. The orthometric heights (H) of the points were also determined with a similar accuracy. The final plan of the church is presented in Figure 3.

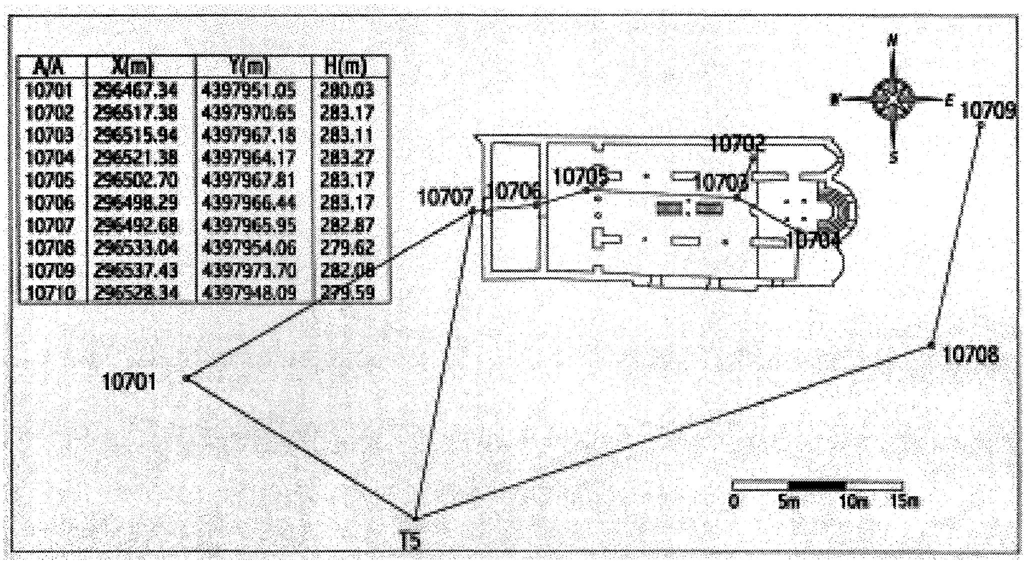
This digital plan is very useful because it permits the accurate extraction of information for all dimensions of the monument, the orientation of important directions and other construction details. The height differences between different rooms in the monument may also be easily determined.

3.3 Determination of the astronomical azimuth

In order to investigate the astronomical orientation of a monument, its plan needs to be astronomically oriented. So the astronomical azimuth of one side of the polygonometric network must be determined. This was done by observations of the Pole star (Polaris, α UMi) with a new system, consisting of the digital total station Leica TDM5000 connected to the GPS receiver Trimble 4000DL, which provides accurate UTC time.

About fifty sightings of Polaris were made in fifteen minutes, and the astronomical azimuth of one side of the polygonometric network was determined with an accuracy of ±0.5 arcsec. This accuracy is much higher than the one achieved by classical methods with poles and compass. The adjustment of the network was done holding the azimuth of this direction constant to the value of the determined astronomical azimuth. Therefore, the plan of the monument was oriented with regard to the astronomical north.

The orientation of the cathedral refers to its main axis, defined by the middle of the main entrance and middle of the altar (a distance of 26 metres). The altar is of special importance in the Orthodox Church, so its position is very well defined. The derived astronomical azimuth of the main axis of the cathedral is 90° 9′.7 ± 0′.6. This axis and its astronomical azimuth were calculated from the plan of the church, with the highest possible accuracy, and there was no need to realize it on site.



A/A	X(m)	Y(m)	H(m)
10701	296467.34	4397951.05	280.03
10702	296517.38	4397970.65	283.17
10703	296515.94	4397967.18	283.11
10704	296521.38	4397964.17	283.27
10705	296502.70	4397967.81	283.17
10706	296498.29	4397966.44	283.17
10707	296492.68	4397965.95	282.87
10708	296533.04	4397954.06	279.62
10709	296537.43	4397973.70	282.08
10710	296528.34	4397948.09	279.59

Figure 2. The polygonometric network of the Church of the Assumption of the Virgin Mary.

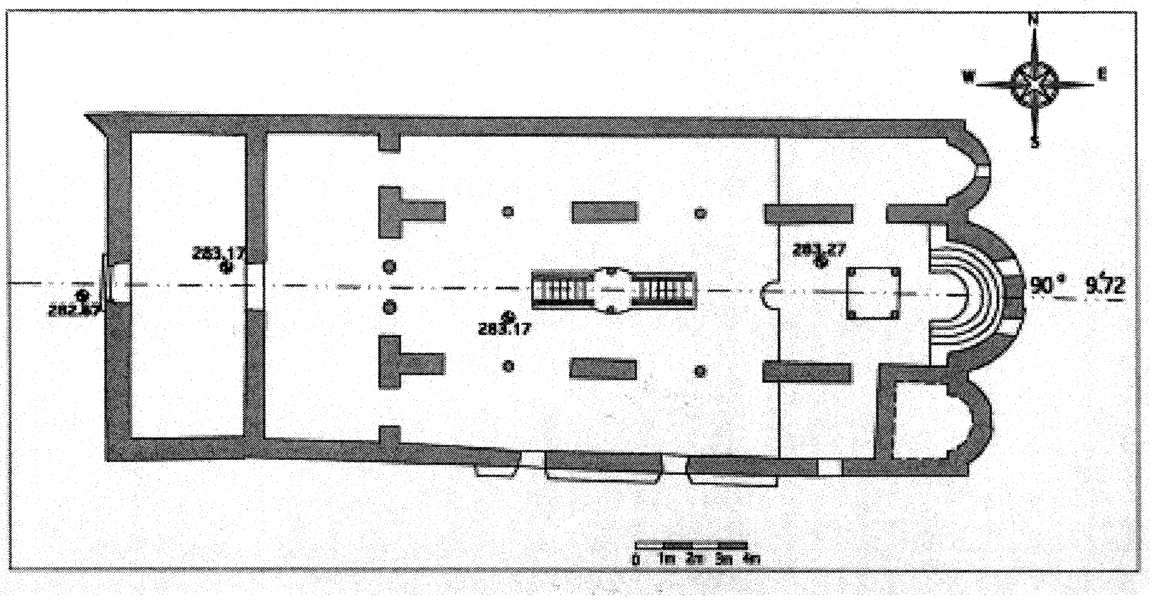


Figure 3. The plan of the Church of the Assumption of the Virgin Mary.

3.4 Determination of the perceptible horizon

A very important piece of information is the diagram of the perceptible horizon in front of the monument. The perceptible horizon is formed by the actual terrain, either natural (mountains, hills, big rocks etc.) or man-made, that is buildings that already existed at the time of construction of the monument. The diagram of the horizon is produced by measuring the azimuth and altitude of a series of points that 'silhouette' the horizon.

The measurements were done from two points of the polygonometric network. The view of the horizon as would be seen from the middle of the altar is very important, since this is the position where the priest mostly stays and performs the holy ceremony. Therefore, all measured points of the perceptible horizon (azimuths and altitudes) were adjusted to conform with the view of the horizon from the middle of the altar. This adjustment was calculated easily, since the coordinates of the points of the horizon, the network points, and the altar are all known from the geodetic network and the digital plan of the cathedral.

The final diagram of the view of the perceptible horizon to the east of the cathedral, as would be seen from the middle of the altar, is presented in Figure 4.

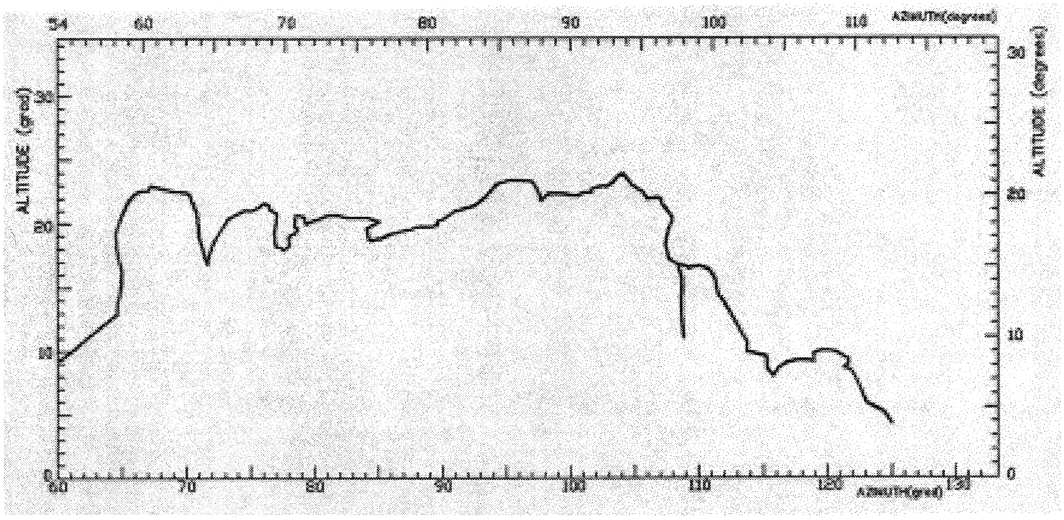


Figure 4. The diagram of the perceptible horizon of the cathedral towards the east.

The x-axis of Figure 4 shows the azimuth and the y-axis the altitude (in degrees and grads) of the horizon. The distance of the perceptible horizon from the cathedral is about nine hundred metres. The diagram of the perceptible horizon was calculated with an accuracy of about 1arcminute, due to pointing uncertainties.

As it is clearly shown in Figure 5, the huge naked rocks of Meteora dominate the eastern visible horizon.

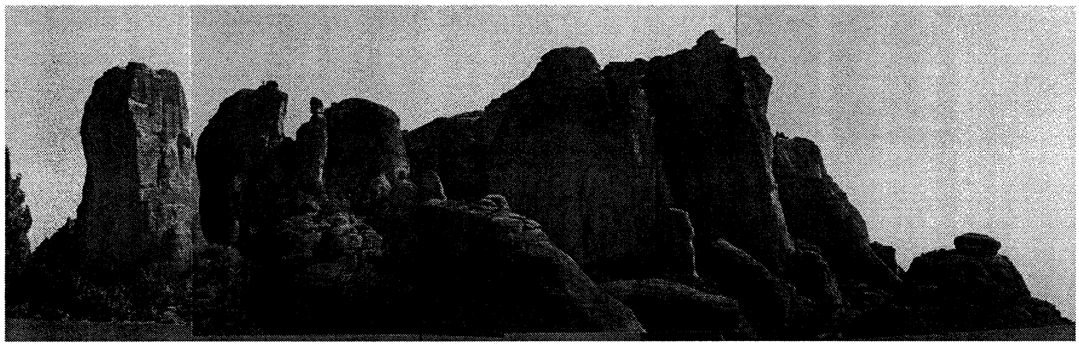


Figure 5. Photographic panorama of the eastern part of the horizon.

3.5 The path of the Sun

The apparent diurnal path of the Sun, as seen from a specific place and for a given date, was determined using available software (digital almanac & virtual planetarium SkyMap Pro 8, Marriot (2001)). Necessary input data are:

- The astronomical coordinates, φ λ, of the monument (determined with satisfactory accuracy from the GPS measurements of the geodetic network).
- The date (any date between 4,713 BC and AD 8,000) and the time interval between successive points of the position of the Sun in the sky.

A local ephemeris of the Sun is then produced, listing altitude and azimuth of the Sun (accurate to about 2 arcseconds; see Meeus, 1991) as a function of local time.

The church of the Assumption of the Virgin Mary in Kalabaka celebrates the Assumption on August 15 each year, so the calculation of the path of the Sun was performed for this date, following the Julian calendar. According to historical data, the cathedral was founded between the eighth and the twelfth century, so the path of the Sun was determined for August 15 in several years during these centuries (see Figure 6).

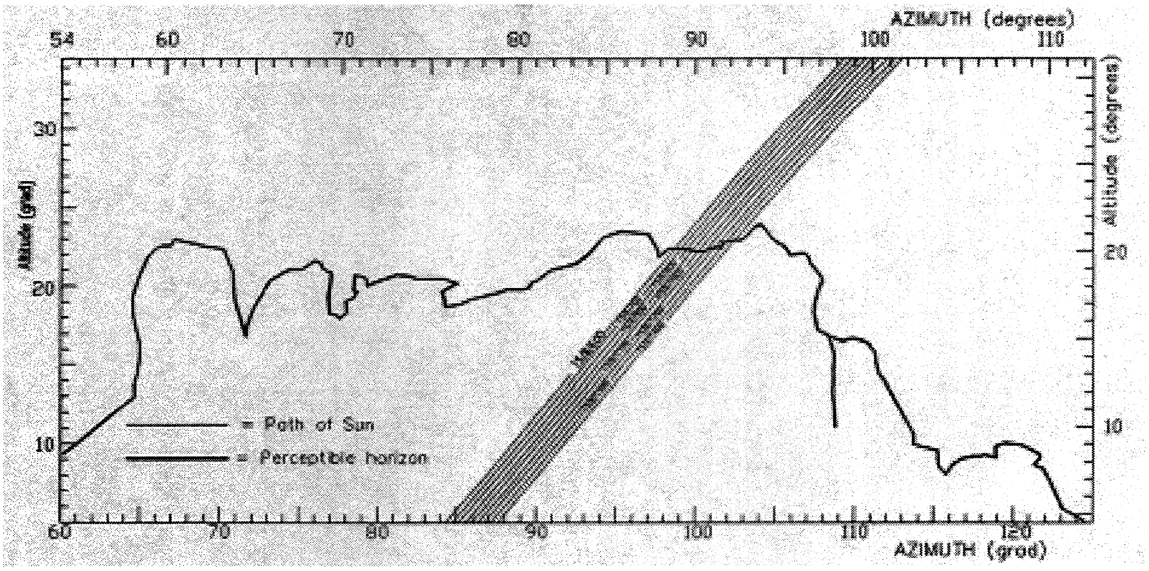


Figure 6. The apparent path of the Sun, drawn for several centuries, superimposed on the visible horizon of the cathedral of the Assumption of the Virgin Mary.

3.6 Determination of the time of foundation of the church

The orientation of a church and its time of foundation are interdependent through the apparent path of the Sun, since sunlight plays a major, symbolic role during the holy ceremony, especially on the celebration day of the church. Therefore, we compared the position of the Sun, as it was rising above the perceptible horizon, with the direction of the axis of the church for several years, always on the celebration day (August 15). This comparison is graphically shown in Figure 7, which is a combination of data from Figures 3, 4, and 6.

In this Figure one can see that the intersection of the perpendicular line representing the azimuth of the main axis of the church and of the line of the perceptible horizon almost coincides with the point of sunrise on AD 1,000 August 15. We conclude, therefore, that the year of the foundation of the church is AD 1,000 ± 13 years.

The error of this determination depends on:

- The rate of change (horizontal drift) of the apparent path of the Sun on the same day each year through the centuries. In this particular case, the drift is about 5.7 arcsec/year.
- The accuracy of the determination of the

orientation of the church. In turn, this depends on the errors of the azimuth of the axis of the church and of the diagram of the perceptible horizon. These errors are shown in the rectangular inset of Figure 7, where the dark area in the middle represents their combined effect. In this particular case, this combined error is about 73.3 arcsec.

The inclined line shows the path of the rising Sun on the date of interest. The circular inset of Figure 7 is an enlargement of the area of intersection and shows the near coincidence of the three lines for AD 1,000 August 15. The probable error in the determination of the time of construction (13 years) is derived by the ratio of the previous errors (73.3arcsec/5.7arcsec per year).

4 CONCLUSION

In the present work, we demonstrated that it is possible to determine the orientation of a monument with high accuracy using geodetic and astronomical methods. By applying the proposed method to the case of the Byzantine church of the Assumption of the Virgin Mary in Kalabaka, we also demonstrate the possibility to date a monument by the proper combination of historical, cultural, and geometric data.

In conclusion, the combination of geodetic and astronomical data, measured using modern digital total stations, allows for the determination of the orientation of a monument with high precision and reliability. Combining the above with historical data referring to the time of construction, the final interpretation of the orientation of the monument may be achieved.

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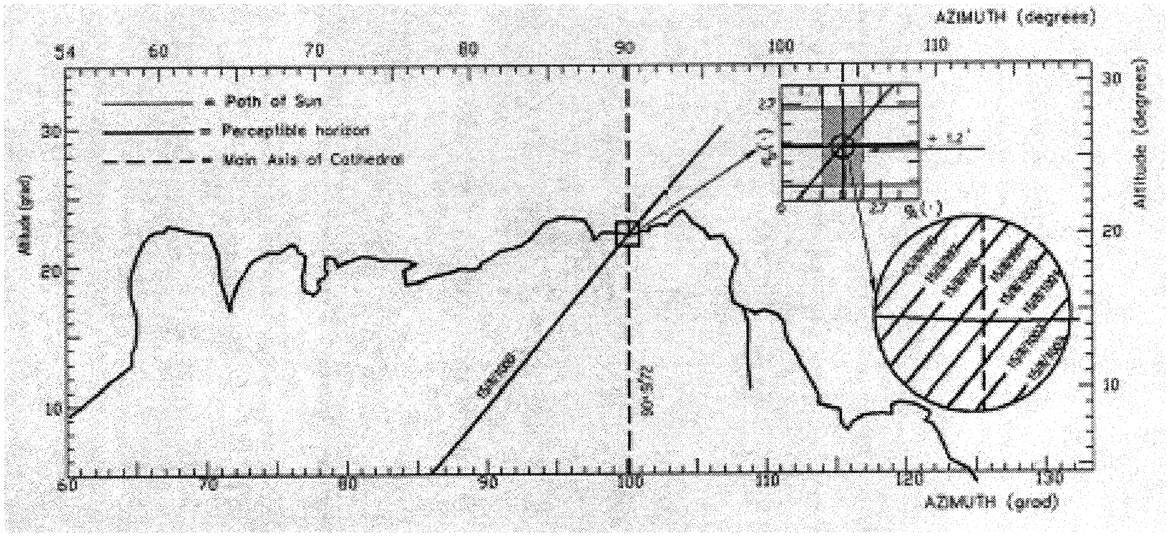

Figure 7. Determination of the time of foundation of the church.

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The publication of the astronomical observations of Buenaventura Suárez SJ (1679–1750) in European scientific journals

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Abstract

Many of the observations of Buenaventura Suárez (1679-1750), a Jesuit astronomer who worked in the missions of Paraguay, were made known in prestigious contemporary scientific European periodicals such as the *Acta Societatis Regiae Scientiarum Upsalensis* and the *Philosophical Transactions of the Royal Society*. Suárez recorded lunar and solar eclipses, and immersions and emersions of the satellites of Jupiter for the purpose of determining the longitude of the mission towns he lived in. He was able to keep abreast of the state of the field and communicate his results through the intermediacy of an epistolary net with correspondents in Europe and the New World.

Keywords: *Buenaventura Suárez, Jesuit astronomy, early Latin American astronomy, Royal Society, Wargentin.*

1 INTRODUCTION

In a recent paper Troche-Boggino (2000) surveyed the life and work of the astronomer Buenaventura Suárez SJ (1679-1750), who was born in Santa Fe (present-day Argentina) and was active in the Jesuit mission towns of historical Paraguay.[1] The main purpose of this paper is to call attention to the little-known publication of Suárez's observations in the *Philosophical Transactions of the Royal Society*. We will also discuss the contemporary diffusion of his work by way of his European correspondents.

2 SUÁREZ'S DATA IN THE *ACTA SOCIETATIS REGIAE SCIENTIARUM UPSALIENSIS*

One of Suárez's achievements was the observation of 147 Jovian satellite eclipses, carried out over a thirteen-year period while he lived in the mission town of San Cosme (in present-day Paraguay). The observations were made with a number of refractors that he built himself. Suárez (1748, Introduction) states that he sent his Jovian eclipse data to the Jesuit astronomer Nicasius Grammatici (1684-1736). When the Swedish astronomer Pehr Wilhelm Wargentin (1717-83) published his second *Mémoire* on the first satellite of Jupiter in 1748 he used observations made at different locations around Earth between 1668 and 1742. In a table, Wargentin included 43 observations made by Suárez between 1720 February 10 and 1726 December 23, and he remarked that Suárez's data were "... not only outstanding, but also beautifully consistent." (Wargentin, 1748:5). Wargentin took care to mention that he got these results from Celsius, who in turn had obtained them in the course of his travels through the Continent. Celsius had also secured some of Grammatici's own observations through the intermediacy of Christfried Kirch, Johann Doppelmayr, and Eustachio Manfredi (ibid.). Although Wargentin does not explicitly says so, it is likely that Suárez's data originally came into Celsius's hands via Grammatici. It has been claimed by Caraman (2001) that Suárez corresponded with Ignaz Kögler SJ in Beijing and Joseph-Nicolas Delisle in St Petersburg, but Suárez (1748) explicitly affirms in his book, *Lunario de un Siglo*, that the data from Kögler and Delisle were communicated to him by Grammatici, along with the latter's own observations from Amberg and Madrid. Suárez used this information to calculate the longitude of San Cosme (321° 45′), based on the meridian passing through the Isla de Hierro in the Canary Islands. Since Delisle arrived in St Petersburg in 1725 and began communicating his Jovian satellite observations in 1726 (Delisle, 1728), it is evident that Suárez's figure for the latitude of San Cosme was obtained during the second half of the 1720s.

3 THE PAPERS IN THE *PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY (OF LONDON)*

In 1725-26, while he was teaching in Ingolstadt, Grammatici published the *Planetolabium Novum*, a two-part work based on the Copernican heliocentric system. In 1726 he also compiled lunar tables that could be used to predict eclipses on the basis of Newton's theory of the Moon (Sommervogel, 1890-1900). The 1750 *Littera Annua* of the Society of Jesus for the province of Paraguay (an annual register of the activities of Jesuits in a given region) attributes to Suárez the translation into Spanish of Sarmento's treatise on the Newtonian theory of tides (cited in Furlong, 1929:139). This work was the *Theorica Verdadeira das Marés* (Lisbon, 1737) by Jacob de Castro Sarmento (1692-1762), a Jewish Portuguese physician who escaped persecution by emigrating to London where he was admitted to the College of Physicians. Afterwards he obtained an MD at Aberdeen, and in 1730 he joined the Royal Society (Stephen, 1967-1968). Castro Sarmento was one of the first to introduce Newton's theories in Portugal, but his *Theorica* was mostly based upon Edmond Halley's 1696 paper in the *Philosophical Transactions* (Carvalho, J. de, 1936; Castro Moreira, 1987; Halley, 1696). It could well be the case that Suárez first became interested in Newton's theory through his early relationship with Grammatici.

While Suárez's Jovian satellite data from 1720-26 were used and published by Wargentin, some of his lunar eclipse, Jovian satellite, and other observations obtained over a longer period were communicated by Castro Sarmento to the Royal Society (of London) and published in their *Philosophical Transactions* in 1748 (and as far as I know, this was first mentioned in Carvalho, R. de, 1955:259). This paper describes a series of observations made from 1706 through 1730, and is in two parts; one deals with lunar and solar eclipses, and the other with eclipses of the Jovian satellites (Castro Sarmento, 1748). The first group of observations report on six lunar eclipses, three solar eclipses, and an eclipse of Jupiter by the Moon, obtained during the twenty-three year period from 1706 November 5 to 1730 January 18. All of these were obtained with a five-feet refractor, except for the lunar eclipse of 1728 February 24 which was seen through a ten-feet refractor. In order to determine the progression of the umbra, Suárez used a pendulum clock (made by himself) which was accurate to a second. The eclipses were seen from different locations, corresponding to Jesuit mission towns in the region. Most of the observations were made from San Ignacio, but there are three observations from San José, San Cosme, and San Miguel Arcángel respectively. In each case Suárez indicated the longitude of the place, as calculated from the meridian of Paris. This first part of Castro Sarmento's paper also refers to a naked-eye observation of the lunar eclipse of 1700 March 4, made when Suárez was a 21 year-old student in the Jesuit College of Corrientes (in present-day Argentina).

The second part of this 1748 paper is much longer, and deals about thirty-four immersions, emersions, and conjunctions of the satellites of Jupiter, all seen from the town of San Ignacio between 1729 January 26 and 1730 May 10 with thirteen- and eighteen- foot telescopes. These observations are arranged in three groups. The first records three observations made between 1729 December 21 and 1730 December 8, each of them compared with the corresponding data obtained by Delisle in St Petersburg.[2] The second group mentions ten immersions, emersions, and conjunctions of Jovian satellites seen between 1729 December 29 and 1730 April 1, to which should be added observations of Saturn's rings and an eclipse of Jupiter by the Moon. The final data-set presents twenty-one observations made by Suárez between 1729 January 26 and 1730 March 27. The paper also gives the longitude of San Ignacio with respect to St Petersburg, Paris, London, and San Cosme.

Castro Sarmento (1749-50) also communicated a second paper to the Royal Society, although in the title it is erroneously attributed to a non-existent "D Suárez, MD" (and this, incidentally, is the reason why it has remained unnoticed until now). This paper describes two lunar eclipses, which were seen on 1747 February 24 from the mission town of San Miguel Arcángel and on 1747 August 19 from Santa María la Mayor. Both were observed with the aid of a ten and a half-foot telescope, and on each occasion Suárez registered the time to within a second when Earth's shadow reached different distinctive features on the Moon's surface. These lunar eclipse observations were far more sophisticated than any of his previous ones, so it is just possible that they were made with one of the two English telescopes that Suárez received in 1745. Counting against this supposition is the fact that neither of these instruments is described in contemporary sources as being ten and a half-foot refractors (see Furlong, 1945:62-68).

4 THE PORTUGUESE CONNECTION

The fact that Castro Sarmento communicated Suárez's data to the *Philosophical Transactions* while Suárez translated Castro Sarmento's short Newtonian treatise into Spanish suggests that at the very least some kind of indirect communication existed between these two men. In the decade of the 1720s, King John V of Portugal summoned two Italian Jesuit astronomers to his court. Giovanni Battista Carbone (1694-1750) and Domenico Capassi (1694-1736) established two observatories in Lisbon, one at the Jesuit College of Santo Antão (St Anthony) and a smaller one at the Royal Palace (Carvalho, Rómulo de, 1985:37-55). Carbone (who became a member of the Royal Society in 1729) was exceedingly active during the said decade: between 1724 and 1730 he published ten papers in the *Philosophical Transactions* on his observations of solar and lunar eclipses and Jovian satellites, and on the determination of the longitude and latitude of Lisbon. He also sent the *Philosophical Transactions* twelve groups of observations by other astronomers, among them Francesco Bianchini (Rome), Eustachio Manfredi (Bologna), Ignaz Kögler and Andrea Pereira (Pekin), and a selection from those made at Ingolstadt in 1726 (Carvalho, R. de, 1955). These were sent directly by him or through the intermediary Isaac de Sequeira Samuda (1696-1730), and later Castro Sarmento. Samuda (who was also a London-based Portuguese Jewish physician and member of the College of Physicians and of the Royal Society). When Samuda died, his role as a link between the Jesuit astronomers in Lisbon and the Royal Society was taken up by Castro Sarmento (Carvalho, R. de, 1955:245 and 254).

Did Suárez become in contact with Castro Sarmento directly or through the Portuguese Jesuits at St Anthony? Suárez's obituary in the 1750 *Littera Annua* for the province of Paraguay mentions that he corresponded with people in Lima, Brazil, Ingolstadt, and London (cited in Furlong, 1945:68), which squares rather well with what we know from other sources. To begin with, Grammatici was in Ingolstadt, while it would seem that Suárez did maintained a correspondence with an astronomer in Lima—a local savant named Diego Peralta, who also made almanacs (Furlong, 1945:58). Suárez's initial contact with Castro Sarmento in London could have been mediated through the Portuguese. The second edition of the *Lunario* was published in Lisbon in 1748, the same year in which his first paper in the *Philosophical Transactions* appeared. Besides, the person who handled the purchase of the British astronomical instruments for Suárez in 1744 was the Portuguese Jesuit Manuel Campos (1681-1758), a Professor of Mathematics and Cosmography at the College of St Anthony (Furlong, 1945:63-64). Campos had spent four years at the Spanish court as cosmographer of Philip V of Spain, and he was a natural mediator between Spaniards and Portuguese (Dinis, 2001).

What about Suárez's correspondence with Brazil? In 1730 two Jesuit astronomers who had been sent by King John V arrived in Rio de Janeiro, charged with calculating the coordinates of Colonia del Sacramento, a city on the northern bank of the River Plate (the possession of which was disputed by Spain and Portugal). One of these was Capassi, and the other was the Portuguese Diogo Soares (1684-1748). Both measured the coordinates of many locations in south-eastern Brazil, and they made several maps of the region. Capassi died in 1736, and Soares continued with the work until he too died in 1748 (Cortesão, 1958). It is conceivable that Suárez came into contact with these 'mathematical fathers' (as they were called), although it is unlikely that there would have been much exchange of information with them, for the interests of the mission towns in Paraguay were quite different from those of the Portuguese crown.

5 CONCLUSIONS

Three conclusions can be derived from this brief account of Suárez's activities. Firstly, his observations were made from the various Jesuit missions scattered throughout the region. Though all the data from the period 1720-26 used by Wargentin came from San Cosme, the papers of the *Philosophical Transactions* show that between 1729-30 Suárez worked in San Ignacio—and there are documents that confirm that he actually was in charge of that town between 1728 and 1730 (see Furlong, 1929:86). Besides, there are isolated observations made from several other missionary towns. Secondly, Suárez was certainly one of the first Jesuits in the River Plate region to take a serious interest in Newton's theory, as suggested by his translation of Castro Sarmento's treatise. This shows that he was not a purely observational astronomer, and that he also had an interest in theoretical matters. Thirdly, Suárez's correspondence with Grammatici allowed him to become acquainted with recent publications in the field as well as to make known his own observations. These were then published by Wargentin in the highly-respectable journal of the Swedish Academy of Science. Suárez may have been in contact with Castro Sarmento, either directly or through the Portuguese Jesuit astronomers working at the observatory of the College of St Anthony in Lisbon. He corresponded with Peralta in Lima and it is possible that he also had some contact with the Jesuit astronomers sent by the King of Portugal to survey the southern part of Brazil.

The Royal Society eagerly sought to publish strategic information about Latin America, since Spanish policy kept a tight grip on any kind of geographical information concerning its overseas territories (Allen, 1947). It is thus only logical that publication of the data obtained by Suárez, which included the co-ordinates of a few mission towns, would find ready acceptance. We should recall that Suárez's *Lunario* went through three European editions during the eighteenth century, besides those published in Latin America (see Troche-Boggino, 2000). The myth of the 'isolated scientist' in the midst of the Paraguayan jungle has been criticized by Glick *et al.* (1975) when applied to the eighteenth-century naturalist Félix de Azara, and neither was the Jesuit Suárez in an absolutely marginal position with respect to the astronomical community of his time. On the contrary, as with most missionary-astronomers in 'exotic' lands, he managed to interact with his European colleagues through a complex network of scientific correspondents (see Harris, 1996).

6 ACKNOWLEDGEMENTS

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7 NOTES

1 Troche-Boggino (2000) gives 1648 as Suárez's year of birth, but his birth certificate indicates that he was born on 1649 July 14 (Furlong, 1929: 81).

2 The Jesuit Collection of the Biblioteca Mayor of the University of Córdoba (Argentina) holds Volumes 1, 2, 5 and 6 of the *Commentarii Academiae Imperialis Scientiarum Petropolitanae*, where Delisle published his papers. These could have been the volumes actually used by Suárez.

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Investigations of the interstellar medium at Washburn Observatory, 1930–58

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Abstract

Between 1930 and 1958, the Washburn Observatory of the University of Wisconsin-Madison was home to pioneering photometric research into the interstellar medium by Joel Stebbins and Albert Whitford. Between 1933 and 1941, Stebbins and Whitford published seminal research on the photometry of stellar reddening, using the Washburn 15-inch refractor and the 60- and 100-inch reflectors at Mount Wilson Observatory.

Many factors were responsible for the Washburn Observatory's pre-eminence in this area. This paper reviews their research on interstellar dust during the years 1922–58, the observational technology and scientific methods that were developed at the Washburn Observatory during that time and the scientific discoveries that originated there. We discuss the factors that enabled Washburn Observatory to become a leader in photometry during the first half of the twentieth century. We also draw on the recollections of past and present Washburn Observatory scientists[1] to understand how Washburn's standing led to a subsequent programme of research into the interstellar medium at the University of Wisconsin-Madison. The resulting portrayal of Washburn Observatory provides insights into the evolution of astronomical research in America, from the beginning of the twentieth century until today.

Keywords: *photometry, reddening, Stebbins, University of Wisconsin, Whitford*

* To whom correspondence should be addressed.

1 INTRODUCTION

Washburn Observatory was established in 1884, serving as the primary astronomical research facility for the University of Wisconsin-Madison (UW) until the construction of the UW-Pine Bluff Observatory in 1958. Joel Stebbins was Washburn Observatory's Director from 1922 to 1948. During this time, Stebbins and faculty members Albert E Whitford (Washburn Director 1948–58) and Charles M Huffer were responsible for greatly advancing astronomical photoelectric photometry, and our understanding of the effect of the interstellar medium (ISM) on the measurement of starlight.

During Stebbins' tenure, Washburn Observatory was unusual among American astronomical facilities. In addition to having smaller than average staff[2], the facility was dedicated solely to photoelectric photometry, and had a long-term programme of research concentrating on two topics: eclipsing variable stars and stellar reddening. These characteristics caused us to ask how Stebbins, Whitford, and Huffer came to lead the field of photoelectric photometry, especially in the face of competition from more generously-funded, staffed, and equipped observatories (e.g. the nearby University of Chicago-Yerkes Observatory). We chose to examine how these early observations of reddening grew into a long-standing research programme at the University of Wisconsin on the nature of the interstellar medium.

No comprehensive history of the Washburn Observatory has been written, and this paper does not attempt to fill that need. Several shorter publications cover the Observatory's early history (Holden, 1881) or are popular accounts written to commemorate anniversaries and other special events (Bless, 1978; Greenstein, 1948; Huffer and Flather, 1959; Osterbrock, 2003a, 2003c; Shane, 1941; Stebbins, 1940, 1958; Whitford, 1953a). In this paper we review the early history of Washburn Observatory, Joel Stebbins' early career, and his rôle in the development of photoelectric photometry. We describe the problem of interstellar reddening and how the research conducted by Stebbins and Whitford contributed to our current understanding. In conclusion, we discuss how Washburn achieved its leadership in photometry, and its place in the evolution of American astronomy.

2 EARLY HISTORY OF WASHBURN OBSERVATORY

During the late nineteenth century, astronomical observatories in the United States were predominantly associated with educational or government institutions like Harvard, University of Michigan, or the U.S. Naval Observatory (Lankford, 1997). Unlike Europe, there were few American astronomical facilities owned and operated by wealthy amateurs (e.g. Percival Lowell's observatory founded in Flagstaff in 1894), and these were often donated to public institutions (e.g. George Ellery Hale's Kenwood Observatory founded in Chicago in 1888). For the University of Wisconsin, an astronomical observatory was considered to be a necessary part of the academic resources of a 'Great University', and instruction in astronomy was considered part of the basic science curriculum taught along with the related fields of mathematics and navigation.

In 1876 the Wisconsin Legislature authorized the allocation of $3,000 for "... astronomical work and instruction in astronomy so soon as a complete and well-equipped observatory shall be given the University on its own grounds without cost to the State ..." (Bless, 1978:1). Rising to this challenge in 1878, former Governor Cadwallader C Washburn used $65,000 of his own money to build and equip the eponymous observatory atop a hill overlooking Lake Mendota (Figure 1). In keeping with the high aspirations of the University, the Observatory was equipped with a world class telescope—the 15.6-inch aperture Clark refractor, the third largest such instrument in America at that time.

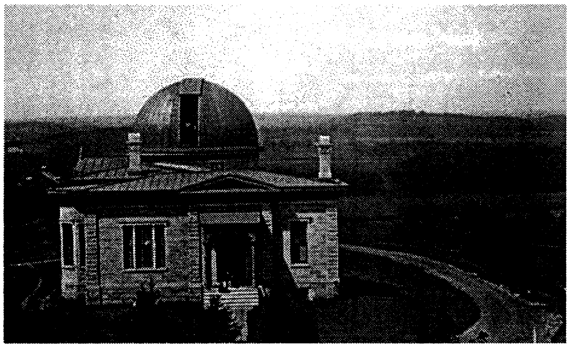

Figure 1. Washburn Observatory *c*. 1880. The open land behind the Observatory was completely urbanized by 1958 (Courtesy: Wisconsin State Historical Society).

The research carried out at Washburn Observatory during this early period was exclusively observational astronomy, with an emphasis on the measurement of the position of stars and the separation of double stars. During the years prior to 1922, Washburn Observatory had three directors: James C Watson from 1878 to1880, Edward S Holden from 1881 to 1885, and George C Comstock from 1887 to 1922. The stature of these men was such that when the newly-formed Lick Observatory was searching for a director, first Watson (who died shortly thereafter) and then Holden were approached. Holden's acceptance in 1885 was the beginning of a long relationship between the Wisconsin and Californian institutions (Osterbrock, 1976; see also Osterbrock, 2003b).

3 JOEL STEBBINS' EARLY CAREER

Joel Stebbins received his undergraduate degree from his home University of Nebraska in 1899, before moving to Madison to study with G C Comstock. Comstock's research had made him one of the pre-eminent astronomical teachers of his day (Whitesell, 2003), a founder of the American Astronomical Society, Wisconsin's first member of the National Academy of Science, and an influential rôle model for the new UW-Astronomy graduate student (Stebbins, 1939). Comstock's regard for Stebbins' ability was so great that he recommended that he complete his degree at Lick Observatory on Mount Hamilton, California, because of its greater research opportunities (Whitford, 1978).

Comstock and Stebbins were to stay in close communication over the coming years, and Stebbins' lasting respect and admiration for his early mentor is evident in his Biographical Memoir of Comstock (Stebbins, 1939). Comstock advised him on his choice of a faculty position at the University of Illinois (Stebbins, 1984; 1957), and his eventual move to Washburn Observatory (Lankford, 1997). By the time Stebbins left Illinois 1922, he had been the recipient of the Rumford Medal, the Draper Medal, had served as Secretary of the American Astronomical Society, was a member of the Organizing Committee of the International Astronomical Union, and had been elected to the National Academy of Sciences. Most importantly, however, he had helped transform photoelectric photometry from an experimental technique into a powerful tool for the measurement of starlight.

4 PHOTOELECTRIC PHOTOMETRY IN THE EARLY TWENTIETH CENTURY

As early as 1897, Washburn Observatory was an active site of photometric research, where G C Comstock was using atmospheric diffraction to pioneer the determination of the effective wavelength of stars (Stebbins, 1939; Weaver, 1946). Comstock employed a wire grating over the aperture of the 15-inch refractor to create diffraction images of a star. He then measured the energy distribution of the resulting spectrum, using a filar micrometer to gauge the separation of the visually-brightest diffraction lines on either side of the primary image, and transformed these measurements into effective visual wavelengths for the star. We can speculate that Comstock's early experience with the photometry of stars, especially his development of this innovative measurement technique, would predispose him toward the talents of his later protégé, Joel Stebbins.

Meanwhile in Europe, the need for a quantitative measurement of starlight had led to the development of the pyrheliometer, bolometer, radiometer, and the first electric photometer by George Minchin in 1892. Minchin's photovoltaic selenium photocell, while able to detect starlight, was not successfully adopted by other European astronomers (Stebbins, 1940). Fifteen years later, Stebbins and F C Brown at the University of Illinois (Hearnshaw, 1996), adapting a selenium photoconductive cell, made the first astronomical photoelectric measurements of the phases of the Moon (Stebbins, 1957).

Photoelectric photometry is distinct from visual or photographic photometry, in that the photon creates a transient electrical effect as it falls upon the detector. The main advantages of the photoelectric cells used by Stebbins were the linear response (number of incident photons directly proportional to the number detected over an extended range) and improved reproducibility compared to other photometric methods of the time. By 1910, Stebbins had used photoelectric photometry to describe the light curve of the variable star Algol, with an accuracy unobtainable by the visual or photographic methods of the day (Stebbins, 1910). This work not only established the existence of Algol's bright eclipsing companion star, but showed the utility of photoelectric photometry for astronomical measurement, and the skill of Stebbins as an observer. Stebbins had enough confidence in the value of his own work that he began promoting photoelectric photometry beyond academic publications (Stebbins, 1914, 1915). At the same time, he continued to improve upon the technology, leading to increases in the sensitivity (signal to noise ratio) of the cells.

When the Swiss physicist Joseph Kunz joined the Illinois faculty in 1911, he brought with him the latest European technology, the Elster and Geitel potassium hydride photoelectric cell. This photoemissive cell had increased sensitivity to light, and more rapid recovery time than the selenium cell, which allowed for more frequent observations and reduced error (Stebbins), 1915). Stebbins was quick to recognize the potential application of the new cell to astronomy. Travelling to Berlin during the 1912-13 academic year, he met with Paul Guthnick and Hans Rosenberg who were beginning their own experiments with the new technology (see Greenstein, 1948; Huffer, 1955). By 1916, Kunz and Stebbins had developed the potassium photoelectric cell to the point where they were publishing accounts of its manufacture, and the application of the 'Kunz cells' to astronomy (Kunz and Stebbins, 1916). Stebbins had even taken his new photometer to the Lick Observatory to study the eclipsing binary β Lyra using the 12-inch refractor (Svec, 1992).[3]

One has only to read the accounts by Stebbins' contemporaries and students (DeVorkin, 1977a; Greenstein, 1948; Kron, 1996; Shane, 191; Whitford, 1978) to realize that he as an astute professional, and a meticulous observer, who could focus his energy and interet over long periods of time to achieve an objective unseen by others. As C D Shane (1941:10) put it on the occasion of Stebbins' receipt of the Bruce Medal in 1941, Stebbins was "...an investigator who, starting with most modest resources in a nearly virgin field, has developed methods, and discovered and extended their application, until now the field deserves to be ranked among the most important in astronomical research."

5 PHOTOELECTIC PHOTOMETRY AT WASHBURN OBSERVATORY

When Joel Stebbins came to the University of Wisconsin in 1922 as Director of the Washburn Observatory, his development of the photoelectric photometric technique using the Illinois 12-inch refractor had improved sensitivity from magnitude 3 to magnitude 6, and he provided measurements with an accuracy in the range of thousandths of a magnitude (Stebbins, 1921). At Wisconsin, Stebbins took the bold step of converting the Washburn 15-inch refractor from a visual instrument to a dedicated photoelectric photometer, making it the first such instrument at a major American observatory, and it would remain the only such instrument for another dozen years (Whitford, 1978).

Continuing with his photometric studies of eclipsing variable stars, Stebbins soon employed C M Huffer as both a photometrist and instructor in the University's newly-formed undergraduate course on astronomy (Taylor, 1877). In 1932, Albert Whitford (then a UW graduate student in physics) developed a single-stage thermionic (Pilotron FP-54 vacuum tube) amplifier that greatly improved the signal-to-noise ratio of the Washburn photoelectric photometer. By enclosing the photoelectric cell and amplifier components in an evacuated casing, Whitford was able to further reduce noise, increasing the sensitivity of the instrument from about magnitude 7.7 to magnitude 9.6 (Whitford, 1932).

Following the example of his own mentor, G C Comstock (Jaell, 1995), Stebbins arranged for a two-year post-doctoral fellowship for Whitord at Mt. Wilson Observatory in 1933. This had the dual effect of piquing the young physicist's interest in observational astronomy, and placing Washburn's photoelectric photometer (complete with photometrist) in the midst of the thriving California astronomical community (Whitford, 1986). Shortly thereafter, Whitford joined Washburn Observatory as a researcher, continuing to improve the efficiency of the photoelectric equipment while gaining experience in astronomical research.

The relationship between these three researchers seems to have been moulded early on. Stebbins, as Director, set the research agenda and managed the facility. Huffer, (who had been a student of Stbbins' prior to a tour of duty at Lick's Chilean observatory) was primarily a photometrist and instructor, responsible for making accurate and systematic measurements for Stebbins' observing programmes, and teaching undergraduate students (DeVorkin, 1977b). Whitford served as the electronic specialist, designing and constantly improving the photoelectric equipment (DeVorkin, 1978).

After the development of the thermionic amplifier, the technological improvements to photoelectric photometry continued. While none would have the same dramatic effect on sensitivity as Whitford's innovation, the Washburn group was quick to explore and exploit new tools. In 1929, the caesium oxide (developed over silver) photocell was introduced. This new material increased the wavelength response range to 3,500-10,000Å (Whitford, 1986), as compared to the Kunz cell's range of 3,500-5,800Å (Stebbins and Huffer, 1934). This permitted photoelectric measurements at red wavelengths, allowing for the development of the six-colour photometry method (see Section 7.3 below), and the measurement of extended objects such as nebulae (Stebbins and Whitford, 1943).

In 1937, Gerald Kron and Albert Whitford used the new RCA photomultiplier tube to create an automatic telescope guider for the Mt. Wilson 60-inch telescope. Kron had completed an M.S. in Mechanical Engineering at UW in 1934, when he obtained a position at Washburn Observatory helping to maintain the photoelectric equipment in Whitford's absence (DeVorkin, 1978). In 1935, Kron accompanied Stebbins on a summer observing session at Mt. Wilson, where he worked with Whitford as a research assistant. As he had for Whitford, Stebbins recommended Kron for a doctoral research programme, this time at Lick Observatory (Jarrell, 1995). Kron successfully received his Ph.D. in 1938, and became a leading photometrist, creating another strong link between the Wisconsin and California astronomers.

6 THE PROBLEM OF INTERSTELLAR REDDENING

In 1930, Lick Observatory's Robert Trumpler noted that, "For more than a century astronomers have interested themselves in the question: Is interstellar space perfectly transparent, or does light suffer an appreciable modification or loss of intensity when passing through the enormous spaces which separate us from the more remote celestial objects?" (Trumpler, 1930b:214). The inability to obtain a factor for the 'extinction' of starlight, that could be applied to measurements of stars at different distances and positions, posed serious problems for the study of astrophysics at the beginning of the twentieth century.

While considerable thought and observation had been expended on this problem, no agreement had been reached into the cause of this reddening (or, colour excess) of starlight. Perhaps most fundamental was the need to accurately measure astronomical distances. Several investigators, including J C

Kapetyn, P J van Rhijn, and H Shapley, had based estimates of the size of our Galaxy on data that proved to be erroneous because it did not account for reddening from interstellar dust (Oort, 1972). Also, multiple-wavelength observations of starlight and stellar spectra used to understand the evolution of stars were uncertain if dust was selectively obscuring some wavelengths and not others.

Trumpler's approach to resolving this problem was to use two methods to estimate the distances of open clusters of stars at differing galactic latitudes. The first method estimated 'photometric distances' from the apparent magnitude and spectral type of the stars in each cluster. For the second method, he reasoned that clusters at similar stages of evolution (i.e., total number of stars and central cluster density) should have the same linear diameter. While the photometric estimate could be affected by absorption of starlight, the dimensional method would not be. Assuming a 1:1 correlation between the two methods (in the absence of absorption), he demonstrated that the correlation that was observed did indeed result from absorption. He also found that open clusters near the plane of the galaxy suffered more from extinction by the ISM, indicating that the obscuring material was more concentrated there (Trumpler, 1930a, 1930b, 1930c).

Trumpler's proposal that interstellar dust caused reddening of the light from stars along the plane of our Galaxy was a turning point for research at Washburn Observatory. In 1930, Stebbins was visiting Lick Observatory, where Trumpler suggested that the observed reddening effect could be measured more precisely with the Washburn photoelectric photometer (Jarrell, 1995; Stebbins and Huffer, 1933, 1934). Stebbins immediately embarked on a programme of research into the properties of the ISM that was to occupy Washburn Observatory for the next two decades.

7 RESEARCH INTO DUST AT WASHBURN OBSERVATORY

Over the years 1933–41, Stebbins, Huffer, and Whitford published seminal research on the photometry of stellar reddening, using the Washburn 15-inch refractor and the 60- and 100-inch reflectors at Mount Wilson Observatory. UW Professor Emeritus John Mathis (pers. comm. to David S Liebl, 2002) sums up the advantage Washburn Observatory held over other observatories at this time:

> Studying dust extinction from photography was pretty marginal because plates were difficult to calibrate quantitatively, and the reddening of starlight is only obvious when there is large extinction. By contrast, photoelectric photometry is rather quantitative. So Stebbins and Whitford, and almost only they, could study the effects of extinction in stars that were lightly or moderately reddened.

Their measurements provided a precise confirmation of Trumpler's hypothesis. Indeed, when the Washburn data were used to correct Harlow Shapley's estimate of the size of our Galaxy, Stebbins is said to have remarked "We have shrunk the Universe." (DeVorkin, 1977b).

Stebbins was careful to mention the many difficulties that arose while attempting to take accurate photometric measurements for these studies. Problems with the equipment included: inconsistent sensitivity of individual cells, problems with cell voltage control, and frequent difficulties with the electrometer. He also emphasized the importance of good astronomical seeing conditions, and favourable winds to drive off the wintertime coal smoke! Stebbins (1928:18) ends on a typically-droll note by noting that "This dismal picture is brightened somewhat by the knowledge that conditions are worse elsewhere."

Washburn Observatory research on interstellar dust can be organized under the following headings: Photometry of O and B Stars; Photometry of Globular Clusters; Six-colour Photometry of Stars; and the Law of Interstellar Reddening. Each of these is discussed below.

7.1 Photometry of O and B Stars

O and B type stars were chosen by Stebbins for study because of their high intrinsic luminosities, and because they are among the most distant stars for a given apparent magnitude. This helped to compensate for the limited light-gathering power of the Washburn refractor, and provided the greatest contrast with un-reddened stars of known apparent magnitude. Beginning with a list (provided by Trumpler) of B stars likely to show the effects of reddening, Stebbins and Huffer soon expanded their list of programme stars to include all B0-B5 type stars in the Henry Draper Catalog north of Dec -15° and $m_v > 7.5$ (producing a data set of about 700 stars). This allowed them to make comparisons between stars at similar galactic latitudes, and increase the sample number in areas where reddening seemed to be most pronounced. This list eventually grew to a total of 1,332 stars with the addition of observations made by Stebbins at Mt. Wilson Observatory using the Kunz cell and Whitford's thermionic amplifier, mounted on the 60- and 100-inch reflectors (Stebbins and Huffer, 1933, 1934; Stebbins, *et al.*, 1939, 1940, 1941)

To obtain a colour index (the ratio of the brightness of a star at more than one wavelength), two calibrated filters (4,200Å and 4,700Å) were used to measure 100 standard stars situated above the plane of the galaxy. These stars were assumed to be free of significant reddening, and were termed 'normal stars'. Then, over 144 nights between 1930 and 1932, measurements were taken for the first set of 700 stars. The colour excess for these stars was determined by subtracting the colour index of a 'normal star' from the colour index of the programme star.

The results of this work clearly showed a correlation between colour excess and galactic latitude, concentrated along the median plane of the galaxy. Stebbins and Huffer (1934:258) concluded that the distribution of the reddening effect was "... quite irregular or spotted in nature ... [and] considering the Galaxy as a spiral nebula, the distribution of the observed B-stars suggests a clear space or lane inclined about 40° to a radius from the center." Figure 2 (reproduced from Stebbins, 1940) shows the distribution of reddened B-stars (colour excess (E) >0.16) in relation to the Galactic plane.

7.2 Photometry of Globular Clusters

Alongside the observations of O and B stars using the Mt. Wilson 100-inch telescope, Stebbins and Whitford measured the reddening of globular clusters over a range of galactic latitudes using the Mt. Wilson 100-inch reflector (Stebbins, 1933; Stebbins and Whitford, 1936). Globular clusters were of interest because their intrinsic luminosities and their great distance (as compared to individual stars in the plane of the Galaxy), provided a means of comparing Galactic absorption with absorption in intergalactic space. Stebbins and Whitford continued to use the same method for measuring the globular clusters as used for the O and B stars. However, they chose to use the colour excess of stars in the vicinity of each cluster as their standard, and centred the diaphragm of the large reflector on the core of the cluster to measure its colour index. They typically made six measurements of a single cluster, alternating filters and interspersing the sky background.

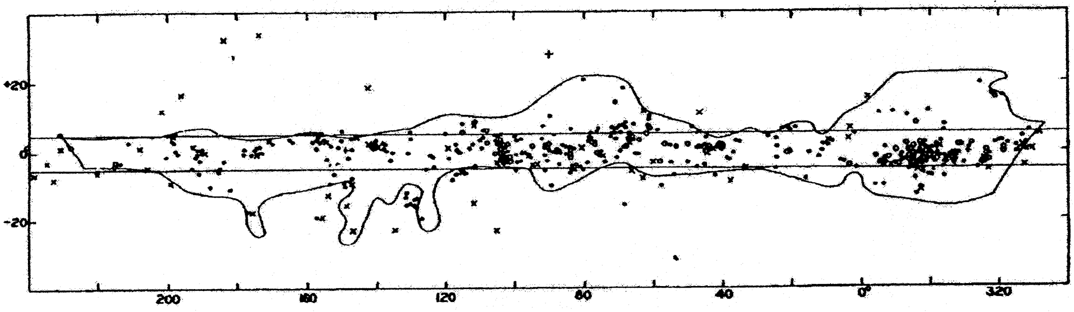

Figure 2. Distribution of reddened B stars along the Galactic plane (after Stebbins, 1940:242; reproduced courtesy of the Editors of the Astronomical Society of the Pacific).

In all, sixty-eight globular clusters visible from the latitude of Mt. Wilson were measured. Once again they found that objects near the plane of the Galaxy were reddened in comparison to those at high Galactic latitudes (Figure 3). Comparing their results to Trumpler's estimate of the diameter of the Galaxy based on open clusters (10,000 parsecs) and Harlow Shapley's estimate based on Cephied variables (80,000 parsecs), they concluded that the Galaxy was probably 30,000 parsecs in diameter (Stebbins and Huffer, 1934). Their correction of these distance measurements for the extinction by interstellar dust once again demonstrated the utility of the more quantitative photoelectric method, and it helped to secure the Wisconsin astronomers' growing reputation for precision photometry.

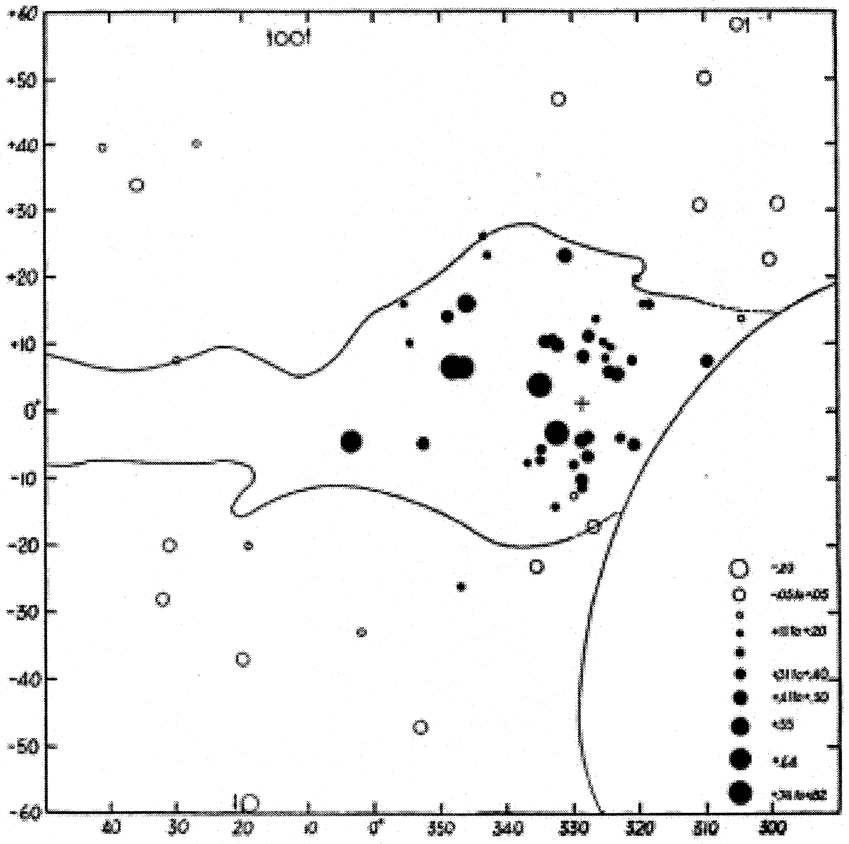

Figure 3. The colours of globular clusters (in Galactic co-ordinates) (after Stebbins and Whitford, 1936:145).

Table 1. Absorption at different wavelengths (after Stebbins and Whitford, 1943: 32).

Filters	U	V	B	G	R	I	I_1	I_2
λ	3530	4220	4880	5700	7190	10,300	12,500	∞
$1/\lambda$	2.83	2.37	2.05	1.75	1.39	0.97	0.80	0.00

7.3 Six-colour Photometry of Stars

The availability of the caesium oxide photocell, with its expanded wavelength response (3,530–10,300Å), made it possible for the Washburn group to explore spectrophotometric measurements. Their first approach was an attempt to measure stellar spectra directly using a dispersing prism on the Mt. Wilson 100-inch reflector. However, difficulties with maintaining the wavelength calibration of the instrument (Whitford, 1958) led to the adoption of a more reproducible approach (i.e., expanding the range of filter photometry from two to six colours).

Between 1943 and 1956, Stebbins, Whitford, and Kron used both the 60-inch Mt. Wilson reflector and the 36-inch Crossley reflector at Mt. Hamilton to obtain six-colour photometric measurements of 409 stars of types B, A, F, G, K, and M (Stebbins and Whitford, 1943, 1945, 1947; Stebbins and Kron, 1956). The increased spectral range of the six-colour system allowed them to more accurately discriminate among the spectra of different types of stars, and the observed effects of interstellar dust. This provided the raw data for calculations of wavelength-specific extinction, and led to the development of a law of interstellar reddening.

7.4 The Law of Interstellar Reddening

The development of a wavelength-specific interstellar absorption curve that could be applied to astronomical observations was an important goal for the Washburn Observatory scientists (Stebbins, *et al.*, 1940; Whitford, 1948, 1961). From 1930 through the 1940s, measurement of the colour excess of stars, star clusters, and galaxies remained the main research activity of the Washburn faculty.

In addition to the study of stellar luminosities, the six-colour approach proved to be an especially powerful tool for measuring the wavelength-dependence of interstellar absorption. Early in the course of their investigation, Stebbins and Whitford used measurements from thirty O and B type stars to explore the selective absorption of the ISM over the range of their photometric system. The results (Table 1) clearly show the wavelength dependency of the absorbing material for each filter. Here, for the first time, they attempted to extrapolate the absorption curve deeper into the infrared region (I_1 and I_2 are beyond the cut-off of the I filter), estimating the zero point of the curve.

The Washburn group found a simple inverse ($1/\lambda$) relationship between the absorption of light and wavelength. Compare this to Rayleigh scattering (typical of the scattering of incident light by atmospheric molecules), which is inversely proportional to the fourth power of wavelength ($1/\lambda^4$). While the group put forward an initial hypothesis describing a "... law of space reddening ..." (Stebbins and Whitford, 1943:25), it was Whitford (1958) who took this information and developed from it a systematic description of interstellar reddening in his landmark paper "The law of interstellar reddening."

By 1940, Stebbins, Whitford and Huffer had evaluated the distribution of interstellar absorption by mapping the colour excess of O and B stars within 2,000 parsecs of the Sun against the galactic latitude of the stars. While they were able to measure a strong absorption effect near the galactic plane, they failed to observe a distance-absorption relationship that would allow them to determine reddening caused by a homogeneous ISM. Whitford questioned whether the properties of the ISM (either density or composition) were sufficiently uniform to be able to apply a general law of reddening along any given line of sight.

Whitford also recognized that their six-colour measurements of O and B type stars could not stand alone as evidence for a relationship between total and selective absorption (Whitford, 1953b). To resolve this, he correlated the Washburn estimates of colour excess with data that had been obtained from other researchers using spectrophotometric methods (i.e., J Borgman, L Divan, J Dufay, J J Nassau and W W Morgan, P J van Rhijn, and C Schalén), and with data acquired using a new photoelectric scanning spectrograph developed at Wisconsin by Arthur Code (Figure 4). Earlier work (Whitford 1948) using a lead sulphide photoconductive cell with deeper infrared response (2.4 μ), had also indicated that the zero point of the coefficient curve might not be a simple linear extension of the measured values. Figure 5 shows these slopes diverging at 2μ. In addition, Whitford (1958:201) hints at preliminary indications of "... anomalies in the ultraviolet portion of the reddening curve ...", an effect that was soon to direct attention towards the first astronomical observations made from above the Earth's atmosphere.

8 WASHBURN AFTER THE STEBBINS ERA

Joel Stebbins retired in 1948, with Albert Whitford succeeding him as Director. Under Whitford, the Observatory was transformed from an independent scientific facility into an academic department within the UW-College of Letters and Science. It also expanded to include a fully-fledged graduate programme in astronomy, after having graduated only four Ph.D. students (J D R Bahng, O J Eggen, T E Houck, and C M Huffer) in the previous seventy-five years (Osterbrock, 2000a).

Nineteen fifty-eight stands as a milestone in the history of Washburn Observatory. Albert Whitford stepped down to take up the Directorship of Lick Observatory, and Arthur Code returned to Madison from Caltech to begin his own ten-year stint as Director. Whitford had recommended Code for the Washburn Directorship because of Code's experience with Washburn's photometric instrumentation, just as Stebbins had recommended Whitford for the post (D Osterbrock, private communication).

It was Code (with Robert Bless) who would expand Whitford's work on the interstellar extinction curve into the ultraviolet region of the spectrum using satellite-based observatories. John Mathis joined the group to study the nature of interstellar grains, and was followed by Blair Savage who conducted research into the physical properties of the ISM. When Joel Stebbins returned to Madison in 1958 to dedicate the new Pine Bluff Observatory and its 36-inch Cassegrain reflector, it marked the beginning of a new chapter in astronomical research at the University of Wisconsin-Madison.

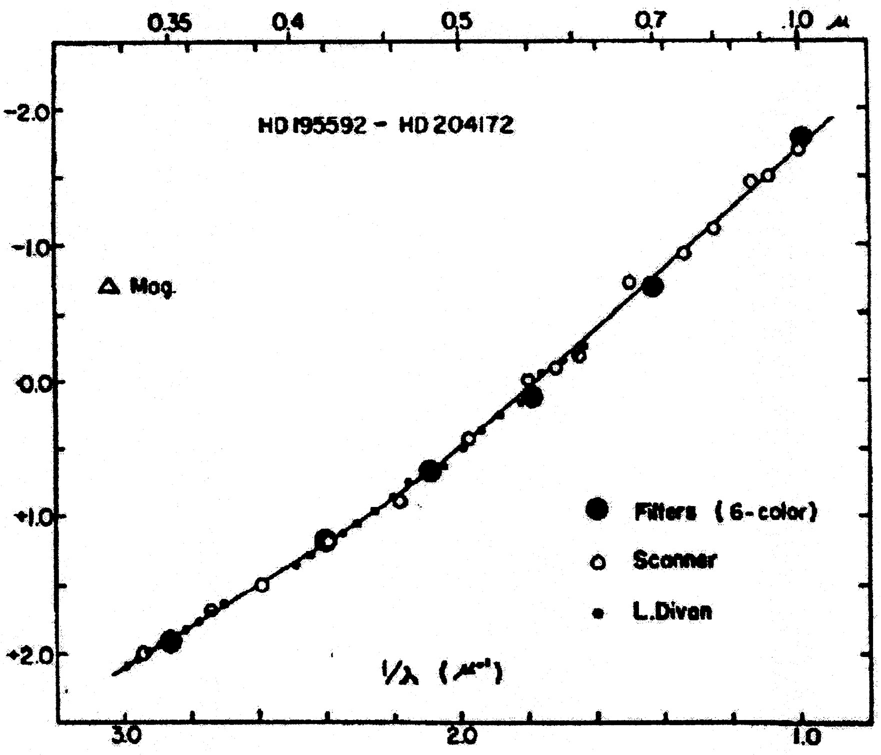

Figure 4. Monochromatic magnitude differences between a reddened star and a normal star, as observed by three methods (after Whitford,1958:203).

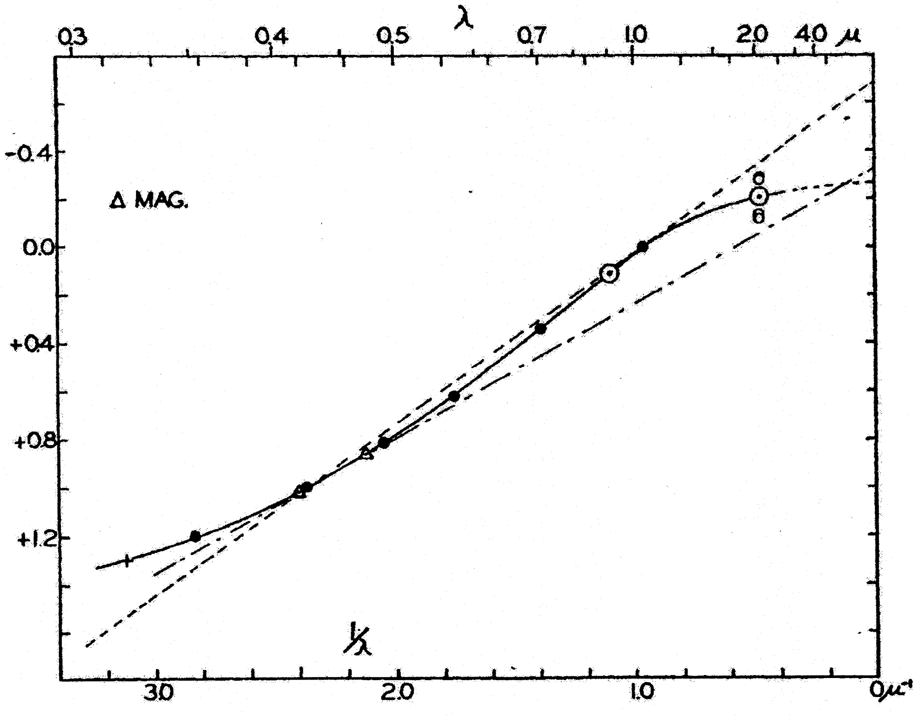

Figure 5. Mean interstellar absorption curve from four pairs of stars, reduced to V – I = 1.00m (after Whitford, 1948:105). Filled circles are six-colour observations; small open circles are individual lead sulphide observations, with large open circles showing the means; and the cross is the silver-filter observation of one pair only. The triangles show the baseline of the C1 colours.

9 DISCUSSION

As we have seen, a number of factors contributed to Washburn Observatory's pre-eminence in photoelectric photometry. Certainly, Stebbins' persistent pursuit of the development and application of photoelectric photometric technology and techniques was responsible for the Washburn Observatory's outstanding contributions. While other astronomers were quick to adopt photographic techniques for recording starlight, adoption of the photoelectric technique was less rapid. Stebbins was keenly aware of George Minchin's earlier failure to successfully promote the selenium photo-voltaic cell as an astronomical tool (Stebbins, 1940). While Stebbins' early measurement of the period of Algol convinced the scientific community of the value of photoelectric photometry, his tireless advocacy of photoelectric photometry, both within the discipline and with the interested public (Stebbins, 1934a, 1934b, 1935, 1948), did much to promote acceptance of the new technique.

The application of electrical and electronic technology to astronomy was in its infancy during these years. That Stebbins would recruit a physics graduate student (Whitford) to maintain and improve his photoelectric apparatus underscores this point. When Stebbins and Whitford first took the new photometer and thermionic amplifier to Mt. Wilson they found that no one there had a good grasp of the principles of its operation (Whitford, 1986). The ongoing refinement of photoelectric equipment and technique required skills and experience that were, to some extent, self-reinforcing. This meant that the Washburn faculty would naturally come to lead other observatories.

Specialized skill was also required to make consistently-accurate photoelectric measurements of starlight under demanding conditions. The photometrist was required to manually observe and record many observations over the course of the night, with equipment that was often challenging to use. Not only did the photometrist need to be intimately familiar with idiosyncrasies in the equipment, but judgment (based on experience) was needed to evaluate the quality of each measurement taken. This degree of experience would have been difficult to obtain anywhere else than at Washburn Observatory, with its dedicated photoelectric photometer.

Photoelectric cells were difficult to make, and more difficult to make well. Stebbins clearly valued his long-term relationship with Kunz, which afforded him access to the most sensitive and stable cells coming from Kunz's laboratory. He was even careful to cite the number of the specific cell used for a set of measurements, and transported the best cells to California when working with the Lick and Mt. Wilson telescopes. The general lack of availability of highly sensitive cells, combined with cell-to-cell inconsistency, may have discouraged other astronomers from employing the technique. This problem would only be resolved when 'off the shelf' photomultiplier tubes became available following World War II. Retired Lick Observatory Director, Donald Osterbrock (pers. comm. to David S Liebl, 2002) adds:

> The Kunz cells first, and the amplifier afterward, were much better than any other astronomers had, and enabled them [Stebbins and Whitford] to go far ahead of all the visual and photographic photometrists who were still grinding out marginal results with outdated techniques. It wasn't the telescope or the site [Madison], but because their results were so good, and their contacts too (Whitford had been at Caltech and Mt. Wilson Observatory 2 years) that they could go to the big telescopes to get data on faint stars, clusters and galaxies.

It is reasonable to conclude that it was the persistence, ingenuity, equipment, technique, and accumulated expertise of the Washburn group that put them, and kept them, at the forefront of American photoelectric photometry.

Washburn Observatory during these years serves as an interesting case study of the changing character of American astronomical research in the early twentieth century. Donald Osterbrock's comment (ibid.) about the Stebbins' era is telling in this respect: "Astronomy departments did not have research programs back then, especially ones as small as UW's. Astronomers did the research they were most interested in, and could do with the equipment they had."

We have seen how Comstock and Stebbins directed the research at Washburn, following their own interests with the assistance of a few staff. The Observatory was nominally independent of the academy at UW, and although Comstock and Stebbins did teach (and even this responsibility was delegated, in part, by Stebbins to Huffer), Washburn was primarily a research facility. As we have seen, the astronomy programme at Wisconsin did not have a graduate degree programme as such until Albert Whitford began the process of integrating it into the College of Letters and Science after Stebbins' retirement.

The continuity of interest between the Directors, combined with Washburn's standing as a dedicated photoelectric photometry observatory, likely served to support the consistency in mission and approach to astronomy that we see over this period. In each case (Stebbins, Whitford, and Code), the new Director was a former student and/or colleague, who shared the vision and skills of his mentor. While it might be difficult to argue that Comstock and Code had much in common, one can easily trace their lineage as scientists through Stebbins and Whitford.

While Washburn Observatory between 1922 and 1958 might seem quite different from modern astronomy as characterized by the 'Big Science' of the post-Cold War period, some things have not changed. Stebbins excelled in his rôle as a mentor for talented young astronomers (e.g. Gerald Kron), first helping to place them at prestigious institutions and then collaborating with them. Establishing and maintaining productive collaborations with other astronomers was also a high priority, along with service to the institutions that supported astronomy in America (Stebbins, 1931; Whitford, 1972). Perhaps it is most accurate to describe Washburn Observatory during this time as a case study of astronomy in transition, from the era of the solitary observer to that of the highly co-ordinated and directed research programme.

We end with these final word of inspiration for future astronomers from Joel Stebbins (1958: 449), expounded at the dedication of the University of Wisconsin's Pine Bluff Observatory: "It is a far reach from the simple methods of astronomical observation in 1878 to the rather complicated procedures of today, but whatever the direction that research takes at Washburn Observatory, we can be sure that its value will be limited only by the imagination and energy of its staff."

10 ACKNOWLEDGMENTS

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11 NOTES

1. During the fall of 2002, one of us (DSL) established a dialog about the Stebbins years with the following Washburn Observatory scientists: Arthur Code (1951–56, 1958–68), Donald Osterbrock (1958–73), John Mathis (1959–), and Blair Savage (1968–). Quotations in the text by these individuals are taken from the author's interview notes or the subject's correspondence.
2. Lankford (1977) Table 11.5 shows that staffing in American observatories averaged four individuals during 1931.
3. Other accounts of Stebbins' photometric work at Illinois include: DeVorkin (1985); Huffer (1955); Stebbins (1914, 1915, 1940, 1950, 1957).

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A leading nineteenth century instrument-maker in Norway and his astronomical and geodetic instruments

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Abstract

Christian Holberg Gran Olsen introduced the European continental standards of scientific instrument making into Norway in 1861, following a four-year tenure with A & G Repsold, Hamburg. This paper lists and discusses the major astronomical and geodetic instruments made by Olsen. The geodetic instruments are now in museums or in university storage. The first universal instrument was extensively used to carry out the Norwegian part of the European Geodetic Arc 1863-1878, both as a theodolite and as a transit instrument for astronomical observations at selected geodetic stations. Other instruments contributed to the mapping of Norway. Olsen's last model was used to determine the position of Fridtjof Nansen's polar ship *Fram* during its three-year expedition (1893-1896) in the Polar Sea. It was also used on other Norwegian polar expeditions during the next thirty years. A copy was made for the first winter expedition to Antarctica, with Belgica in 1897-1899. The first astronomical refractor by Olsen (with a 10.8-cm Steinheil objective lens) was made for Bergen Observatory in 1869. Its current whereabouts have been investigated, but the instrument has not been found. Two larger refractors have been successfully searched for. A 13.2-cm Merz refractor with mounting by Olsen, made for the University Observatory in Christiania (Oslo) in 1883, exists in refurbished condition. The largest refractor, with a 36.0-cm objective lens by Olsen (7 metres focal length), was the centrepiece in what appears to be the first astronomical observatory in Europe founded and operated exclusively for the public on a commercial basis. When erected in 1885, the refractor of the Peoples' Observatory in Oslo was unsurpassed in size in Scandinavia, even at professional observatories.

Keywords: *Olsen, refractors, universal instruments, theodolites, equatorial mounting, people's observatory*

1 C H G OLSEN: HIS TRAINING AND PROFESSIONAL LIFE

Christian Holberg Gran Olsen was born into a large family of gifted and creative people in 1835. He grew up at the family farm, Einstapevoll, in Valestrand, north of the city of Haugesund, on the west coast of Norway. His father had done extensive international travelling as a young sailor and many impressions were implemented at the farm in modified forms. He was also innovative in his own right and developed new solutions and machinery to deal with everyday challenges. Music, religion, and hard work were integral elements of life at the farm.

About 1850, C H G Olsen moved to Bergen to become an apprentice at the optical and mechanical workshops of Ulrik Fredrik Krog, the city's Master of Weight and Measures. Krog had been trained by Repsold in Hamburg, and upon realizing Olsen's talent he arranged for his further training at the firm of A & G Repsold, then recognized as one of the world leaders in the development and construction of astronomical and geodetic instruments. Olsen arrived at Hamburg in 1857 April to work for Repsold (Cranner, 2001:44), and he became a valuable and trusted instrument maker. In 1859, Repsold wrote a flattering statement in support of Olsen's application for a stipend from Norway's National Assembly to study mathematical and physical instruments in Paris. Repsold also asked Christopher Hansteen, Professor of Applied Mathematics in Oslo and Director of the University Observatory, to support the application (Cranner 2001: 46). Apparently this application was successful, and in 1861 Olsen spent six months in Paris before returning to Hamburg. On 1861 September 21 he resigned from the Repsold firm, moved to Christiania (now Oslo), the capital of Norway, and established his own company. He gradually expanded and soon dominated the national market for scientific instruments and complex mechanical innovations. He also trained a number of talented, young men, thus improving the standards and availability of scientific instrument-makers in the country. Some started their own workshops. In 1898 Olsen sold his business to two of his senior staff and in the remaining twenty-three years of his life devoted himself to innovative projects carefully selected to match his personal interests. His company is still active today.

On several occasions, Olsen's innovative and beautifully-handcrafted instruments won him medals and awards at national and international exhibitions (Paris in 1875, 1878, and 1881, and Christiania in 1883), and he obtained several patents. He seems to have been more preoccupied with technical developmental work and less focused on business smartness. He thus ended up taking rather large risks at times, and his annual income varied by orders of magnitude. All his major designs were prototypes or one-of-a-kind units.

Olsen married Pauline Dobbertien in Hamburg in 1859 July when he was still employed at A & G Repsold. After moving to Oslo in 1861, they eventually had six sons and two daughters. The bilingual Olsen residence cultivated a variety of cultural activities, including science, literature, music, religion, and foreign languages. Olsen's children sought professions in music, education, and engineering. The oldest son, Henrik, moved to Munich and became a senior optician with Steinheil. Olsen's youngest daughter assisted him during the second phase of his People's Observatory, from 1911 to 1921.

2 THE INSTRUMENTS AND THEIR PRESENT STATE

Each scientific instrument fabricated by Olsen was custom-made to the buyer's specifications. Some of them are signed and dated, but many were not. Olsen was obliged to acquire some parts and elements from reputable companies abroad because he lacked the necessary tools and equipment to make them in his own workshop. Thus he obtained objective lenses from Steinheil in Munich and accurately-divided circles from Oertling in Berlin and from England. The latter was a costly solution, and Olsen started to develop his own circle dividing machine in 1865 based on the idea that the primary divisions should be limited to increments of ½ degree. Finer divisions were added by subdividing the primary intervals with special apparatus. This was successfully applied to the production of small theodolites and apparently also to the astronomical refractor for Bergen Observatory. In 1875 Olsen received government funds to construct another machine in order to divide larger circles. Many of his later instruments were delivered with his own graduated circles.

Table 1 lists Olsen's major astronomical and geodetic instruments in chronological order. Their current whereabouts and condition are commented upon in Sections 3 and 4, where we also describe all of the instruments and review their use (as documented in published papers and unpublished archival sources).

Table 1. Instruments by CHG Olsen 1863-1896.

Year	Instrument	Receiving institution	Characteristics
1863	Universal instrument	Geographical Survey of Norway	Ø=54 mm f/9 25 cm circles
1869	Refractor telescope	Bergen Observatory	Ø=108 mm Steinheil, equatorial mounting
1869	Theodolite	Geographical Survey of Norway; University Observatory, Oslo	Ø=20 mm f/9 10 and 6 cm circles
1883	Refractor telescope	University Observatory, Oslo	Ø=132 mm f/14 Merz, equatorial mounting
1885	Refractor telescope	C.H.G. Olsen's Peoples' Observatory, Oslo	Ø=360 mm f/19 Olsen, equatorial mounting
1887	Universal instrument	Geographical Survey of Norway	Ø=70 mm f/10 32 cm circles
1892	Universal instrument	Fridtjof Nansen's Arctic expedition	Ø=50 mm f/9 21 cm circles
1892	Universal instrument	Fridtjof Nansen's Arctic expedition	Ø=20 mm f/8 10 cm circles
1896	Universal instrument	Adrien de Gerlache's Antarctic expedition	Ø=50 mm f/9 21 cm circles

3 THE GEODETIC INSTRUMENTS AND THEIR USE

3.1 A universal instrument for the Norwegian section of the European Geodetic Arc

Norway joined the newly-established Commission for European Geodetic Arc Measurements in 1862, one year after Olsen established his company in Oslo. A major observing programme was planned for a 5° latitudinal arc from the southern point of the Norwegian-Swedish border (Svinesund) to Trondheim. This would be the northern extension of a multinational geodetic arc through Europe with its southern terminal at Palermo, Italy. The intention was to improve the accuracy of the parameters of Earth's ellipsoid and to establish interconnections between separate national geodetic networks in order to improve the mapping of Europe. Two historical decisions are attached to this project. At the second General Assembly in 1867 the metre was selected as the scale unit. This led to the Metre Convention beginning its meetings in 1870, with national ratification in 1875. At the seventh General Assembly in 1883, Greenwich was selected as the reference meridian.

Due to the challenging project requirements, the Geographical Survey of Norway found itself in need of a new universal instrument for the geodetic and astronomical observations. Olsen delivered the instrument (Figure 1) in late 1863, and each summer from 1864 to 1870 (and then occasionally till 1883) this instrument was taken to selected mountaintops. Theodolite observations were made to determine azimuth directions to other sites in the triangular network. These stations were then successively occupied for further observations. Thus new triangles developed along the geodetic arc as the observing programme progressed.

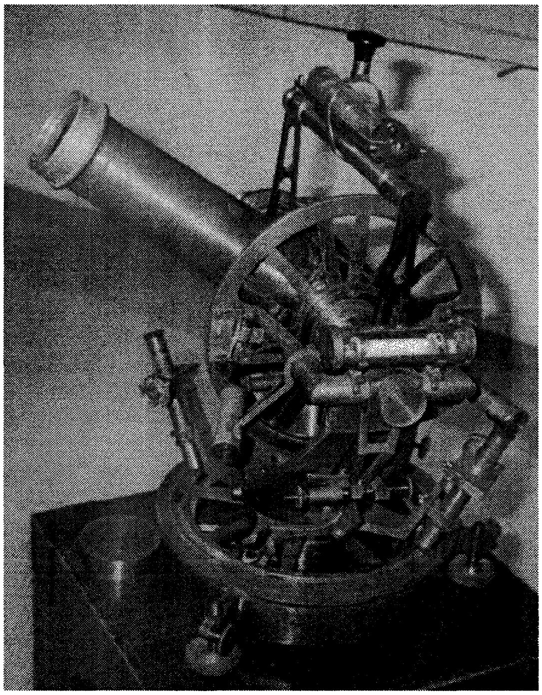

Figure 1. The universal instrument for the European Geodetic Arc, 1863.

The universal instrument was thoroughly tested by Henrik Mohn (1865) in 1864 and 1865. The telescope optics are from Steinheil of Munich. The objective lens has a diameter of 5.4 cm and a focal length of 48 cm. The optical axis is broken orthogonally by a prism into the horizontal axis of the mounting. At f/9 this instrument is well suited for stellar observations during the bright sky background conditions prevailing at daytime and during the Norwegian summer nights. It was used with an eyepiece giving a magnification of 37×. For astronomical transit observations the focal plane had one horizontal and six vertical thin wires. The circles were divided by Oertling of Berlin. The horizontal circle (diameter 25 cm) is divided in units of 10 arc minutes and is read by two microscopes. Crosshairs and micrometer adjustments allow readings to 10 arc seconds and estimates to 1 arc second. A small vertical circle is divided into four quadrants 0°-90°, in units of 20 arc minutes and can be read to 20 arc seconds from two vernier positions. Technical data for this instrument are given by Ebbesen (1891), Geelmuyden (1895), and Haffner and Mohn (1880).

In Oslo and Levanger (north of Trondheim) permanent baselines were established and measured in 1864 to a precision of 0.7 ppm (Fearnley *et al*., 1882). They set the linear scale of the geodetic network. These baselines were connected to the triangular net in 1864 and 1865 (Haffner and Mohn, 1880; Haffner *et al*., 1882) and the remaining stations were measured in 1866-1872 to complete the geodetic arc between latitude 59° at the southern point of the Norwegian-Swedish border and latitude 64° north of Trondheim (Fearnley *et al*., 1885, 1887; Haffner *et al*., 1888). Additional measurements were made 1877-1883 to improve results at individual stations and add nine new ones. Olsen's universal instrument was used as a theodolite at 40 of the 49 stations and contributed the great majority of geodetic data for this project. Other observations were made with universal instruments by Reichenbach, Repsold, and Breithaupt. The Olsen and Reichenbach instruments were used for astronomical observations at eleven sites in 1868-1872 and 1877-1881 (Geelmuyden, 1895). Local time was mainly determined from meridian transits of stars. Geographical latitudes (of nine stations) were determined by observations in the prime vertical. The azimuth directions of eleven triangle sides were determined with the Olsen instrument. This was used for the global orientation of the geodetic net. The azimuth angles were determined with typical errors of 2-3 arc seconds (rms), while geographical latitudes have errors of 1-2 arc seconds (rms).

The precision obtained throughout the Norwegian geodetic arc may be assessed by calculating the length of a selected triangle side, starting from each of the two baselines. The length difference is about 0.4 m (Fearnley *et al*., 1887; Geelmuyden 1895) for the distance between the two mountaintops Spåtind and Neverfjell (44 km), that is a relative error of about 10 ppm.

The Olsen instrument continued to serve the Geographical Survey of Norway on a regular basis until 1913 (i.e. for 50 years). It received an additional vertical circle in 1900 and was also used for trigonometric height determinations. During the 1920s it was used for astronomical positioning of Norwegian dominions in the Arctic. It is now on display at the museum section of the Norwegian Mapping Authority in Hønefoss.

3.2 Miniaturized universal theodolites

For the general surveying purposes in the districts of the country, scientific accuracy was not a requirement. This invited the use of miniaturized theodolites that were much easier to carry. A larger area could thus be surveyed in shorter time than was the case when a heavy, high-precision universal instrument was required. In 1869 Olsen delivered two small theodolites to the Geographical Survey of Norway and one to the University. The latter, a signed copy (*Olsen, Christiania, 1869*) is in storage at the University of Oslo (Figure 2), and an unsigned copy is on display at the museum section of the Norwegian Mapping Authority.

During each summer from 1869 till 1872, Olsen's miniaturized theodolites were used to establish a dense triangular network to support mapping of the County of Troms in Northern Norway. It turned out, despite rather long distances between each surveying site, that the results were better than expected. This was confirmed by re-measuring several sites with the far more accurate universal instrument in 1872. Part of the success was the precision obtained by Olsen when he divided the 10-cm diameter horizontal circle and 6-cm vertical circle on the small circle dividing machine he constructed in 1865.

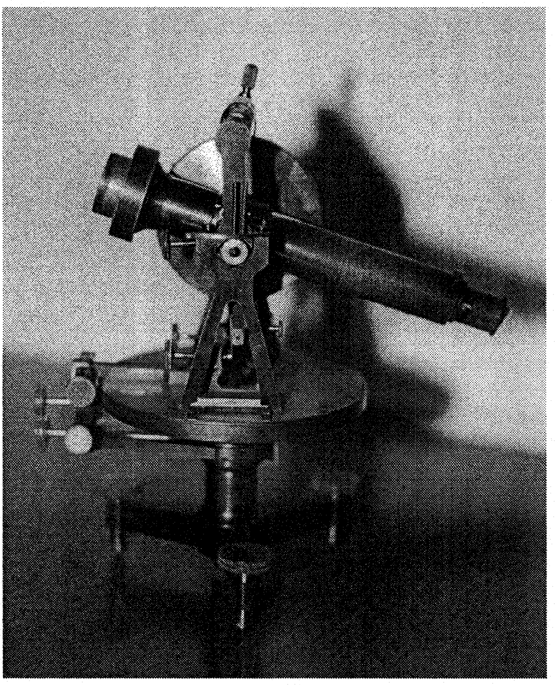

Figure 2. The miniaturised universal theodolite for the Geographical Survey of Norway, 1869.

3.3 A large universal instrument for the Geographical Survey of Norway

In 1886-87 Olsen produced a large universal instrument for the Geographical Survey of Norway (Figure 3). It was accompanied by a box chronometer (made by the clockmaker, Michelet of Oslo, in 1888), and was used until 1894 for astronomical control of the Norwegian first order geodetic net by determination of latitude, longitude, and azimuth in selected stations throughout the country.

The telescope optics were made by Steinheil. The objective lens has a diameter of 7.0 cm and a focal length of 70 cm. The optical axis is broken orthogonally into the horizontal axis of the mounting, leaving the eyepiece and micrometer at a constant height above the floor for all azimuth and altitude directions. The circles were divided in London. The horizontal circle (diameter 32 cm) has two concentric scales, one for full and half values of degrees and another divided in units of 5 arc minutes, which requires microscopes to be read. There are two vertical circles. The small one reads full and half values of degrees. The larger 32-cm diameter circle is divided in units of 5 arc minutes and is read by microscopes. The two large circles are subdivided to 1 arc minute. Mechanical micrometers allow readings to 1 arc second and estimates at the 0.1 arc second level (Ebbesen, 1891). This instrument is in storage at the

University of Oslo.

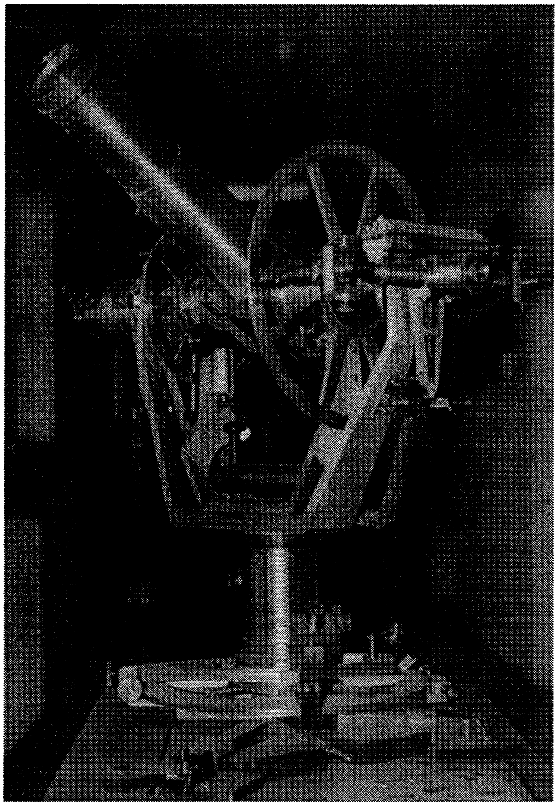

Figure 3. The large universal instrument for the Geographical Survey of Norway, 1887.

3.4 Universal instruments for polar expeditions

In preparation of the Arctic expedition with the polar ship the *Fram*, Olsen delivered two universal instruments (Figure 4) to Professor Fridtjof Nansen in 1892 (Geelmuyden, 1901). One remained on the ship, which was intentionally frozen in from 1893 September until 1896 August to track the currents of the Polar Sea through frequent astronomical determinations of position. The horizontal and vertical circles are both 21 cm in diameter and graduated to 10 arc minutes. Microscopes allow readings to 10 arc seconds and estimation to 1 arc second. The telescope has an objective lens of 5.0 cm diameter and a focal length of 42 cm, and is equipped with thirteen lines engraved on glass. The optical axis is broken orthogonally into the horizontal axis by a reflective prism. This instrument, signed "C.H.G. Olsen, Christiania 1892", accompanied the *Fram* on later polar expeditions, for example by Otto Sverdrup to Greenland in 1898-1902 (Isachsen 1907), and Maud on the North Pole Expedition of 1918-1925 (Sverdrup 1928). Latitudes determined from circum meridian altitudes of the Sun or stars were accurate to a few arc seconds, while longitudes were uncertain to 1 or 2 arc minutes at these high northern latitudes (Sverdrup 1928).

The second universal instrument is miniaturised with circles of 10 cm diameter, graduated to half a degree. Microscopic reading with verniers gave 1 arc minute. The telescope, also with a broken axis, has an aperture of 2 cm and a focal length of 16 cm. The focal plane wires, one horizontal and two vertical, are fine lines engraved on glass. This instrument accompanied Fridtjof Nansen and Hjalmar Johansen on their ski and sledge expedition of 1895 March – 1896 June.

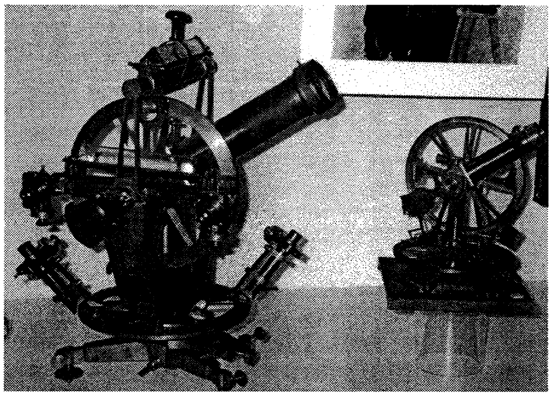

Figure 4. The universal instruments for Fridtjof Nansen, 1892.

Both instruments are illustrated in Nansen (1942:72, 165, and 185) and are now on display at the Fram Museum in Oslo.

A copy of the 21-cm universal instrument was made for Adrien de Gerlache's expedition with Belgica (Internal memo, 1898). The Belgian Antarctic Expedition of 1897-1899 was the first scientific winter expedition to Antarctica. It was realized in response to a resolution of the Sixth International Geographical Congress in London in 1895 July, which urged the exploration of that continent before the close of the century. George Lecointe of Belgium was Navigation Officer and Expedition Astronomer.

4 THE ASTRONOMICAL INSTRUMENTS AND THEIR USE

4.1 A refractor for Bergen Observatory

Olsen made his first astronomical telescope in 1869 in response to a request from Bergen Observatory. A new observatory building had just been completed and a 10.8-cm refractor by Olsen was mounted in a tower with a revolving conical roof. The brass telescope tube had an objective lens from Steinheil. The equatorial mounting was equipped with a mechanical clockwork to track stars. A selection of eyepieces made it a useful instrument for public viewing of astronomical objects under the supervision by the City Astronomer. Occasionally more than one hundred persons attended such events, according to annual reports (e.g. Åstrand, 1871, 1873). The telescope was also equipped with a micrometer for determination of relative positions of astronomical objects (Åstrand, 1895). Bergen Observatory discontinued its operation in 1902 and the property was taken over by Bergen School of Navigation. Two years later, the refractor was transferred to an astronomical dome at the new school building (Helland, 1916). This dome is empty today, and no further reports on the fate of the telescope have been found.

4.2 A mounting for the University Observatory in Oslo

In early 1883, Olsen completed an equatorial mounting with setting circles and mechanical clock drive for a 13.2-cm Merz refractor that was acquired by the University Observatory in Oslo (see Figure 5). He was invited to display and demonstrate the telescope at the Christiania Exhibition that summer,[1] and it was mounted in a small observatory where visitors were allowed to view astronomical objects. This proved quite an attraction and won Olsen a silver medal for his craftsmanship. This unexpectedly large public interest made Astronomy Professor Carl Fredrik Fearnley decide that the telescope should remain in its temporary observatory for at least another year. It was used for star parties, but also served science: on 1884 October 4 Henrik Mohn observed occultations of faint stars during a total lunar eclipse (Fearnley, 1885). Later it was mounted in a new pavilion at the University Observatory and was used occasionally for timing lunar occultations, partial solar eclipses, and transits of Mercury (see Fearnley, 1888; Geelmuyden, 1891a, 1891b, 1900, 1905, 1908; Schroeter, 1921). Over a period of more than fifty years, the telescope was made available to the public once per week (weather permitting).

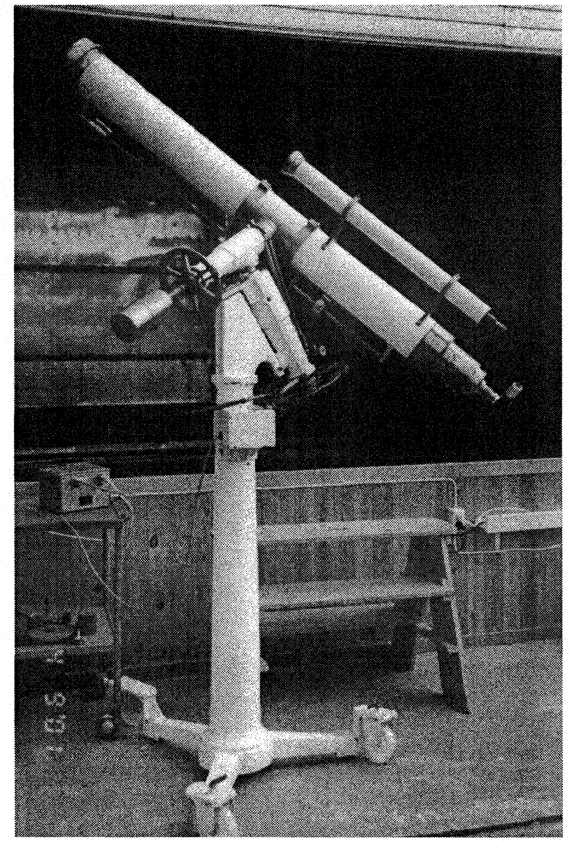

Figure 5. The 13-cm Merz refractor, mounting by Olsen, 1883.

The Merz-Olsen refractor was transferred to nearby Ruseløkka Primary School in 1937 where a teacher with a personal interest in astronomy showed sky objects to his classes. Upon his retirement this activity was discontinued, and after decades of non-service and neglect it was dismantled in 1988 and returned to the University of Oslo. It was then refurbished and set up at their Harestua Observatory (a former solar research observatory; now an excursion centre for school classes and astronomy training of teachers), where it is once again used for star parties.

4.3 The People's Observatory

The public interest shown in astronomy during the Christiania Exhibition was the direct experience that led Olsen to plan, construct, and operate a large refractor in a public observatory in the centre of the Norwegian capital. Visitors to the Christiania Exhibition willingly paid a small fee to look through the telescope and showed considerable interest in Olsen's entertaining mini-lectures on astronomical objects. He decided that a market existed for an observatory that was open to the public. Although it was not uncommon in other countries for private and university observatories to welcome visitors, sometimes on a regular basis, the People's Observatory in Oslo appears to be the first establishment to be founded and operated exclusively for the astronomical entertainment of the public. It was run on a commercial basis, and was never used by professional or amateur astronomers to collect astronomical data. Olsen's idea attracted attention in other European countries, and he was approached by several individuals and societies wanting to establish similar facilities. In a newspaper interview reported in the 1910 October 16 issue of the *Aftenposten*, Olsen stated that Wilhelm Foerster in Berlin corresponded with him in the late 1880s and that his project became the model for the Urania Observatory. There was a similar story about Antwerp in Holland.

Olsen acquired optical blanks from Charles Feil[2] in Paris and spent eighteen months grinding and polishing the two-element objective lens for his 36-cm refractor. Astronomer Hans Geelmuyden at the University Observatory in Oslo calculated the theoretical characteristics of each surface of the achromatic lens, upon Olsen's request. This formed the basis for the optical tests during the production process. Further input was received from Carl Lundin, senior optician with Alvan Clark & Sons, USA. With an older brother, Lundin had emigrated from Sweden to Norway and worked in Olsen's company around 1870 (Geelmuyden, 1918). He moved to America in 1873 and began working for Alvan Clark & Sons the following year (Briggs and Osterbrock, 1988). During the summer of 1883 Lundin travelled to St.Petersburg to deliver the 76-cm objective lens for the new giant refractor at Pulkova Observatory (Krisciunas, 1978, Struve, 1889). He also visited his homestead in Sweden and his brother in Christiania before returning to America. In Oslo he met up with Olsen, his old master, who was making money through the refractor at the Christiania Exhibition and was developing the idea of a telescope of his own. Lundin explained to Olsen the unusual techniques of final adjustments of the lens surfaces developed by the Clark firm, by corrective polishing and direct testing of stellar images at the telescope focus, and Olsen adopted this approach for his own objective lens. Family anecdotes report that he was seen polishing the objective lens (while mounted in the telescope tube) on many occasions over the next thirty years. Apparently he challenged himself to continually improve its quality.

The equatorial mounting and other mechanical parts were completed in Olsen's workshop during 1884, and in 1885 March the 7-m long telescope tube was mounted on top of a 4-m high pillar inside the observatory building (Figures 6 and 7) in the south-eastern corner of the park surrounding the Royal Castle in Oslo (*Naturen*, 1885). This was the largest telescope in Scandinavia at the time (Figure 8). The 36-cm crown glass front lens was separated from the flint glass lens by an air space of 7 mm. The focal length was 680 cm. The equatorial mounting had a mechanical clock drive to track stars, and was equipped with 48-cm setting circles that could be read to 2 seconds of time in right ascension and 1 arc minute in declination. A collection of eyepieces allowed magnifications of up to 1200 times.

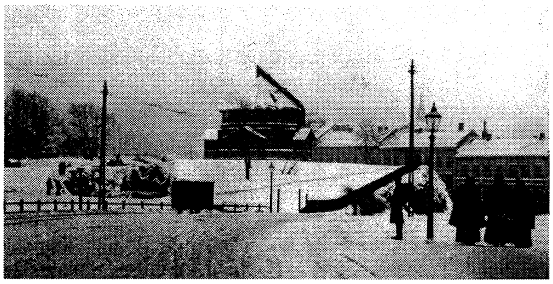

Figure 6. The People's Observatory in Oslo about 1894, viewed from the west.

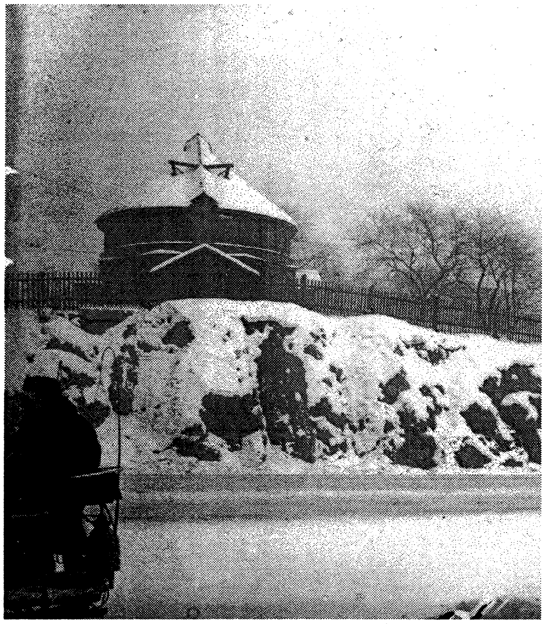

Figure 7. The People's Observatory viewed from the south.

Olsen named his facility *Folkeobserv-atoriet*, or the 'People's Observatory',[3] and individuals and school classes could view astronomical objects through the telescope and hear Olsen and his assistant lecture on the various objects and other topics. On 1885 August 29, when showing the Andromeda Nebula to a group of visitors, Olsen noticed that the galaxy core had an unusual appearance and he checked his observation several times (Tromholt, 1885). In effect, this was an independent discovery of what today is known as the first supernova in an external galaxy; it had been discovered one and a half week earlier at Dorpat Observatory in Estonia. Olsen operated his People's Observatory until 1894, when it had to be moved to another area of the park because of road construction work by city authorities. During this time it is said to have received 60,000 visitors.

In its new location the foundation for the observatory building was insufficiently set, and some time after re-erecting the telescope, the conical, revolving roof jammed and would not rotate. The People's Observatory was no longer operational. It took more than ten years to raise money for a new observatory building, but by then the observing conditions in downtown Oslo had deteriorated significantly, and a new location was selected at Holmenkollen, in the hills to the west of Oslo (Figure 9). In 1911, the telescope once again became available to the public (Figure 10). By this time, Olsen was 76 years old, but he was still the driving force and leading personality on the observing floor, although now assisted by his youngest daughter. Many fewer visitors turned up, however, possibly due to the longer travel distance, and the observatory was finally closed soon after Olsen's death in 1921. After being dismantled and removed from the Holmenkollen Observatory, there was a dispute about the ownership of the telescope, and its future use. Finally, parts of the telescope and mounting ended up in various storage locations at the Norwegian Technical Museum in Oslo. Decades later, in 1989, a request to the Museum revealed that the objective, a set of eyepieces, and several small telescope components were still in storage. Subsequently, a search by the author of storage halls containing partially unregistered heavy and large units led to the identification of the major telescope parts, including various pieces of the telescope tube, axes, counter weights, bearings, and the pier. A recent revisit to the new storage premises of the Museum revealed that all major pieces are now identified and collectively stored. This allowed us to measure different features of the telescope and mounting and to reconstruct details of the design.

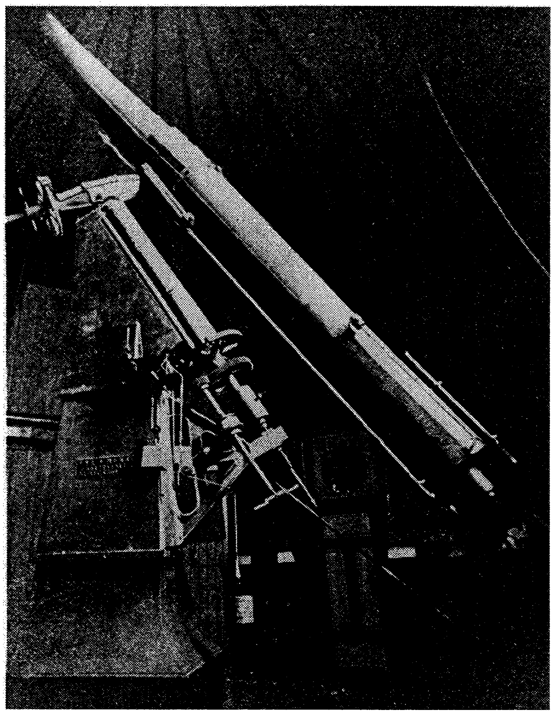

Figure 8. The 36-cm refractor inside the People's Observatory.

5 CONCLUSIONS

After serving both the national surveying community and international geodesy for several decades, the geodetic instruments are in well-preserved conditions in the museum section of the Norwegian Mapping Authority in Hønefoss, the Fram Museum in Oslo, and at the University of Oslo. Two of the astronomical instruments have been 'rediscovered' by tracking printed and hand written sources. The 13.2-cm Merz refractor had already been refurbished by technical personnel at the University of Oslo when the author accidentally came to know about it and identified it as Olsen's refractor of 1883. The large 36.0-cm Olsen refractor was searched for by the author in unmarked storage areas of the Norwegian Technical Museum, and identified in 1989. A future mounting test is required to determine if all essential parts are present. Hopes are that this large instrument can again be displayed to the public. This study has revealed numerous new and original pieces of information from family sources and the successors of Olsen's company, in handwritten notes, memorabilia, and unpublished family and company photographs.

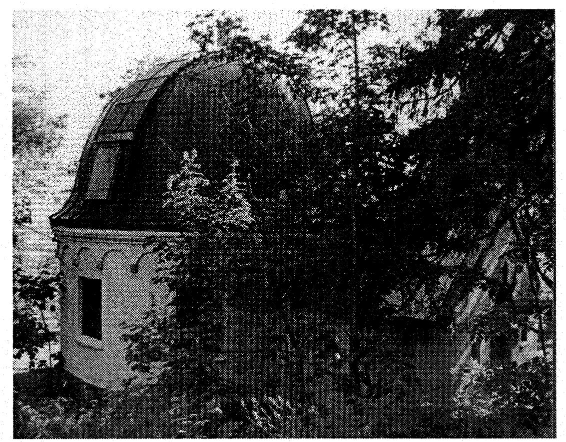

Figure 9. The observatory at Holmenkollen (recent photograph).

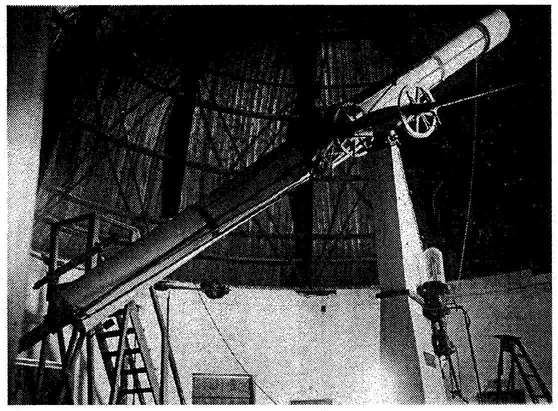

Figure 10. The 36-cm refractor, photographed in 1912 inside Holmenkollen Observatory.

6 ACKNOWLEDGEMENTS

It is a pleasure to thank the staff members of several museums, city and state archives in Norway and Germany, and at the universities of Oslo and Bergen for expertise and assistance in locating documents and other source material. Mr Karl E Ljungmann, Director and owner of the company started by C H G Olsen in 1861, has shared information and photographic material about Olsen, his projects and company, as have a number of Olsen descendants, but notably Mr Alf Cranner.

7 NOTES

1. The 1883 Christiana Exhibition was open from June 16 to October 14 and attracted 238,000 visitors, twice the population of the city at that time!
2. Charles Feil was the son-in-law and business partner of Henri Guinand (1771–1851), a son of Pierre Louis Guinand (1748–1824) who improved the techniques for producing homogeneous optical glass blanks. This led to Henri's employment with Reichenbach, Utzschneider & Liebherr in Munich from 1805 to 1813, where he collaborated with Fraunhofer for a number of years. Guinand returned to his homeland, Switzerland, in 1813 to start his own glass factory. Upon his father's death, the son brought their optical techniques to France and trained C Feil, who made blanks for the 76-cm refractor at Pulkova Observatory (Strömgren, 1945). Impressed by the quality of the Pulkova blank, Lundin possibly recommended Feil's company to Olsen when he visited Oslo in 1883. Thus, Lundin may have played a key role in the choice of glass for the 36-cm Oslo refractor.
3. A comparable term, *Volksternwarte*, was later in common usage in Germany.

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Bright stars and the history of stellar astronomy

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Abstract

"It is a poor workman who blames his tools" – but that said, a stellar astronomer can only work with the material and the tools that have been provided. When it comes to the early history of stellar astronomy the "tool" was the naked eye and the "material" was the collection of so called 'fixed' stars that were visible, these being scattered over that part of the celestial sphere that rose above the horizon. Complaining that there were too few stars or too many was nugatory, as was complaining that these stars had the wrong magnitude distribution, or were too far away, or were not moving fast enough, or were too white. This paper investigates how the history of astronomy, and specifically the pace of astronomical development, was governed by the "material" that the early astronomers were provided with.

Keywords: *visible bright stars*

1 INTRODUCTION

Astronomers are extremely lucky. The reason their science developed first, millennia before say physics, chemistry, and biology, was because the data that they were presented with was extremely limited. Initially they only had four 'species' to look at and to wonder about. In the daytime, their realm, the sky, contained a single large circular yellow disc, which they called the Sun. At night they were confronted with the cold silvery glory of the Moon and a background panoply of the stars. Five of the 'stars' wandered about. All in all, only eight things moved. The Sun, the Moon, Mercury, Venus, Mars, Jupiter, and Saturn moved around the sky in a rather restricted way, sticking at all times to the zodiacal band, and for the most part moving in the same direction. And the sky itself moved, spinning uniformly about an axis through the Pole Star (as seen by northern hemisphere observers). This produced the regular sequence of easterly rising, southerly transiting, and westerly setting.

Also, from a scientific standpoint, the 'fixed' stars were well behaved. Unlike, say, a flock of birds—each bird of similar appearance—the visible fixed stars had different brightnesses. But the range was tractable; the shiniest visible star was only a few hundred times brighter than the faintest that could be seen. Also, rather like baby bear's porridge, the number was 'just right'. The ancient night-time sky-watcher was not confronted with a mere handful of stars, or with a confusing multitude. There are, for example, only about 450 stars brighter than visual apparent magnitude four, and only about half of these are above the horizon at any one time. Many (e.g. see Ferguson 1803:349) commented on the fact that even though an observer's first impression was that the stars in the sky "… seem to be without number …", they are actually "… much thinner sown than he was aware of …", and that we should ponder "… how seldom the Moon meets with any star in her way."

The task of recording the movement of the Moon and the planets was helped greatly by the 'celestial graph paper' that the background stars provided. Here again astronomers were extremely lucky because this celestial graph paper was easy to memorize. Far from its being a boring grid of equally-spaced uniform points, the stellar background formed a hugely varied pattern of bright and less bright stars. These could be easily collected into memorable groups, the differing shapes of these groups leading to the sky being dragooned into a host of constellations representing animals, birds, fish, gods, dragons, rivers, and the like. Again, luckily, the shapes of the constellations and their relative positioning did not change detectably from generation to generation.

In this paper we are going to consider some of the important characteristics of the visible stellar background, and we are going to briefly allude to their historical significance.

2 STELLAR NUMBER VERSUS BRIGHTNESS

Lang (1991:168) provides a useful list of the four hundred and forty-six stars in the whole sky that are brighter than magnitude 4. The 'magnitude' system was introduced by Hipparchus in around 134 BC. Supposedly encouraged by the appearance of a nova in the constellation of Scorpio, he decided to produce a new catalogue of stars, working from his observatory in Rhodes (latitude 36° N). Not only did he list the positional coordinates of each of 1,028 stars (1,025 plus three duplicates), grouped into forty-eight constellations (twelve zodiacal, twenty-one in the northern sky and fifteen in the southern sky), he also introduced a grading system representing the relative 'importance' of the star. This started at 1 for the brightest fifteen stars and increased to 6, the latter grade containing all those stars that were barely visible to the naked eye. (According to Arago (1854), Hipparchus recorded fifteen stars of first magnitude, forty-five of second magnitude, two hundred and eight of third magnitude, four hundred and seventy-four of fourth magnitude, two hundred and seventeen of fifth magnitude, forty-nine of sixth magnitude, nine obscure, and five nebulous stars.) The extant version of this catalogue is reproduced in Ptolemy's *Syntaxis,* published around AD 150. Considering the stars that Hipparchus could see from his Rhodes vantage point, his 'importance' magnitude 1 star group contains stars which have modern measured visual magnitudes less than about +1.5. From Rhodes, Hipparchus would have seen no stars with southern declinations greater than 54° South, and, due to atmospheric absorption, would have had difficulty seeing stars below about 50° South. This rules out about 12% of the celestial sphere, including the very bright stars Achernar (α Eri, apparent magnitude 0.46, declination 57° S), Rigel (or Rigil) Kentaurus (α Cen, apparent magnitude –0.01, declination 60° S) and possibly Canopus (α Car, apparent magnitude –0.72, declination 52° S).

During the astronomical renaissance, up to about the year AD 1600, all celestial maps were based on the Ptolemaic (Hipparchus) star catalogue. On these maps, stars of magnitudes 1 through 6 were represented by circular glyphs of ever-decreasing sizes, a symbolization that is still in use today.

No one seemed to question the number of stars included. Why did Hipparchus catalogue 1,025 stars, as opposed to say 500 or 2,000? Using equation (3) below we note that from Rhodes Hipparchus could have seen about 1025 stars brighter than visual magnitude 4.8, with 500 brighter than magnitude 4.2 and 2,000 brighter than magnitude 5.4. Maybe the astronomers of the day were fatalistic. Following *Genesis* 1:16 where God "... made the stars also ...", they may have concluded that he made just enough for their purpose, no more and no less.

The dawn of astrophysical photometry saw greater significance placed on stellar brightnesses. William Herschel concluded that the actual brightnesses of stars with magnitudes 1, 2, 3, 4, 5 and 6 were as 100, 25, 12, 6, 2, and 1 (see Chambers, 1867: 493). When Sir John Herschel was at the Cape in the mid 1830s he noted that the average star of 1st magnitude was about a hundred times brighter than one of 6th magnitude (see Herschel, 1871).

The magnitude-brightness relationship was first placed on a firm footing by the Oxford astronomer N R Pogson (1857), who suggested that an absolute scale of magnitudes be introduced such that a star of magnitude m was *exactly* $10^{2/5}$ brighter than one of magnitude $(m+1)$. Pogson tacitly assumed that the human eye responds uniformly to the logarithm of the stellar brightness (logarithms being much in vogue during the eighteenth and nineteenth centuries).

This response characteristic was formalised in 1860 by G T Fechner, and can be expressed as sensation ∝ log (stimulus). Under these circumstances the measured brightnesses of stars, b, with given 'Hipparchus' magnitudes, m, are forced to fit a relationship of the form

$$\log b = c_1 + c_2 m, \tag{1}$$

where c_1 and c_2 are constants. If it is assumed that a star of 1st magnitude is exactly a hundred times brighter than one of 6th magnitude then $c_2 = -0.4$. The assumed logarithmic physiological response of the eye's retina to visual stimuli led to the stellar visual magnitude scale 6th, 5th, 4th, 3rd, 2nd and 1st being equivalent to stellar brightnesses of 1, 2.51, 6.3, 16, 40, and 100 respectively.

Unfortunately, even at the time, some researchers doubted the veracity of the logarithmic relationship. It is now realized (e.g. see Stevens, 1961) that the eye's perception of stellar brightness is better described by a power law such that sensation ∝ $(\text{stimulus})^n$. Here

$$\log m = c_3 + n \log b. \tag{2}$$

Young (1984) compared the stellar magnitudes in Ptolemy's *Almagest* with modern accurate photometric brightnesses and concluded that $n = -(0.4 \pm 0.1)$, a value close to $n = -0.5$ which was suggested by John Herschel in 1847. The universal acceptance of equation (1) in present-day astronomical circles is rather surprising considering the doubts expressed by, for example, John Herschel (1871:563-564).

There is another important question concerning the work of Hipparchus. Sirius (apparent magnitude –1.46) was about 960 times brighter than the faintest visible 6th magnitude star that could be detected. Why was this range of brightnesses divided into six intervals as opposed to say four or ten? We are not told. Interestingly, in the early fifteenth century Ulugh Beg (1394–1449) subdivided each of the Hipparchian six categories into three subdivisions, with a distinction being made between small, intermediate and large stars of each specific magnitude (see Humboldt, 1851:121). Decimal gradations were introduced in the early nineteenth century by F G W Struve and F W A Argelander.

Following the accepted logarithmic tradition expressed by equation (1), Richard Proctor (1892:717) introduced the number 1.585 (this being $10^{2/10}$), which he referred to as the 'distance-ratio'. This corresponded to the proportional distance to which a star must be removed in order that its magnitude would be increased by unity. Imagine a series of spherical shells with radii r, $1.585r$, 1.585^2r, 1.585^3r, etc., centred on the observer. These shells enclose volumes that increase successively by a factor of 3.981 (this being $10^{6/10}$). So (i) *if* stars were distributed uniformly throughout space, and (ii) *if* stars all had exactly the same luminosity, and (iii) *if* space was completely transparent (i.e. there was no interstellar absorption) we would therefore expect the number, N_{m+1}, of stars brighter than magnitude $(m+1)$ to be exactly 3.981 times greater than the number N_m, of stars brighter than magnitude m. Is it? Figure 1 shows the way in which the number of bright stars with magnitudes between m and $m + 0.25$ vary with magnitude over the range $-1.4 < m < 4.0$. Figure 2 is a plot of the logarithm of the cumulative number N_m, as a function of m, over the same range. The linear fit to the data in Figure 2 has the form

$$\log N_m = (0.654 \pm 0.04) + (0.5003 \pm 0.0013)\, m. \tag{3}$$

Notice that $10^{0.5003}$ is equal to 3.164 and not 3.981, so one (at least) of the three assumptions listed above is erroneous. Notice, however, that the three assumptions do present a reasonable explanation for the number-brightness relation of the stars that are seen in the sky. If one asks "Why do stars have different brightnesses?" the fact that they might all have similar luminosities but be at different distances from the observer is a reasonable explanation. If one asks "Why are there many more faint stars than bright ones?", then the way in which the volume of a uniformly-dense visible cosmos increases with its radius offers a reasonable clue to the explanation.

Before we consider who, in the history of astronomy, worried about questions such as these, let us briefly discuss limiting stellar magnitudes. In the period before 1609, before Galileo introduced the astronomical telescope to our subject, astronomers were confined to the use of the naked eye, with its limited dark-adapted pupil diameter. On a clear dark moonless night the typical dark-adapted eye can detect stars brighter than about 6th magnitude. The objective lens or mirror of a telescope enables more light to be collected and concentrated into the eye pupil. The limiting magnitude, m_{lim}, of the stars that can then be detected increases as a function of the clear aperture A

(mm) of the instrument being used. According to, for example, Ridpath (2004:29)

$$m_{\mathrm{lim}} = 2.68 + 5.0 \log A. \qquad (4)$$

Robert Hooke, in his famous work *Micrographia* (1665:241), was the first to consider the relationship between telescope aperture and the number of stars that could be detected. Cassini (1717:260) also touched on the same problem. But unfortunately he suggesting that the ability of a telescope to detect faint stars was a function of its magnification. William Herschel commented on the magnitude-distance relationship, echoing the views expressed by Isaac Newton around 1692 (see, for example, Hoskin 1977). Herschel (1782:102) wrote, "Let the differences in their apparent magnitudes be owing to their different distances, so that a star of the second, third or fourth magnitude is two, three or four times as far off as one of the first." Halley (1720) perceptively took the Newtonian suggestion to its correct conclusion. Thinking in terms of stars all being similar in luminosity and size to the Sun, and being distributed at more or less regular intervals throughout space, the first magnitude stars were clearly those closest to the Sun. Halley assumed that there were thirteen first magnitude stars. He wrote that

> … at twice the distance from the Sun there may be placed four times as many, or 52; which, with the same allowance, would nearly represent the number of the Stars we find to be of the 2d magnitude: so 9 × 13 or 117, for those at three times the distance: and at ten times the distance 100 × 13 or 1300 Stars; which distance may perhaps diminish the light of any of the Stars of the first magnitude to that of the sixth, it being but the hundredth part of what, at the present, they appear with. (ibid.).

This is exactly the definition that Pogson used 130 years later when introducing the present-day logarithmic magnitude scale.

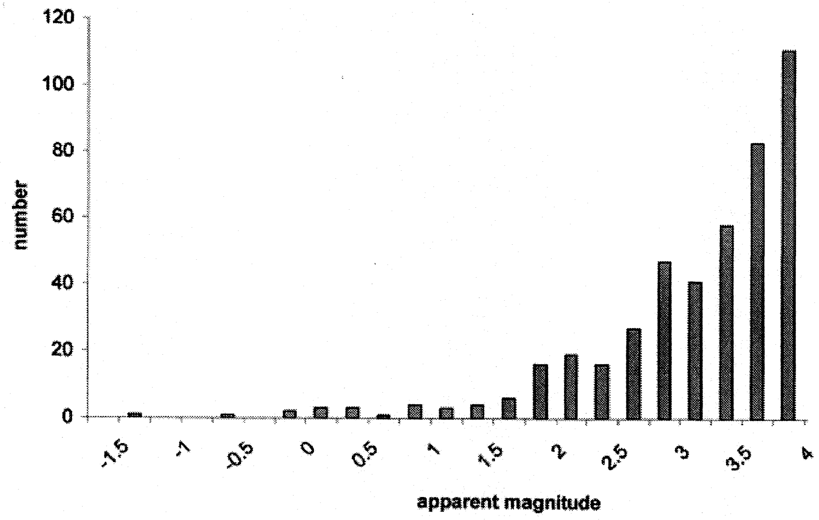

Figure 1. The histogram shows the way in which the number of 'fixed' bright stars with magnitudes between m and m+0.25 vary as a function of magnitude. These data have been taken from Lang (1991).

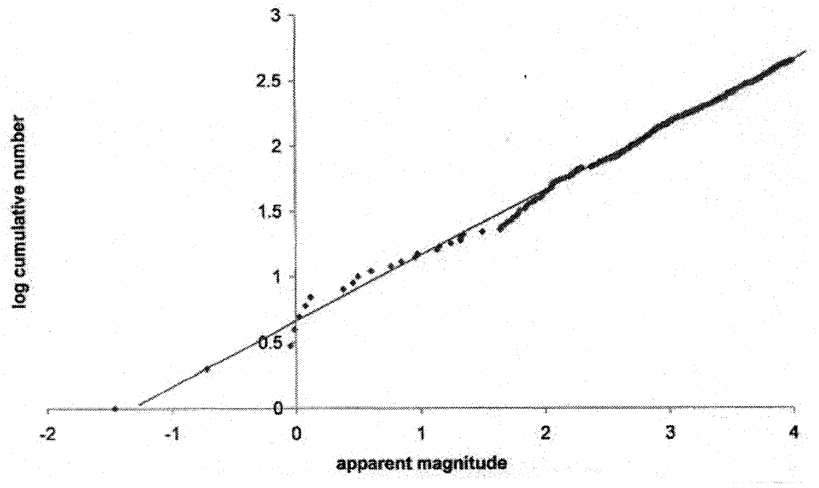

Figure 2. A plot of the logarithm of the cumulative number of stars brighter than a visual magnitude m, as a function of m. The line through the data is represented by equation (3).

Galileo's first telescope, in 1609, had a usable aperture of 1.5 cm. The telescope used by Huygens in 1655 had an aperture of 5.7 cm, the Dollond 1757 achromat, 10 cm, and the 1820 great Dorpat refractor, 24 cm. These increases in aperture lead to an increase in stellar limiting magnitude and a huge change in the number of stars that could be detected. The size of this increase was a matter of considerable debate. What was the teleological significance of this profusion of faint stars? Why had God bothered to make stars that could not be seen with the naked eye? Perhaps it was to encourage us to build ever-larger telescopes! As larger and larger lenses were used, more and more increasingly fainter stars were seen. Where would it end? Would one arrive at a telescope size that would enable the astronomer to detect all the stars in the Universe? Would larger telescopes then become nugatory? Did anyone ask "Just how many stars are there in the Universe?" (When my students ask this question I simply say "10^{23}", quickly multiplying my guess as to how many galaxies there are (10^{11}) by 10^{12}, the estimated number of stars in each galaxy.)

In the early seventeenth century, Galileo noted that "... the Galaxy is nothing else than a congeries of innumerable stars distributed in clusters. To whatever region of it you direct your spyglass, an immense number of stars immediately offer themselves to view, of which many appear rather large and very conspicuous but the multitude of small ones is truly unfathomable." (cited in van Helden, 1989:62). Around this time in the history of astronomy we quickly saw the rather cosy confined Aristotelian cosmos, with its small number of nested spheres, being replaced by a cosmos of infinite and homogeneous extent governed by Euclidean geometry. It was suggested that the newly-observed telescopic stars were not seen by the unassisted eye simply because they are too far away. Telescopes did not change the 'look' of the stars. They were still points of light. Ferguson (1803:2) suggested that this "... proves them to be at least 400 thousand times farther from us than we are from the Sun." Thereafter, many popular books were rather loose in their terminology. Aspin (1825: 26), for example, writes: "... the fixed stars, according to their size and brilliancy, are divided into magnitudes." Size surely did not come into it. With the exception of the Sun, all stars appeared as point sources.

Let us return briefly to equation (3) and the magnitude distribution index, that is the number 3.164. William Herschel (1784) tried to estimate the extent of our Galaxy by 'star gauging'. To this end he simply pointed his telescope in six hundred and eighty-three different directions and counted the number of stars that he could see in each field of view. Here he was simply counting down to a specific limiting stellar magnitude and the number of stars seen was taken to be proportional to the cube of the distance to the edge of the system. The results were used to arrive at a rough shape of the universe, which in those days was regarded as being a single galaxy with the Solar System in a central position. A more sophisticated approach was to count stars down to different magnitude limits. This, however, needed an accurate estimation of the magnitudes of all the stars in the field of view. Many years had to elapse before this was possible (e.g. see Seares, 1928).

In 1856, Lardner wrote "The number of stars in each succeeding magnitude increases rapidly as their splendour diminishes. Thus there are no more than 18 or 20 of the first magnitude, there are 50 or 60 of the second, and about 200 of the third, and so on; the total number visible to the naked eye, up to the sixth magnitude inclusive, being from 5000 to 6000." (Lardner, 1856:150). Later he wrote that stars usually "... derive their variety of lustre almost entirely from their places in the universe being at various distances from us." (Lardner, 1875:377). Some years earlier, Humboldt (1851:141) reviewed the work of many star-counters and concluded that "... it is well known that on considering the whole mass, we find each class contains about three times as many stars as the one preceding." He went on to quote a population of twenty for 1st magnitude; sixty-five for 2nd magnitude; one hundred and ninety for 3rd magnitude; four hundred and twenty-five for 4th magnitude; 1,100 for 5th magnitude; 3,200 for 6th magnitude; 13,000 for 7th magnitude; 40,000 for 8th magnitude, and 142,000 for 9th magnitude.

3 STAR NUMBERS VERSUS DISTANCE

Our perception of the cosmos changed drastically with the general acceptance of the heliocentric model put forward by Copernicus (1473–1543) in *De Revolutionibis Orbium Coelestrium*. Copernicus insisted that the visible world of the fixed stars was immeasurably large (see Koyré, 1957:32). Not only, as asserted by Ptolemy, was the Earth, in comparison to the skies, "as a point", but, to Copernicus, Earth's annual orbit around the Sun was so small as to be similarly point-like. More importantly, the Copernican system presented astronomers with a possible mechanism for distance measuring. In its six-monthly journey from one side of the Solar System to the other, Earth moves two astronomical units (2 AU), a distance we now know to be around 3×10^8 km. This movement makes nearby stars move with respect to more distant stars, and a measurement of this parallactic movement would enable stellar distances to be calculated. However, astronomers had to wait until 1838 before their instruments were sufficiently sophisticated to enable this parallactic angle to be measurable.

How far away are the stars that can be seen in the sky with the naked eye? The fact that they appear as point sources indicates that their discs subtend at Earth an angle less than the 1 minute of arc resolution of the naked eye. If they had the same physical dimension as the Sun this fact alone put them further away than 30 au. Michell (1767) noted that if the stars had the same luminosity as the Sun, the fact that the brightest star was about as bright as the planet Saturn indicated that it was around 220,000 times further away than the Sun, and had a parallax of less than 2 seconds of arc.

Let us, however, return to the list of the four hundred and forty-six stars in the whole sky that are brighter than 4th magnitude (Lang, 1991:168). Figure 3 shows how the number of these stars varies as a function of their distance. The median distance is about 39 pc. The closest star in the group is α Cen (at 1.3 pc) and the four most distant are π^4 Ori, α Col, ε Cma, and δ Seg at around 100 pc. We would expect the median distance, d_{med} (pc) to vary as a function of the apparent magnitude of the stars being observed. The $m < 4$ data have been sorted according to apparent magnitude and divided into seven equally-populated star groups (see Figure 4). The relationship is rather crude, and it is roughly represented by

$$d_{\mathrm{med}} = (29.7 \pm 6.9) + (2.6 \pm 2.1)\ m. \qquad (5)$$

This relationship lends some credence to the suggestion that the brightest stars are the closest.

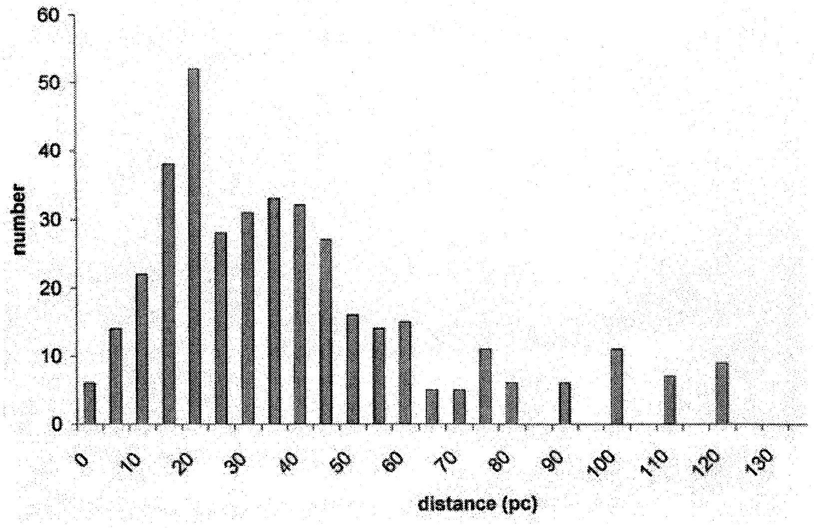

Figure 3. The histogram shows the way in which the number of bright, $m < 4$, stars vary as a function of their distance, the data being divided into 5 pc bins. Fifty percent of the stars are closer than 39 pc.

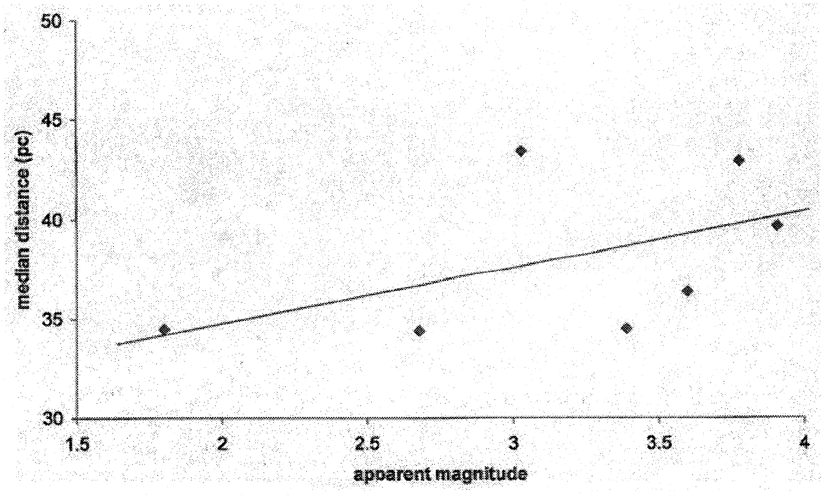

Figure 4. The median distance of the visible stars is plotted as a function of magnitude. As is to be expected, the brighter stars are somewhat closer than the fainter ones but the relationship is rather weak over the magnitude range of naked-eye stars.

A star at the median distance of 39 pc has a six-monthly parallactic shift of 0.05 arc seconds. The smallness of this angle underlines the great accuracy required for stellar distance measurement. October 1838 saw the first announcement of a measured parallax. This was for the 5.2 magnitude star 61 Cygni, a 'flying star' with a huge proper motion of 5,260 arc seconds per millennium. Friedrich Bessel had been observing this star using the Königsberg Observatory's 6.25-inch Fraunhofer heliometer. At very nearly the same time, Thomas Henderson, working in South Africa, reported the parallax of α Centauri, another 'flying star'. Other parallaxes were reported in steady succession over the next fifty years, and by 1887 the parallaxes of twenty-eight stars had been published, eleven of these stars having $m < 4.0$. Note that beyond 20 pc parallaxes measured using photographic plates taken on long-focus telescopes become too inaccurate; distances to more distant stars are usually obtained using the spectroscopic parallax technique.

4 NUMBER VERSES LUMINOSITY

In the previous Section we mentioned the common assumption that all naked-eye visible stars might be thought of as having the same luminosity. As the distances to more and more of these stars became known this assumption could be checked. The four hundred and forty-six stars brighter than 4th apparent magnitude can be sorted according to their absolute magnitude, and this is shown in Figure 5. The median absolute magnitude is found to be +0.29, making the median star about sixty-six times more luminous than the Sun (which has an absolute visual magnitude of +4.83). The histogram in Figure 5 is a reasonably approximation to a Gaussian curve, and indicates that about 23% of the $m < 4.0$ bright, visible stars have absolute magnitudes in the 0.29 ± 0.5 range. Increasing the range to 0.29 ± 1.0, 0.29 ± 1.5, and 0.29 ± 2.0 encompasses 44, 61, and 77% of the stars. The 'similar luminosity' assumption for the stars that can be seen with the naked eye is not too unreasonable. William Herschel and Richard Proctor were not, however, to know that the absolute magnitude histogram was a reasonably-narrow quasi-Gaussian. This could not be confirmed until the distances of all the $m < 4^{th}$ magnitude stars had been obtained, in the early twentieth century.

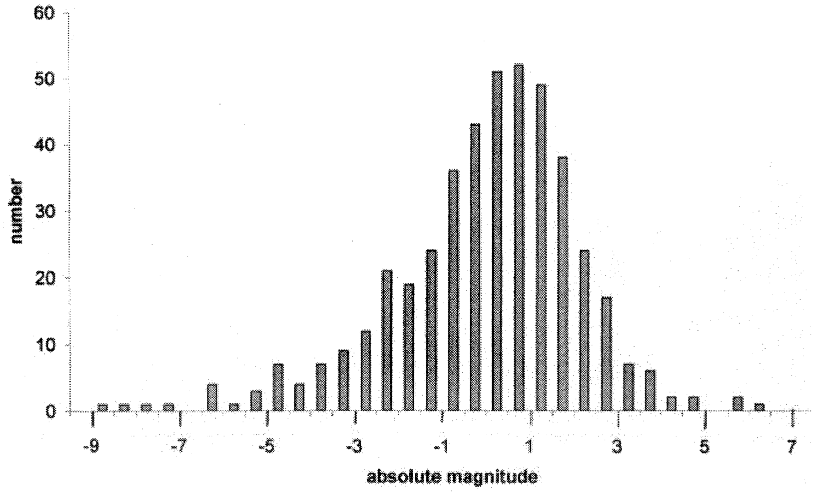

Figure 5. The histogram shows the distribution of the absolute magnitudes of the $m < 4$ naked eye stars, the stars being sorted into bins that are 0.5 magnitudes wide. The median absolute magnitude is about +0.29.

5 NUMBER VERSUS COLOUR

A crude estimation of a star's colour can be derived from the colour index. This is defined as being the difference between the apparent magnitudes of the star when observed through a blue filter, B, (centred on a wavelength of 4,400 Å with an effective bandwidth of 980 Å) and through a visual filter, V, (centred on 5,500 Å with an effective bandwidth of 890 Å).

There has been much debate about the ability of the naked eye to estimate the colour of a bright star with any degree of accuracy (e.g. see Cohen & Oliver, 1981; and Murdin, 1981). Murdin (1981) compared colour observations recorded by William Herschel, Admiral W H Smyth and F G W Struve and concluded that the colours were real but also somewhat subjective. He also found that the correlation between the results given by Minnaert (1940:300) and the modern colour index scale was excellent. This analysis is expanded on in Table 1.

It is quite clear that not all stars appear white, but it is also obvious that star colours are subtle. In Ptolemy's star catalogue, the *Almagest*, only six stars were marked as having distinctive colouration. These were α Boö (Arcturus, B–V = 1.24), α Tau (Aldebaran, B–V = 1.53), β Gem (Pollux, B–V = 1.00), α Sco (Antares, B–V = 1.83), α Ori (Betelgeuse, B–V = 1.86) and, somewhat surprisingly (see Malin & Murdin, 1984:88) α CMa (Sirius, B–V = –0.01). Ptolemy referred to all the stars as being 'hypokirros', a word that translates as 'somewhat yellow'. This term is considerably more conservative than the rather florid adjectives employed by Allen (1899), who had Arcturus as golden yellow, Aldebaran as pale rose, Pollux as orange, Antares as fiery red, Betelgeuse as orange, and Sirius as brilliant white.

Why Ptolemy only singled out six out of the 1,025 recorded stars is somewhat surprising. Cohen and Oliver (1981) note that only the cool stars of spectral classes K and M really show any colour other than white or blue-white. Two factors explain the small number of stars that are perceived to be coloured. First, only about 30% of the stars in Lang's (1991:168) list fall into the K and M classes, and secondly, the dark-adapted eye only records the brighter of the cool visible stars as being coloured. The typical first magnitude naked-eye star is in spectral class A, with a colour temperature of 10,000 K. Averaging over the fainter naked-eye stars gives a colour temperature of around 8,000 K, equivalent to a mid-F spectral class.

Two other things affect the perceived colour of a star. Star light is reddened due to scattering in Earth's atmosphere, and this reddening increases as the stars near the horizon. The fact that Sirius (declination –16°.6) and Antares (declination –26°) stay relatively close to the horizon for an observer in Rhodes might explain their perceived colour. The interstellar medium also reddens starlight, so the more distant stars of a specific spectral class are generally redder than the closer ones.

6 NUMBER VERSUS PROPER MOTION

Edmond Halley (1656–1742) discovered proper motion—that stars are actually moving and changing their relative positions, and therefore the shape of the constellations. After Halley the 'fixed' stars were fixed no more. As with many astronomical discoveries, Halley (1717) was actually looking for something else.

He was preparing his *Astronomical Tables* and redetermining the rate of precession of the equinoxes, mindful of the fact that the obliquity of the ecliptic was slowly decreasing. To this end he was comparing the declination of stars given in current catalogues with the declinations measured by Timocharis and Aristyllus in about 300 BC and Hipparchus around 130 BC and recorded by Ptolemy in the third chapter of the seventh book of the *Almagest*. The obliquity had changed by about 20 arc minutes since the time of Hipparchus. Three stars had not, however, followed the expected change in ecliptic latitude and longitude, these being Aldebaran (α Tauri, which was called Palilicium, or the Bulls Eye, by Halley), Sirius (α Canis Majoris) and Arcturus (α Boötes). To quote Halley (1717:737):

> ... all these three Stars are found to be above a degree more Southerly at this time than the Antients reckoned them ... What shall we say then? It is scarce credible that the Antients could be deceived in so plain a matter, three Observers confirming each other. Again these Stars being the most conspicuous in Heaven, are in all probability the nearest to the Earth, and if they have any particular Motion of their own, it is most likely to be perceived in them, which in so long a time as 1800 Years may shew it self by the alteration of their places, though it be utterly imperceptible in the space of a single Century of Years.

Table 1: An astronomical colour scale for bright naked eye stars based on the work of Minnaert (1940) and Murdin (1981)

Colour	Examples	Colour index range	Stellar temperature range (K)*	Number# %
White	α CMa (Sirius) α Lyr (Vega) α Vir (Spica) α Ori (Rigel)	<0.0	>10,000	21
White-yellow	α Leo (Regulus) β UMa (Merak) α CMi (Procyon) α Aql (Altair)	0.0–0.45	6500–10,000	25
Pale yellow	α UMi (Polaris) Sun [Venus, Jupiter]	0.45–0.75	5300–6500	7
Pure yellow [Saturn]	α Boö (Arcturus) α UMa (Dubhe)	0.75–1.0	4600–5300	13
Deep Yellow	β UMi (Kochab) β Gem (Pollux)	1.0–1.3	3950–4600	18
Orange-yellow	α Tau (Aldebaran)	1.3–1.65	3400–3950	14
Orange	α Sco (Antares) α Ori (Betelgeuse) [Mars]	>1.65	<3400	2

* The stellar temperature (K) has been obtained by using the empirical relationship $(B-V) = -0.865 + (8540/T)$.

\# The percentages have been obtained by analysing the 446 stars brighter than 4th magnitude given in Lang (1991)

Turning to, for example, Motz & Duveen (1977: 257) and Dibon-Smith (1992:35, 45,193), we find that Aldebaran, Sirius, and Arcturus have listed proper motions of 203, 1,324, and 2,285 arc seconds per millennium respectively. The latter two could certainly have been detected by Halley over the 1,800 years interval, but the proper motion of Aldebaran could not. Interestingly, Ronan (1969:201) lists the three moving stars as Arcturus, Procyon, and Sirius. Procyon (α Canis Minoris), with a proper motion of 1,247 arc seconds per millennium, is a much more likely inclusion as a member of the 'moving three'.

Figure 6 shows a histogram of the proper motions of the 100 brightest stars in the sky. Rigel Kentaurus (α Centauri) has the largest proper motion, this being 3,678 arc seconds per millennium. This very bright star would have been easily recorded by Halley from St Helena, but, with a declination of –60.4°, was much too far south to have been recorded in the *Almagest*. Figure 6 shows that 96% of the bright stars have proper motions <740 arc seconds per millennium, and this motion was too small to be detected by Halley. In Figure 7 we plot proper motion as a function of apparent magnitude for those stars with proper motions >400 arc seconds per millennium and with $m < 6$. It is clear that Halley's ability to discover proper motion was not only limited by stellar declination but also by stellar magnitude. Northern 'fast' stars such as 61 Cyg and μ Cas were too faint to have detectable proper motion, while β Hydri and ζ Tucanae were also too far south.

The fact that a few of the constellation-defining 'fixed' stars actually moved was a completely revolutionary and unexpected concept in the early eighteenth century. It provided an excellent reason for observing stars again and again, hopefully with ever-increasing accuracy. The fact that Halley had great faith in the data recorded in the *Almagest* is also noteworthy. He pioneered the use of ancient data in the quest for modern astronomical knowledge, his work on the variability of the salinity of the Mediterranean and the Earth-Moon distance being other examples.

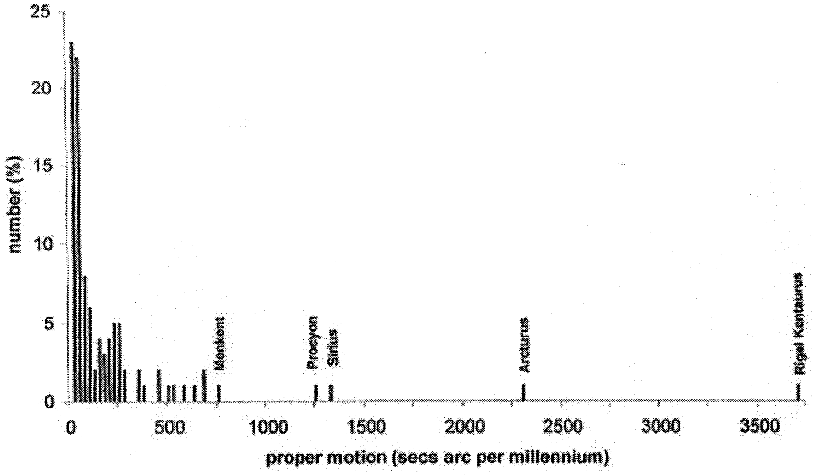

Figure 6 The histogram shows the distribution of the proper motions of the one hundred brightest stars in the sky. Rigel Kentaurus (α Cen, declination –60°.8) was too far south to be recorded by Hipparchus. Menkent (θ Cen, declination –36°.40) wasn't, but the proper motion of 738 arc seconds per millennium was too small for Halley to detect.

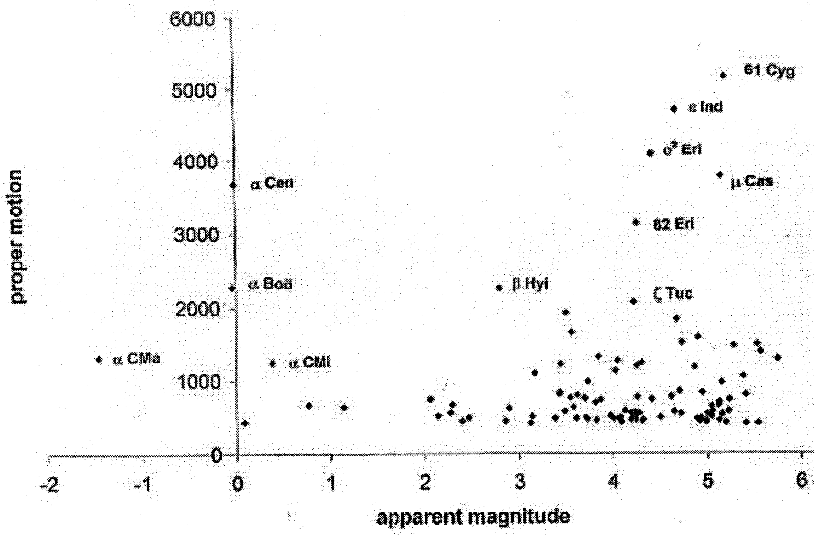

Figure 7. Naked-eye stars with proper motions >400 arc seconds per millennium have been selected from the data given in Dibon-Smith (1992) and the proper motion is plotted as a function of apparent magnitude. Some of the faster stars have been named.

Halley's suggestion of proper motion was soon followed up. In 1738, Jacques Cassini showed that Arcturus had moved by 5 arc minutes in 152 years (see Chambers, 1867:493). By 1783, William Herschel published a list of twenty-seven proper motions (all >200 arc seconds per millennium), and he used these to show that the Sun was moving in the direction of Lambda Herculis:

> That several of the fixed stars have a proper motion is now already so well confirmed, that it will admit of no further doubt. From the time this was first suspected by Dr Halley we have continued observations that shew Arcturus, Sirius, Aldebaran, Procyon, Castor, Rigel, Altair, and many more, to be actually in motion; and considering the shortness of the time we have had observations accurate enough for the purpose, we may rather wonder that we have already been able to find the motions of so many, than that we have not discovered the like alterations in all the rest. (Herschel, 1783:247).

Halley's suggestion that the brightest stars (being probably the closest) have the largest proper motion can be easily put to the test by referring to Figure 7. The relationship is not strong, to say the least. It was not long before accurate meridian-circle work enabled a few much faster and fainter stars to be found. Chambers (1867:493) lists the star 2151 Argus (proper motion 7,870 arc seconds per millennium), ε Indi (4696), 1830 Groombridge (6970, in Ursa Major), and 61 Cygni (5110). At the present time, the record for the largest proper motion is held by Barnard's Star, a 9.56 magnitude star in Ophiuchus that was discovered by the American astronomer, E E Barnard, in 1916. This star is only 1.8 pc away and is moving at 10,270 arc seconds per millennium.

7 RADIAL VELOCITY

In 1842 Christian Doppler (1803-53) noticed that the detected wavelength of a source varied as a function of the relative velocity between the observer and the source. The relationship is $\Delta\lambda/\lambda = v/c$, where λ is the wavelength emitted by the source and $\Delta\lambda$ is the change in wavelength due to the source moving at a velocity v with respect to the observer, c being the well-known velocity of light (299,793 km s^{-1}). The Doppler equation immediately opened up the prospect of being able to measure the line-of-sight velocities of astronomical objects, their wavelengths being red-shifted if they were moving away from the observer and blue-shifted if they were approaching. All one needs is a sensitive well-calibrated spectrometer. Doppler controversially and erroneously maintained that the different colours exhibited by the visible stars were indications of their large line-of-site relative velocities, and this hindered the acceptance of his theory for some time.

Lang (1991:168) lists the parameters of the four hundred and forty-six stars brighter than 4th visual magnitude, and the distribution of their radial velocities is shown in Figure 8. Two things are clear. The histogram is reasonably symmetrical about the zero velocity, showing that about as many bright stars are moving towards us as are moving away. And the maximum velocities are +99 km s^{-1} (δ Lep; declination –21°), +89 km s^{-1} (δ Col; declination –36°), +88 km s^{-1} (σ Pup; declination –43°), –87 km s^{-1} (η Cep; declination +62°), +75 km s^{-1} (α Phe; declination –42°), and –70 km s^{-1} (ζ Her; declination +32°). So the maximum value of $\Delta\lambda/\lambda$ for the visible bright stars is about 3×10^{-4}.

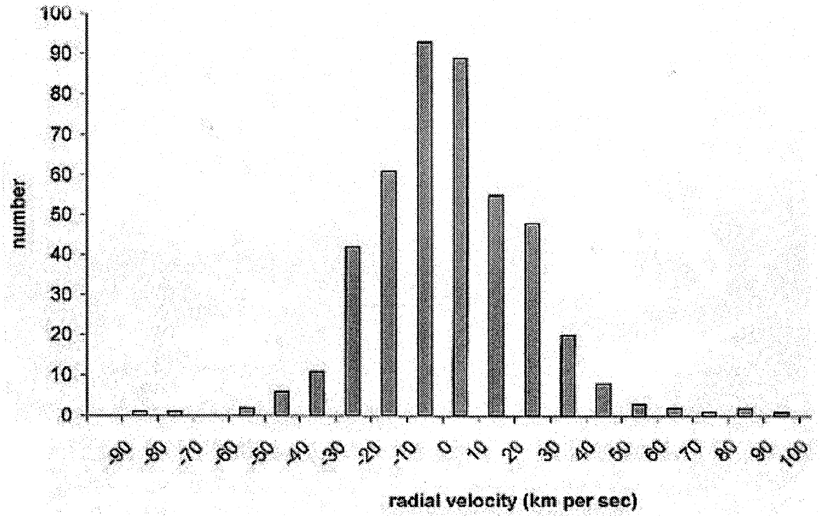

Figure 8. The histogram shows the distribution of radial velocities for the four hundred and forty-six stars with $m < 4$.

The median radial velocity of the $m <$ 4th magnitude stars (ignoring sign) is about 13.5 km s^{-1} making the median value of $\Delta\lambda/\lambda$ about 5×10^{-5}. Measuring this small wavelength shift was clearly not going to be easy.

What were the clues available to nineteenth-century astronomers to enable them to pick out the stars with large radial velocities? Figure 9 shows the modulus of the radial velocity plotted as a function of apparent magnitude. There is no obvious relationship. Figure 10 shows the modulus of the radial velocity plotted as a function of proper motion, but again the relationship is obscure. Considering the difficulty of measuring the wavelength shift, most astronomers took the simple expediency of concentrating on the brightest stars, that is those that would give the clearest spectra at the maximum spectral dispersion. To quote Agnes Clerke (1885:426), the method

> ... needs a powerfully dispersive spectroscope to show line-displacements of the minute order in question; and powerful dispersion involves a strictly proportionate enfeeblement of light. This, where the supply is already to a deplorable extent niggardly, can ill be afforded; and it ensues that the operation of determining a star's approach or recession is, even apart from atmospheric obstacles, an excessively delicate one.

William Huggins (1868) was the first astronomer to practically apply the Doppler relationship to the measurement of radial velocities. He used a twin-prism visual spectroscope attached to the 20.3-cm refractor sited at his private observatory at Tulse Hill in south London. He observed Sirius, not only because it was very bright but also because it had four strong Fraunhofer lines in its visual spectrum. Unfortunately visual results were only accurate to about ±25 km s^{-1}. In 1863, Huggins was also one of the first to photograph stellar spectra, using the wet-plate method, but it took some time before this technique was applied to radial velocity measurements. Henry Draper in the USA followed in 1872. In this same year Huggins published radial velocities for Rigel, Betelgeuse, Castor, Regulus, Arcturus, Vega, and Deneb. By 1890, the Royal Observatory at Greenwich had produced a list of thirty-four radial velocities, concentrating on bright constellation stars (see Chambers, 1890:373), but results were affected by serious systematic errors. Photographic observations of radial velocities started to be made in the late 1880s, and accurate values (to about ±3 km s^{-1}) were first obtained by H C Vogel and J Scheiner at Potsdam around 1887 for Sirius, Procyon, Rigel, and Arcturus. Young (1895:536) lists fifty-one radial velocities taken from the work of Vogel, these being for stars brighter than magnitude 2.4.

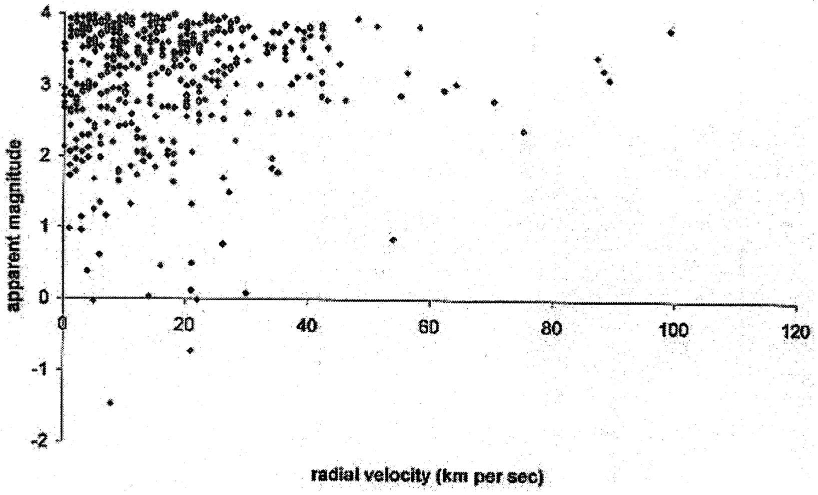

Figure 9. The radial velocity is plotted as a function of apparent magnitude for the four hundred and forty-six stars with $m < 4$. There is no obvious relationship between the two quantities.

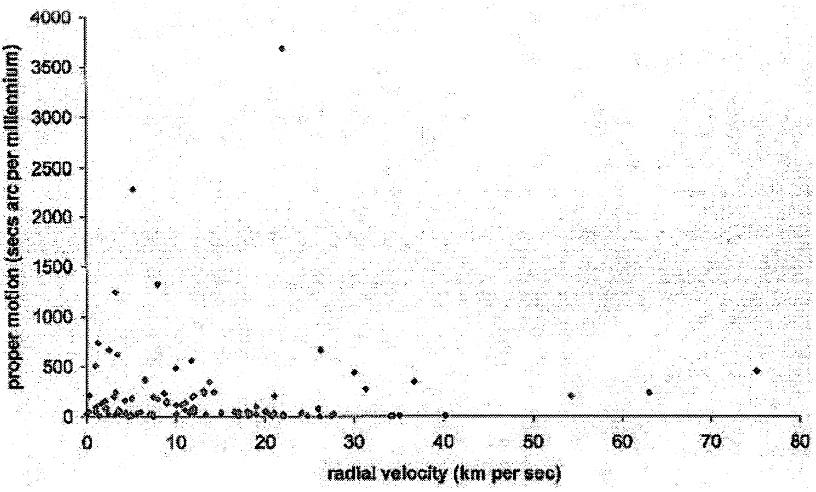

Figure 10. The proper motion of the one hundred brightest stars in the sky is plotted as a function of their radial velocity. If the stellar velocity vectors were inclined randomly to the line of sight one might expect a correlation between these two quantities. On the basis of the bright star data, this relationship is not convincing.

8 CONCLUSIONS

We have analysed the parameters of the bright stars that can be seen in the sky and we have discussed the relevance of these parameters to the rate of progress of historical astronomical research. Early astronomers were extremely lucky that the sensitivity of their eyes was such that they could detect only about 4,500 stars on the whole celestial sphere. Life was made even easier by the fact that only half of these were above the horizon at any one time. These stars clearly did not all have the same brightness, but the range was not huge, the brightest star Sirius (α Canis Majoris) being only about 950 times brighter than the faintest that could be seen. This limited range was such that it seemed reasonable to sort these stars into only six 'importance' categories. These were termed magnitudes and the well-trained eye could distinguish brightness differences of about ±0.1 magnitudes.

The magnitude distribution index was reasonably constant over the range of visible stars. Faint stars far outnumber bright ones. For example the whole sky contains about 3,100 stars with $5 < m < 6$ magnitude, the number dropping to nine hundred and eighty for $4 < m < 5$, three hundred and ten for $3 < m < 4$, ninety-eight for $2 < m < 3$, thirty-one for $1 < m < 2$, and about fourteen for $m < 1$. This greatly facilitated the problem of pattern production and recognition. The mapping of the sky into constellations was relatively easy, this task being greatly helped by the fact that the stars seemed to be distributed randomly across the sky, and not in a regular pattern.

The fact that the number of stars with magnitude m was about 3.2 times greater than the number of stars with magnitude $(m - 1)$ was not only useful, it was also reasonable. This constant factor suggested that the variation of star numbers with brightness might be mainly due to stars having similar luminosities but being at a whole range of different distances, as opposed to stars having a huge range of different intrinsic luminosities but all being about the same distance away. This basic assumption (i.e. similar luminosity) produced a workable cosmic model, and one that enabled greater degrees of complexity to be added later.

Another useful, but slightly less intuitive hypothesis that was proposed during the very early history of Greek astronomy (see, for example Dick, 1982), was that the visible stars in the night sky were nothing more than distant suns. The fact that these stars appeared as point sources of light, as opposed to the large solar disc, and that the constellation patterns of the stars did not perceptibly change from generation to generation meant that they were not just distant suns, but very very distant suns. Astronomers are extremely fortunate that the median distance to the bright, $m < 4$, stars that can be seen with the naked eye is about 39 pc, this being some eight million astronomical units, that is 1.2×10^{15} km. This fact, coupled with the fact the median modulus of the radial velocity is 13.5 km s^{-1}, explains why about 1,800 years had to elapse from the time of the production of the first reasonable stellar catalogue before someone realized that a few of the 'fixed' stars were actually moving.

The supposition that the stars we see are 'suns' was extremely useful during the dawn of astrophysics. Data gleaned from the detailed study of 'our' star could be transposed to its distant stellar neighbours. We must not forget, however, that the median $m < 4$ star has an absolute magnitude of about 0.29 making it about sixty-six times more luminous than the Sun and just over three times more massive. If we compare the median $m < 4$ star with the median member of the *actual* stellar population of the galactic disc the difference is even more stark. Analysis of the data for the one hundred and twelve known stars closer than twenty light years, 6.13 pc[1] indicates that they have a median absolute visual magnitude of about 11.94. The median $m < 4$ star is 46,000 times more luminous than this.

Notice also that the stars we can see with the naked eye appear as single stars. In reality many stars are binaries. There are one hundred and twelve known stars within twenty light years of Earth, and fifty-four of these are single stars, thirty-six form eighteen binaries, eighteen form six triplets, and there is one quadruple group. The average distance between the seventy-nine 'single stars plus groups' is about 2.3 pc. Note that, out of the one hundred and twelve nearby stars only eight fall into the $m < 4.0$ group, these being, in order of distance, the Sun, α Cen, α CMa, ε Eri, α CMi, τ Cet, α Aql, and η Cas. The notion that the visible stars could be other than single Sun-like entities is relatively recent. Telescopic observers of the sky noticed that many stars seemed to be rather close together. In 1767 the Reverend John Mitchell applied statistics to the problem. He calculated that the odds of the six brightest stars in the Pleiades having their observed proximity, if the 1,500 stars of their magnitude had been randomly scattered onto the celestial sphere, was about 500,000:1. The fact that the Pleiades was an actual physical group was confirmed in 1846 when J H Mädler discovered that the Pleiades stars had a common proper motion. In 1779 William Herschel started listing 'double stars' in the hope that their observation throughout the year might lead to a measurement of parallax. A list of two hundred and sixty-nine was produced in 1782. Michell (1784) was firmly convinced that many of Herschel's double stars were actually two stars orbiting a common centre of mass under the influence of Newton's laws of gravitation, as opposed to two stars observed by chance in nearly the same direction. Soon the term 'binary' star was being used for a gravitationally-bound pair.

The concept of randomness is also relatively recent and dates from the time of mathematical physicists such as Siméon-Denis Poisson (1781–1840) who published a book on probabilities in 1837. If stars have been scattered on the celestial sphere in a random fashion and you expect to find λ stars in a specific area of the sky, then the chance P_β of finding β stars in that area is given by $P_\beta = \lambda^\beta e^{-\lambda}/\beta!$. Let us, for example, illustrate the effect of randomness by looking at the bright stars along the celestial equator. This region of the celestial sphere has been divided into forty-eight equal areas, each of one hundred and forty-seven square degrees. These areas are one hour of right ascension across and extend from the equator either up to a declination of +20° or down to a declination of –20°. A count is then made of the number of stars brighter than an apparent magnitude of 4.5 in each area. The average number turns out to be 6.7. The way in which the actual number varies about this average is shown in Figure 11 together with the expected Poissonian distribution if randomness is assumed. Forty-five out of the forty-eight regions obey the Poissonian statistics very well. Three regions contain, however, thirteen, eighteen, and twenty stars, way above the average value of 6.7. The existence of the latter two is way beyond the expectation of a random distribution and this unusual feature of the sky should have been noted by past astronomers. Interestingly, the region with eighteen stars contains the Hyades and the right arm of Orion, and the region with twenty-one stars contains Orion's belt (i.e. 5hr < RA < 6 hr; $0° < \delta < -20°$). The 'thirteen-star' region is the upper part of the constellation of Orion. The ancient astronomers certainly commented on the fact that the majority of stars seemed to be scattered sporadically but they also noted a few 'clusters'. The Pleiades and the cluster of stars near Algenib (α Per) were obvious.

9 NOTES

1. See, for example, the following web site: http://www.anzwers.org/free/universe/nearstar.html

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The discovery of Juno and its effect on Olbers' asteroid explosion hypothesis[1]

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Abstract
In 1806, two years after the discovery of Juno, the asteroid explosion hypothesis made famous by Wilhelm Olbers was effectively demolished by the Swedish astronomer Lars Regner. His work has received virtually no attention, likely because it was written as a poorly-circulated pamphlet in Latin. This paper looks at both Olbers' and Regner's reaction to the discovery of Juno.

Keywords: asteroid, planet, explosion

1 INTRODUCTION

The year 2004 marks the two hundredth anniversary of the discovery of the third asteroid Juno. After the discovery of Ceres in 1801 and Pallas in 1802, many astronomers expected to find additional small planets between the orbits of Mars and Jupiter, so the finding of a third member of the group was no surprise. The main historical interest in the discovery of Juno lies in its impact on the validity of a hypothesis promoted by Wilhelm Olbers (discoverer of Pallas) to explain the existence of Ceres and Pallas.

2 OLBERS' HYPOTHESIS

Within weeks of the discovery of the second asteroid, Pallas, on 1802 March 28, Olbers had formulated a hypothesis to explain the existence of the asteroids. Recent archival research has revealed that Olbers got the idea from his friend and amateur astronomer Ferdinand von Ende. On 1802 April 6, Ende wrote to Olbers suggesting that the "two small planets had formed a bigger one; at least a comet shock (impact) is not more unlikely than throwing a comet against the Sun causing the planets to splinter off." (von Ende, 1802). Olbers never credited Ende with the original idea. He broached the concept to Carl Gauss in a letter of 1802 April 23 and on May 17 he wrote about it with some trepidation to William Herschel in England:

> The similarity in the period of their revolution, of their long axes, and the remarkable position of both orbits in relation to each other, have suggested to me an idea which I hardly dare to put forward as a hypothesis, and about which I should much like to have your, for me, weighty opinion. I mention it to you in confidence. How might it be, if Ceres and Pallas were just a pair of fragments, or portions of a once greater planet which at one time occupied its proper place between Mars and Jupiter, and was in size more analogous to the other planets, and perhaps millions of years ago, had, either through the impact of a comet, or from an internal explosion, burst into pieces? I repeat that I give this idea as nothing more than, hardly as much as, a hypothesis. (Olbers, 1802).

3 OLBERS' REACTION TO THE DISCOVERY OF JUNO

Less than a month after the discovery of Juno on 1804 September 1, Olbers was ready to abandon his hypothesis, as he relates in a letter to Gauss on September 30:

> The fact that in all probability Juno's orbit will also have the same orbital period and major axis as that of Ceres and Pallas, appears to me at least to totally topple my theory. This fact was questionable already with Ceres and Pallas, but could have been coincidental. However, since it is now also confirmed by the 3 asteroids, then one must reject a theory which not only doesn't explain precisely this curious situation but rather contradicts it. The disintegration of a planet would have necessarily imparted very different velocities to the various fragments. These new velocities must have been considerably influenced by the former tangential velocity because the orbits, considering their eccentricities and inclinations, differ so much from each other. (Olbers, 1804).

Upon sober reflection, Olbers reversed himself. In a letter to Baron von Zach in October (published in the November issue of the *Monthly Correspondence*), Olbers does his best to save his now beleaguered theory by invoking the aid of Jupiter.

> The entire situation of Juno's path has nothing which would not be compatible with my hypothesis (which, by the way, I do not wish to pass off as anything more than a hypothesis). Its nodes with the path of Ceres fall some 24 degrees from the node of the path of Pallas. But with the inclinations of the paths that differ so greatly, the nodes must move non-uniformly through the force of attraction of Jupiter. Presently, in its descending node, Juno's path lies on the path of Ceres, to which the path of Pallas is very close, far within the path of Ceres. But since the aphelia of all these paths have a very different movement than the nodes and the positions of the apsides-lines therefore always change against the nodes, and since these paths have almost the same major axes but very dissimilar eccentricities, it follows that these paths will intersect at certain times and will have done so in the past. (Zach, 1804)

4 REGNER'S ATTACK ON THE HYPOTHESIS

In 1806, Lars Regner, professor of astronomy at Uppsala University, launched a broadside against Olbers' hypothesis. The complete Latin text and

English translation of his treatise can be found in Cunningham (2004). At the beginning of his treatise, Regner expresses astonishment that the hypothesis was being seriously considered. It was, he thought, mentioned "purely for the sake of a joke."

Regner states that the hypothesis may be very easily tested because, if true, all of the fragments of the explosion "should penetrate the descending node of the orbit of Pallas in the orbit of Ceres." The discovery of Juno, Regner says, has now provided the means to test the hypothesis. He uses two main arguments.

(1) *The Major axis*: Regner claims that if the hypothesis is correct, the major axis of the orbit of Juno would surpass the major axes of Ceres and Pallas. "In fact, however, the observations indicate it to be somewhat smaller than these: and what is more, by the same reasoning the periodic times of these bodies would be dissimilar, and yet we know that they differ very little from each other."

(2) *The Eccentricity:* Based on the great difference in the perihelia of Juno and Ceres, Regner considers the difference in eccentricity. "If these two bodies were projected into space from the same location and with the same speed, the eccentricity of the orbit of Juno would be about three times greater than the eccentricity of the orbit of Pallas; in fact, the observations show that it is only greater by 0.019".

Regner is scathing in his conclusion:

> Therefore, it is now proven by these facts, if the descending nodes of these three orbits also were to assemble in one and the same moment, or if it were possible to be demonstrated that they had once assembled – which is the chief principle of Olbers' hypothesis – thence it still ought not to be concluded in any way that they were of the same family, that these three named planets were the thrust out pieces of some greater planet, unless indeed it were shown that their masses were equal and the eccentricities of their orbits were in the ratio of the mutually distant perihelia. And so it seems thoroughly amazing to us how, despite all of the arguments, and indeed every appearance of its likelihood stripped away, this opinion was able to thrive for just under four years now, and not only blindly commended by the foremost astronomers of our time, but also, as a portent of its ingenuity, to be extolled with the loftiest praises. Now, even if we are plainly lacking valid reasons, which we have already used to vanquish Olbers' hypothesis, nonetheless it will justly remain absurd.

5 CONCLUSION

Even though Regner did not address himself to the suggestion that Jupiter's gravity caused the nodes of the asteroids to move in a non-uniform way, his refutation of the hypothesis was conclusive. It did not, however, end the controversy. In fact, Regner's work was not referred to by any other writer on the subject, but by mid-century most agreed with the verdict of Walter Mitchell (1866): "The destruction of Olbers' planet is generally consigned to the limbo of hypothesis, as no better than a mere philosophical dream."

6 ACKNOWLEDGEMENTS

I wish to thank Christopher Gordon from the University of Toronto for his Latin translation of Regner's treatise.

7 NOTES

1. A German-language version of this paper was presented at the Juno 200 conference in Bremen, Germany in 2004.

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Clifford Cunningham is contributing editor of *Mercury* magazine; his Annals of Astronomy column has been appearing there since 2001. He has written five books: *Introduction to Asteroids* (1988); *The First Asteroid* (2001); *Jousting for Celestial Glory* (2004); *The Collected Correspondence of Baron Franz von Zach*, Volume 1 (2004); and *The Origin of the Asteroids* (2004). A graduate of the University of Waterloo, Canada in both physics and ancient history, his articles have appeared in many magazines and journals including *Annals of Science*, *The Canadian Aeronautics and Space Institute*, *Sky & Telescope*, *Spaceflight*, and *Space Education*. He writes the Spaceflight section for the annual publication Astronomical Calendar. In 1990, asteroid 4276 was named Clifford in his honour by the Minor Planet Center.

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The 1948 solar eclipse and the genesis of radio astronomy in Victoria

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Abstract
In the early years of radio astronomy, solar eclipses played a critical role in establishing the sources of solar radio emission. During the half decade from 1945, Australia emerged as a leading nation in radio astronomy, with most of the observations made at sites that were concentrated in and around Sydney. Radio astronomy in the state of Victoria was launched when a small group of Sydney scientists successfully observed the partial solar eclipse of 1948 November 1 from Rockbank, near Melbourne.

Keywords: *Solar radio astronomy*, *solar eclipses*, *Victoria*

1 INTRODUCTION

Although radio astronomy has a history that extends back a little over seventy years, it only blossomed following the development of radar in WWII. Young radio engineers from the CSIRO's Division of Radiophysics (henceforth RP) in Sydney carried out the first investigation of solar radio emission towards the end of 1945, and this was soon followed by studies of galactic and extragalactic sources (Sullivan, 1988).

Two years after the end of WWII, Australia was one of the world leaders in radio astronomy, and most of the observations were made at RP field stations in suburban Sydney (see Orchiston and Slee, 2005). However, there were also small research teams at Mount Stromlo Observatory near Canberra and at the University of Western Australia in Perth, and a solar eclipse in 1948 November prompted the RP group to establish temporary observing stations at Rockbank in Victoria and Strahan in Tasmania. These were the earliest radio astronomical investigations undertaken in these two Australian states, and this paper describes the instrumentation used at Rockbank, the observations, and their interpretation.

2 SOLAR EMISSION AND THE ECLIPSE OF 1948 NOVEMBER 1

By 1948, radio emission from the Sun was known to comprise three distinct components: (1) thermal emission from the 'quiet' Sun, (2) on-going enhanced emission associated, in general, with optically-active regions, including sunspots, and (3) intense short-term burst emission that was also associated with optically-active regions. Because of the poor resolving power of radio telescopes at this time, it was difficult to establish the *precise* relationship between photospheric and chromospheric features observed optical and radio-emitting regions, and one approach to this dilemma was to monitor the change in radio emission recorded in the course of a solar eclipse. In 1947, Covington showed that fluctuations in the level of solar emission as the Moon proceeded across the Sun during the 1946 November 23 partial solar eclipse were correlated with the masking and reappearance of different sunspot groups.

The partial eclipse of 1948 November 1 provided Australian radio astronomers with their first opportunity to carry out such an investigation, and staff from RP planned radio observations at 9,400, 3,000 and 600 MHz (Christiansen et al., 1949a, 1949b; Minnett and Labrum, 1950; Piddington and Hindman, 1949), and photographic coverage with the 15-cm (6-in) Cooke guide scope attached to the 45.7-cm (18-in.) Hoskins reflector at Sydney Technical College (see Figure 1).

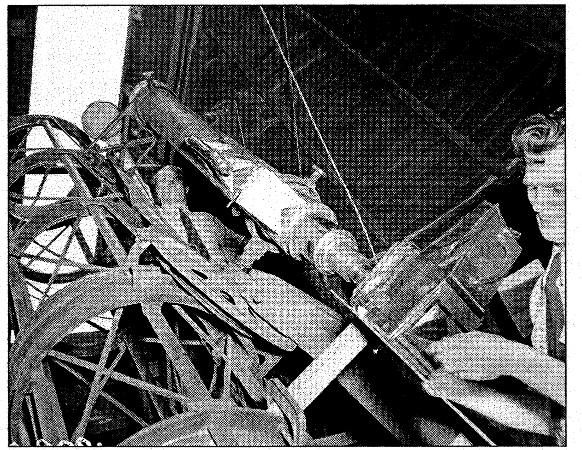

Figure 1. The 45.7-cm reflector at Sydney Technical College, and the 15-cm guide scope that was used for photographic monitoring of the 1 November 1948 partial solar eclipse (ATNF Historic Photographic Archive: B1899-7).

There were three clear research objectives associated with the 600 MHz observations:

(1) To determine the precise positions of enhanced regions of solar emission in the corona. It was reasoned that this should be possible by using three widely-spaced radio telescopes and knowing the path of the Moon's shadow across the solar disk at each site during the eclipse;
(2) To determine whether limb-brightening existed at a frequency of 600 MHz, as postulated by Martyn (1946); and
(3) To determine whether radio-emitting regions in the northern and southern hemispheres of the Sun exhibited opposite senses of circular polarization, as also predicted by Martyn.

Two different types of antennas were used for the 600 MHz observations: a former experimental radar antenna located at the Potts Hill field station in Sydney, and two simple 3-m (10-ft) diameter altazimuth-mounted parabolic antennas at the Rockbank and Strahan observing sites. Figure 2 shows one of these two antennas undergoing testing at the Georges Heights field station (see Orchiston, 2004) prior to the eclipse.

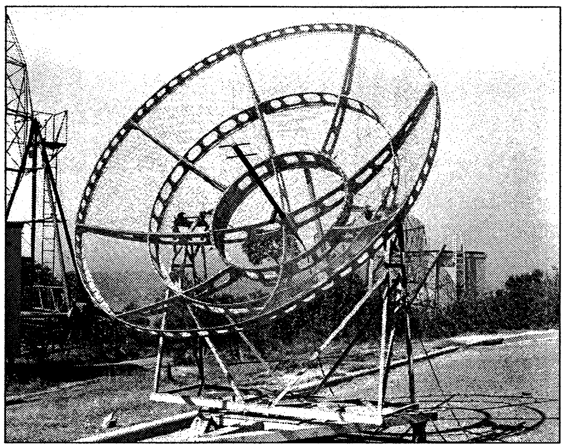

Figure 2. The Rockbank or Strahan portable radio telescope at Georges Heights on 31 August 1948 (ATNF Historic Photographic Archive: B1511)

The custom-made wire-mesh dish had a beam width of 15°, and featured crossed dipoles, as required for the polarization measurements (but note that these are not shown in Figure 2). The receiver consisted of

> … a quarter-wave transmission-type cavity-resonator, followed by a conventional crystal converter, 30 Mc/s. intermediate-frequency amplifier, and diode second detector … After rectification, the signals passed into a D.C. amplifier which was connected to a recording milliammeter.
>
> With the small aerials used at Rockbank and Strahan, the ratio of signal power from the uneclipsed sun to the internal noise power at the receiver input was between 0.10 and 0.15, so that great care was required for accurate measurement of the power flux density from the sun during the course of the eclipse. (Christiansen *et al.*, 1949a: 508).

3 THE ROCKBANK OBSERVATIONS

At Rockbank, the eclipse began at 1639 local time, maximum phase was reached at 1741 when 72% of the Sun disk was covered, and the event ended at 1838, just sixteen minutes before sunset (Christiansen, Yabsley and Mills, 1949a: Table 1). Photographs taken in Sydney on the day revealed the presence of six groups of sunspots, but their total area was small, amounting to only ~0.085% of the total area of the visible disk of the Sun. A table listing the elevation and azimuth of the Sun was used initially to point the antenna at the Sun, and it was then moved manually every few minutes. Tracking sessions using this technique on days prior to the eclipse showed that errors in radio intensity measurements caused by slight misalignment of the antenna would amount to less than 1%.

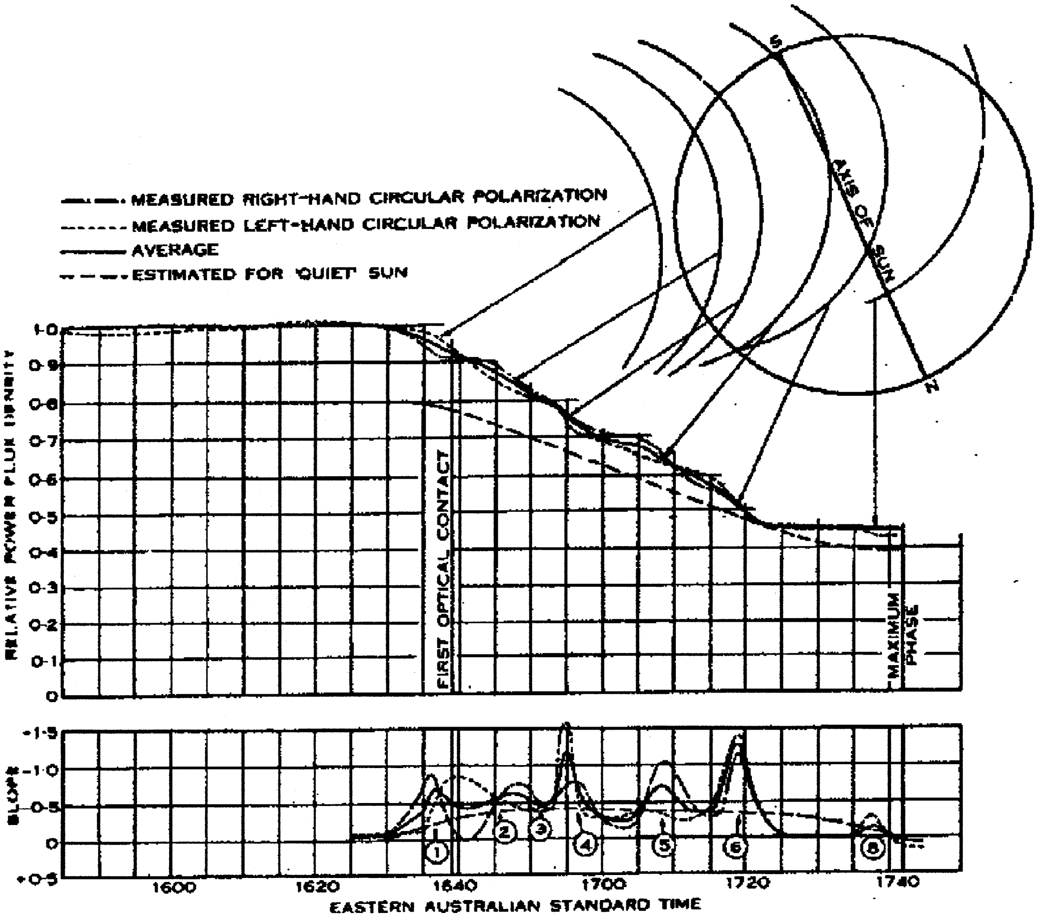

Figure 3. Observation of the eclipse at Rockbank (after Christiansen et al., 1949a: 511).

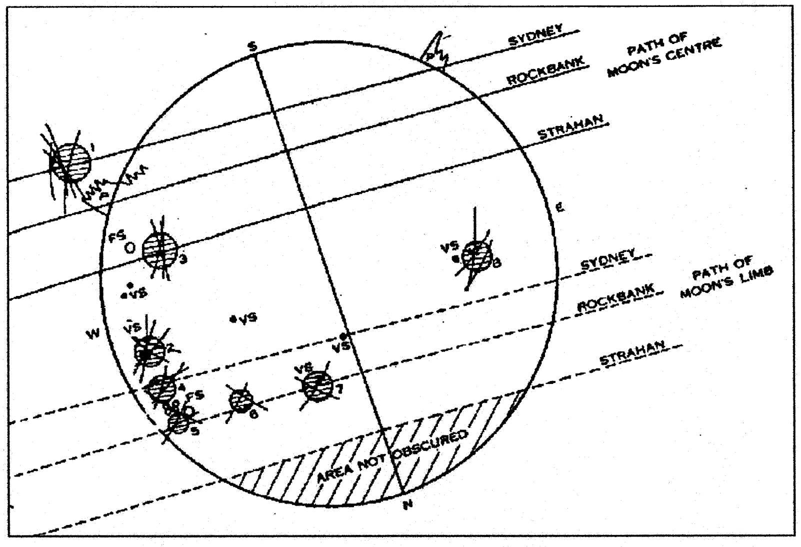

Figure 4. Regions of enhanced solar radio emission at 600 MHz (hatched circles) on 1948 November 1, and sunspots (VS), locations where sunspots were present on the previous solar rotation (FS) and prominences (P) (after Christiansen *et al.*, 1949a: 513).

Figure 3 shows the variation in radio emission received at Rockbank during the first half of the eclipse. Although the plotted curve is unbroken, the original record was discontinuous because the aerial was directed away from the Sun at frequent intervals to calibrate the receiver, and the polarization of the aerial was changed periodically, resulting in a disrupted record of each sense of polarization. The missing segments were interpolated, and it was estimated that "No serious error was likely to be caused by this procedure because the changes were made at close intervals." (Christiansen *et al.*:511). The upper plot in Figure 3 shows the actual trace, while the lower plot has been corrected for the slope and the peaks have been magnified. Both plots show the estimated level of emission from the quiet Sun. The circular arcs at the top right show the positions of the Moon at the occurrence times of the peaks in the lower curve.

The upper curve in Figure 3 shows that radio emission from the Sun began to decrease ~10 minutes before the commencement of the optical event, consistent with the idea that the 600 MHz radio emission originated in the corona. As the eclipse progressed, the declining emission curve was punctuated by a number of minor troughs, best seen in the lower plot. These represented masked localized regions of enhanced solar emission, and their precise positions—projected onto the solar disk—were obtained by plotting the intersections of the appropriate eclipse arcs at the Sydney, Rockbank and Strahan observing sites. These locations are shown in Figure 4, where the numbers correspond to the peaks in the lower plot in Figure 3.

4 ANALYSIS

Calculations showed that the eight localized regions of enhanced emission contributed ~20% of the total solar radiation received on 1948 November 1. These emitting regions were assumed to be approximately circular, and their areas varied by little more than a factor of two, with a mean of ~0.4% of the total area of the visible disk of the Sun. Their effective temperatures varied by more than 10:1, and if we assume a quiet Sun temperature of ~0.5×10^6 K at 600 MHz, then the brightest localized regions in Figure 4 (numbers 4 and 6) would have had effective temperatures of ~10^7 K.

Figure 4 shows that peak number 1 was located ~1.7×10^5 km beyond the solar limb, and above a magnetically-active region in the chromosphere marked by a conspicuous prominence. All other emission peaks were on the solar disk, and in the case of numbers 2, 7, and 8 coincided with sunspot groups. However, peaks 3–6 did not appear to be associated with any obvious photospheric features, although three of these were close to the positions occupied by sunspots groups exactly one solar rotation earlier. Meanwhile, two small sunspots groups in Figure 3 and one large group (near the western limb) were not associated with measurable levels of enhanced solar radio emission.

The second research objective related to possible limb brightening at 600 MHz, and the results were inconclusive:

> … roughly half the (presumed) thermal component of the radiation originated close to, and predominantly outside, the edge of the visible disk of the sun. The details of the brightness distribution could not be derived from the records. The latter were shown to be consistent with two tentative distributions, the first a theoretical one, involving limb brightening … and the second a uniform one over a disk having 1.3 times the diameter of the optical disk of the sun. The existence of limb brightening, therefore, was not proved. (Christiansen *et al.*, 1949b:570).

The polarization analysis proved interesting in that Rockbank was the only site to provide relevant data. Before the eclipse the two modes of circular polarization differed in amplitude by less than 2%, but on 1948 November 1, "The eclipsing of the active areas produced changes that sometimes were confined to one or other circularly-polarized component, or in some cases involved both components." (Christiansen *et al.*, 1949a:521). The changes were of short duration, and the two components quickly returned to equality. This is illustrated in Figure 3, where the most significant variations in the relative levels of left-hand and right-hand circular polarization are associated with active regions 1, 4 and 5. Since the difference in the two polarizations curves was <3% at the maximum phase of the eclipse, this indicated that the general magnetic field strength of the Sun at the poles was <8 gauss. We should note that this is in line with current thinking, but that in 1948 a value of ~50 gauss was assumed.

5 CONCLUDING REMARKS

The Rockbank observations of the 1948 November 1 partial solar eclipse led to a greater understanding of the relationship between optical solar features and areas of enhanced radio emission, and a summary of the overall project was published in *Nature* in 1949 (Christiansen *et al.*, 1949b), with the full account appearing that same year in the *Australian Journal of Scientific Research* (Christiansen *et al.*, 1949a). This pioneering study was the first radio astronomical research project carried out in the state of Victoria.

From a national and international perspective, the Rockbank, Strahan and Sydney observations of the 1948 eclipse marked a watershed in solar radio astronomy, in that they were the trigger that inspired W.N. Christiansen (1984:117) "... to devise some method of viewing the Sun [at high resolution] more frequently than was possible with eclipse observations. This of course meant devising some antenna system of very great directivity." The result was the first solar grating array, an innovative 32-element interferometer operating at 21cm that was constructed at the Potts Hill field station in Sydney during 1951 (see Christiansen and Warburton, 1953).

6 ACKNOWLEDGEMENTS

I am grateful to Drs Don McLean and Bruce Slee (Australia Telescope National Facility) for reading and commenting on the manuscript, and to the Australia Telescope National Facility for supplying Figures 1 and 2.

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Reviews

Misfortunes as Blessings in Disguise, the Story of My Life, by Dorrit Hoffleit (The American Association of Variable Star Observers, Cambridge, Massachusetts, 2002). 176 pp., ISBN 1 878 17448 7 hardback. $25, £15.

The title of this book might lead a reader to expect a self-help guide or a collection of moral tales. In fact, it is something much more inspiring—the autobiography of a distinguished astronomer who at the age of 96 is still active in the science which she has loved and served for three quarters of a century.

Dorrit Hoffleit is well known to two (or three!) generations of astronomers for her prolific contributions to spectral classification, variable stars, stellar photometry, and astrometry. Her official professional life was divided down the middle—the first half at Harvard College Observatory, the second shared between the Maria Mitchell Observatory in Nantucket and Yale University Observatory. There is also a third phase—her ongoing attachment since formal retirement to her old department at Yale, which she regards as the happiest. She also remains deeply committed to the Association of Variable Star Observers to which she has belonged throughout her astronomical life.

Dr Hoffleit was born in 1907 of German parents who had emigrated to the United States as young adults in quest of a better life. Her hardworking father was employed as a bookkeeper on the railroad; her talented mother, daughter of a Physics teacher, had studied music in the conservatoire. Their home, modest by worldly standards, provided an intellectual environment of books and music for Dorrit and her brother, her only sibling, who became a Classics scholar.

Dorrit regarded herself as a very ordinary pupil at school, and received little encouragement from her mother who believed it more appropriate for a lady to choose "fine embroidery, music and modern foreign languages." Nevertheless, she reached university and graduated with honours in mathematics from Radcliffe College at the age of 21 in 1928. Her ambition was to be a high-school mathematics teacher, but posts were scarce, and she accepted employment instead at Harvard College Observatory where the Director could "hire two women for the price of one man." Far from complaining at this turn of events, Dorrit regarded it as her first "blessing in disguise."

At Harvard she found life under the Director, Harlow Shapley, "delightful", and never forgot the encouragement she received from him in those early days: her Memoirs are dedicated to his memory. Tutored by Henrietta Swope, her papers on variable stars and spectroscopy soon appeared in increasing numbers in the Harvard Bulletins. She was overwhelmed when Shapley and Bart Bok suggested that she embark on a Ph.D., something that, in her modesty, had never occurred to her. She obtained her Doctorate in 1938 and was promoted to the rank of Astronomer in 1948. Her work was interrupted by the second World War when she was seconded to do research in ballistics (about which she published a number of papers). After Shapley's retirement in 1952, Harvard lost much of its magic for her, and she moved in 1957 to Nantucket to become Director of the Maria Mitchell Observatory as successor to Margaret Harwood who was due to retire. There followed a fraught few years while Miss Harwood pleaded for time to finish her research project, and even resisted retirement altogether. Here was another misfortune to be turned into a blessing. By special arrangement, Dorrit was able to divide her time between Nantucket and Yale, where she was given an appointment in that observatory's astrometry programme, with charge, among others, of that indispensable *opus*, the Yale Bright Star Catalogue, with which her name will ever be associated. She retired from Maria Mitchell Observatory in 1978, but was granted the privilege of continuing to work to her own liking in the Yale Department of Astronomy.

The most interesting chapters in the book deal with her work at the Maria Mitchell Observatory—named after America's first women astronomer—where she instituted a summer course of instruction for women students (men later took part) who were given original material on variable stars to work on. Students came from far and wide; many went on to careers in astronomy, or became active members of the American Association of Variable Star Observers. All remained her cherished friends. Among them was the late Janet Akyüz Mattei, who persuaded her to write these memoirs, and who has provided a charming Preface.

Dr Hoffleit was in due course rewarded with many academic honours, including Honorary Doctorates and an asteroid Dorrit, named on her 80th birthday. She found happiness in her private life from her students and colleagues, into whose families she was ever welcome: sixteen pages of fascinating photographs illustrate her intertwined personal and professional lives. Her account modestly refrains from parading her own contributions to science. These speak for themselves in an appended bibliography of 450 publications spanning a period of over seventy years. Readers of this *Journal* will be pleased to find among them numerous contributions to the history of astronomy—essays, book reviews, and entries in international and American biographical dictionaries.

This very readable book is a personal story told with good humour and charity, of one who made the most of life as she found it, working hard and loyally, and achieving success and fulfilment in the service of Urania and of her fellow human citizens.

Mary Brück

Shining in the Ancient Sea, by Laurin R Johnson. (Portland, Oregon: Ash Creek Press, 1999), 137 + ixx pp., ISBN 0-9669828-0-0, soft cover, US$20.00, 228 × 140 mm.

Homer's two epic works *Iliad* and *Odyssey* are among the most important creation of our cultural heritage. Their influence in all levels of the human activities has never faded in the three thousand years—or more—of their existence. They belong to the most translated and read books of the world. Scientists of several different disciplines have written thousands of pages trying to describe the beauty and analyse the origin and meaning of their contents.

The present book is an attempt to expound *Odyssey* as an ancient astronomical almanac. It contends that the voyage of Odysseus took place in the night sky among the constellations. Its author seems to know the Vedic texts —something that the writer of this text ignores. He argues on the basis of the widely-accepted theory of the common origin of the Indo-European culture. According to the book, an astronomical almanac was created in the framework of the Indo-European parent culture and took the form of a poem formed around 3500 BC; the aim was to transmit astronomic knowledge to the next generations easily via the poetic rhythm and prosody. Some of the Indo-Europeans would then move to India and create their new culture there, in the heritage of which we would find the Vedic texts. Other Indo-Europeans would move to Europe and, in particular, some of them to Greece, where the original astronomical almanac was transformed to *Odyssey*. A critical element of this metamorphosis was the influence of future generations, that slowly added new elements—these of the new Greek culture—to the original astronomical song.

The author argues that several strophes in Vedic texts correspond to verses in *Odyssey*, and that the metaphors used by the two different texts show clearly the astronomical meaning of the initial almanac. It could be. Also, the book correlates a constellation with each place Odysseus visited during his ten-year journey home, giving another poetic dimension to *Odyssey*. Odysseus was finally a planet, probably Saturn.

There are quite a few interesting remarks in this book, especially in Chapter Two, according to which the time when *Odyssey* started to be created should be "about 2750 (give or take a few hundred years)". In one of these remarks one reads, on pages 48-49, that "... the only reasonable explanation for the fact that the Pleiades were chosen as the beginning point of a continuous circle rather than any of the other 27 asterisms must be that the Pleiades marked the beginning of the year ... The Pleiades did once serve this function between 3000 BC and 2200 BC." Furthermore, the author argues that the beginning of the creation of *Odyssey* could even go back to 4000 BC. Actually, this hypothesis, elaborated on also in other works by many researchers, cannot be rejected.

There is also an exciting idea in the background of the book. We know that early cultures created huge astronomical observatories, even during the pre-historic era. We still admire the remnants of these astronomical sites for their architectural symmetry, as well as the astronomical capacity of their astronomers. We know that astronomers were often priests, and the astronomical orientation of temples and churches is an additional indication of the old traditional link between astronomy and religion. Astronomers and priests worked in these ancient observatories, which often were temples as well. Religion preserved tradition as too did poetry. The suggestion that one of the most marvelous poems in history is in fact an astronomical almanac in which ancient priests and astronomers put their observations is a highly fascinating speculation.

Yet, the book does not deliver any proof of this tempting hypothesis. The arguments are poor, and the correlation of constellations with places in *Odyssey* are not convincing. The task of the author to find persuading arguments was surely not an easy one. Nevertheless, a reader who knows the night sky, has some elementary astronomical background, and loves good poetry will find in the pages of this book plenty of charm—or example by looking at the Hyades and seeing a group of pigs there (Chapter Three, page 70), or at the *Pleiades* as the floating Homeric island *Aeolia* (Chapter Four, page 79). Nonetheless, no convincing arguments in the book support the scientific correctness of the reasoning; therefore the reader's fascination should remain at the poetic and artistic level.

Dimitris Sinachopoulos

Mary Somerville and the World of Science, by Allan Chapman (Canopus Publishing Limited: Bristol 2004), 157pp + XV, ISBN 0 9537868 4 6, hardback. £12.95. 17.5 × 12.0 mm.

Mary Somerville (1780–1872) was one of the most celebrated and most influential *interpreters* (as distinct from *popularizers*) of science of her era. In this study of that extraordinarily gifted woman, Allan Chapman recounts how his interest in her began when he was researching *The Victorian Amateur Astronomer, Independent Astronomical Research in Britain 1820-1920* (1), his invaluable compendium of the personalities who inhabited that period in various capacities. Until late in the nineteenth century, Britain, unlike France and Germany where scientific work was state-sponsored and hierarchically-organized, had very few salaried posts in astronomy (apart from the Royal Observatory at Greenwich). Research was largely in the hands of self-supported individuals whom Chapman calls "Grand Amateurs", the word being used in the literal sense of lovers of knowledge. They operated in a world in which "private independence brought a higher kudos than did a paid job", and—what was particularly relevant in the case of talented women—did not depend on formal academic qualifications. He places Mary Somerville firmly among the Grand Amateurs.

In his new book, Chapman examines Mary Somerville's career in more depth, demonstrating her place as an interpreter of science who may be bracketed with luminaries such as John Herschel or William Whewell. He sets the scene with an account of the organization of science in late Georgian Britain, and in that frame he outlines Mary Somerville's early life, from her childhood in Scotland to celebrity in London. She was largely self-educated, until she reached the advanced stages of her mathematical studies. Then, advised by academics of the Edinburgh school, she progressed, astonishingly, to the works of the great contemporary French mathematicians, culminating in the first published volumes of Laplace's *Méchanique Celeste*. In fact, her mastery of mathematics while still a young woman was ahead of that taught at Cambridge.

Mary and her Army physician second husband, William Somerville, began their married life in Edinburgh where they belonged to that great city's circle of scholars and thinkers. Chapman here makes the point, not generally appreciated, that Mary Somerville, with her progressive views on issues such as education and the abolition of slavery, was very much a product of the Scottish Enlightenment. He also draws attention to the role of William Somerville, a man of considerable standing in his profession, whose encouragement of his wife's talents (unlike the disparaging attitude of her first husband) was of the greatest importance to the furtherance of her career.

Life in Edinburgh for the Somervilles was followed by twenty years' residence in London, the decisive and most fruitful period of Mary's career. She established herself quite naturally among key figures in astronomy and physics such as John Herschel, Thomas Young, Francis Wollaston and many others—a list that, as Chapman says, "reads like a *Whos Who* of late Georgian science." Mary took every opportunity to learn from these experts: John Herschel was until his death her closest adviser. Geology, too, had its Grand Amateurs in William Murchiston and Charles Lyell, who also became her friends and guides. Mary Somerville's scientific circle was not confined to the metropolis: on her first European tour she was received as an honoured guest in Paris and in Geneva, even before she achieved fame as an author.

The proof of Mary Somerville's talent lies, of course, in her writings. Her first book, *The Mechanism of the Heavens*, tackled the most abstruse scientific subject of the day—celestial mechanics—and made her a celebrity. Her second, immensely influential work, *On the Connexion of the Physical Sciences*, drew together all branches of physics and astronomy, presenting, as the author puts it, "an intellectual vision of science that was truly encyclopaedic in scope". So successful was this book that it was almost a full-time job to keep it updated in successive editions throughout her lifetime. Her *Physical Geography* had a similar impact. The commercial success of these books, combined with a Civil List pension, made Mary Somerville a truly independent Grand Amateur. It also made her an icon to her own and later generations of women.

In the last stimulating chapter, the author reviews Mary Somerville's scientific career and discusses whether—as some have suggested—she might have achieved more if she had better opportunities. Certainly, had she been a man she would have been a member of learned societies with direct access in her own right to scientific progress. As regards potential scientific achievement, the historian's answer is that it is not realistic to transplant the conditions of one age into another. Mary Somerville was born in the late eighteenth century, and lived in the world of the Grand Amateurs; and it was in this world that she "found her voice and established her reputation". She used her voice and her reputation to advance the cause of women, and lived to see at least some results thereby.

This enjoyable book places Mary Somerville properly in a historical context, and conveys an entrancing picture of a woman and a scientist who combined formidable talent with a remarkably charming personality. It will make an ideal companion volume to Mary's own *Personal Recollections*, now fortunately available in Dorothy McMillan's recent excellent edition, *Queen of Science* (2). The book, which includes a discussion of the contrasting career of Mary Somerville's older contemporary, Caroline Herschel, may also be recommended as a level-headed contribution to the literature on the history of women in science.

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Mary Brück

Mapping Mars: Science Imagination and the Birth of a World, Oliver Morton, 2003 (Fourth Estate: London), 351 + xv pp, ISBN 1 84115 669 8, soft cover, price £ 8.99, 197 × 128 mm.

Observing the Craters of Mars, Part 1, Rodger W. Gordon, 2003 (Typographica Publishing: Middlesex, New Jersey), 51 pp, no ISBN, soft cover, price $15.00 (US), 217 x 136 mm.

> Already Mars was a small disc showing numerous surface markings even to the naked eye, and there was much peering through telescopes and argument over maps and photographs. Gibson had borrowed a large Mercator projection of the planet and had begun to learn the names of its chief features—names that had been given, most of them, more than a century ago by astronomers who had certainly never dreamed that men would one day use them as part of their normal lives. How poetical those old map-makers had been when they ransacked mythology! Even to look at those words on the map was to set the blood pounding in the veins—Deucalion, Elysium, Eumenides, Arcadia, Atlantis, Utopia, Eos. …
>
> The Sands of Mars, 1951
> A C Clarke

The author of *Mapping Mars* now lives in Greenwich and his book opens with musings on the view from Greenwich Park and on the significance of the Royal Observatory for both astronomy and geography. From this beginning it is obvious that the book is about more than the mapping of Mars in a strict cartographic sense. Rather it uses mapping as a metaphor for increased knowledge, charting the increased understanding which has transformed Mars from a point of light in the sky that moves against the fixed stars, to a real world, with its terrain charted and mapped, and the processes which have moulded it understood. The book is also about the artistic responses to that increased knowledge: the authors who have written about Mars and the artists who have drawn it.

Along the way there is a good deal of information about Martian exploration. There is some coverage of the pre-space age mapping by terrestrial observers, not least the system of nomenclature, largely due to Schiaparelli, which seems at once so evocative and so appropriate. However, most of the book is about the robotic exploration of Mars by spacecraft in the forty-odd years since Mariner 4 was launched to the Red Planet in 1964. The treatment is informal and anecdotal and introduces many of the scientists, engineers, and cartographers who have worked on the Mars missions, many of whom the author has interviewed. In addition to describing the topographic mapping of Mars the book also covers the disentangling of the geologic processes which have shaped the topography, the differences and similarities with the Earth, and the great debates over water and life.

Oliver Morton is a journalist and it shows: his book is well written, in prose that is at times elegiac and poetic. He effortlessly imparts a great deal of information about Martian topography, the process which have shaped it and the means by which this information has been gleaned. The book is aimed at the layman and requires no technical knowledge to follow. It is not a text book in any conventional sense, but nonetheless anyone reading it is likely to learn a great deal. The book is well produced. There is a small collection of colour plates and a grey-scale map as a frontispiece. More illustrations would certainly have been welcome, but would probably have pushed up the price. It is highly recommended.

Morton describes how in 1965 Mariner 4, the first successful US mission to Mars, sent back a few grainy pictures at, by modern standards, a ridiculously low bit-rate. Nonetheless these photographs caused a revolution in studies of Mars. They revealed a world pot-marked with craters and changed the conventional understanding of the planet from a world that was basically Earth-like (though less hospitable) to one which was more like the Moon (later the view would partly swing back again, of course). However, the existence of craters on Mars should not have come as a surprise. They had been observed before, glimpsed under fleeting moments of exceptional seeing by terrestrial observers. Unfortunately, it is impossible to record or reproduce such observations and they were never reported, partly for fear of ridicule. *Observing the Craters of Mars* describes these early terrestrial sightings of Martian craters by E E Barnard, John Mellish, G H Hamilton and others. The terrestrial observation of craters is a little-known byway in the history of Martian mapping, and this book is a good introduction to it. A previously unpublished article on the subject by Charles Capen is included as an appendix.

The author is an amateur astronomer and Mars observer of many years standing. The book is published by Typographica Publishing (see URL http://jersey-mall.com/tpo/) who also publish *The Practical Observer* magazine. It is very much a 'small press' publishing enterprise, but no worse for that. There is a 'printer's widow' and also a couple of other infelicities, but generally the typography and proof-reading are good. The book is well-illustrated in both black and white and colour, with numerous reproductions of sketches made by the various observers. It was published to coincide with the 2003 opposition of Mars. A companion volume is planned for the 2005 opposition, which, if it is similar to the present one, is something to look forward to.

Clive Davenhall

A Popular History of Astronomy During the Nineteenth Century, by Agnes M. Clerke (Facsimile Edition, Sattre Press, 2962 Middle Sattre Road, Decorah, IA 52101, USA; 2003), xviii + 489 pp., ISBN: 0-9718305-5-X, soft cover, US$35.00, 148 × 222 mm.

To my mind, the most invaluable contemporary reference book overviewing nineteenth century astronomy is Agnes Clerke's *A Popular History of Astronomy* …, even if inclusion of the term 'Popular' in the title erroneously conjures up an image of a short simplistic volume. Nothing could be further from the truth. As Mary Brück (2002) has shown, *A Popular History* is a *tour de force*, and Agnes Clerke a skilled writer: her "… breadth of knowledge, her capacity for assembling and collating data, were enormous." (page 44). While the original quickly went through several new editions, each studiously up-dated with the latest findings—often in that newest of astronomies, *astrophysics*—with the passage of the years second-hand copies found their way onto the international book market with increasing rarity, and an automatic corollary of this was an escalation in the sales price.

Bill Sattre from Sattre Press, has therefore done us all a great service by producing a facsimile reprint of the fourth edition (originally published in 1902), and at a very affordable price. Not only is the original content all there, but we are favoured with a new Foreword, penned by Mary Brück. Agnes Mary Clerke (1842–1907), she tells us, was born into an educated Irish family but did not attend school as she and her equally talented sister were tutored by their parents. Agnes

> … was already an experienced writer before she embarked on her *History*, being a regular but anonymous contributor to the prestigious *Edinburgh Review* and the author of several biographies in *Encyclopaedia Britannica*. Yet she remained elusive until the *Popular History of Astronomy during the Nineteenth Century* was put before the public. This, her first book, was an instant success and brought her into fruitful contact with the leading astronomers of the day, at home and abroad. (page iii).

For those with a penchant for nineteenth century astronomy, this facsimile edition has it all: background material on the foundations of sidereal astronomy; new data on sunspots, the chromosphere, prominences and the corona; planetary discoveries; transits of Venus and the astronomical unit; remarkable comets; advances in what we would now term Galactic and extra-galactic astronomy; and a useful introduction to those indispensable tools of the 'new astronomy', photography and spectroscopy. In her very readable presentation, Clerke deftly interweaves elements of positional astronomy and astrophysics, in the process producing a book that will long remain a favourite with many scholars.

At long last, we can now use Sattre's facsimile edition for everyday reference purposes, and endeavour to preserve and protect those cherished originals that decorate our bookcases.

Wayne Orchiston

Reference

Brück, M., 2002. *Agnes Mary Clerke and the Rise of Astrophysics*. Cambridge, Cambridge University Press.

The Maunder Minimum and the Variable Sun-Earth Connection, by Willie Wei-Hock Soon and Steve H Yaskell (World Scientific Publishing Co. Pte. Ltd.: Singapore 2003), 278 pp + 13, ISBN 981-238-274-7 $64/£44, ISBN 981-238-275-5 (PBK) $32/£22.

How constant is our Sun? The question, of practical as well as of scientific interest to its Earthly dependants, motivates this exposition of our present knowledge of the relations between Sun, Earth, and the space between. In answering it, the authors unfold the development of the subject from its beginnings about a century and a half ago, thus combining history with physics in a fascinating manner.

The book's title refers to the 'Maunder Minimum', a period of more than seventy years in the seventeenth century when spots on the Sun were unusually rare. That period, named after the British astronomer E W Maunder, was also a time when contemporary accounts of various kinds point to colder than average temperatures in many countries over the globe. The Maunder Minimum was not the only cold spell in the millions of years of our Earth's existence. Neither was it unique in terms of sunspot absence: modern data on the atmospheric production of carbon 14 by cosmic rays show that prolonged periods of solar inactivity have occurred ten times in the last 7000 years, and another may be anticipated in the future.

The serious study of solar-terrestrial relations dates from the discoveries in the mid-nineteenth century of unmistakable connections between the state of the spotted Sun and certain geophysical manifestations. The famous eleven-year cycle of sunspot numbers was discovered in 1843; then came to light the identical periodic cycle in the variation of the elements in Earth's magnetic field, which in turn was found to be linked with the visibility of polar auroral displays. Individual great magnetic storms and spectacular aurorae appeared to be associated with large sunspots, though the correspondence was far from straightforward, and no plausible physical explanation was in sight.

In discussing these phenomena, and efforts to interpret them, the authors give particular prominence to the long-term systematic work at the Royal Observatory Greenwich. The most complete records of the Sun anywhere in the world were the daily photographs obtained there from 1872 onwards under the charge of (Edward) Walter Maunder, who was to devote over forty years of his life to the task. In 1904-05, drawing on observations accumulated by him over three sunspot cycles, he published a thorough analysis of the subject, its best-known finding being the famous butterfly diagram displaying the latitude drift of spots in the course of a solar cycle. He also investigated earlier records, including the long 'calm' in the seventeenth century that now bears his name. Maunder's discovery of regions on the Sun's face devoid of visible spots, which nevertheless were sources of terrestrial magnetic activity (foreshadowing M-regions), and his suggestion of particles being ejected from certain restricted areas (the later 'coronal mass ejections' and 'coronal holes'), were notable insights. He supported, and—as the present authors show—actually anticipated the corpuscular theory (that magnetic storms were caused by charged particles emanating from the Sun), proposed by Svante Arrhenius in Sweden, which came to be generally accepted by the end of the century. The Maunders (because one must include Walter's mathematician wife, Annie, who was his collaborator from the time she joined the Greenwich staff as his assistant 'lady computer' in 1891, and having resigned her post on marriage four years later, continued as his close helper and adviser), were confident of their own conclusions. The authors suggest that Maunder, who lacked a university education, was sidelined at this time by the academic establishment. A case in point was the debate on the rôle of giant sunspots in triggering large magnetic storms and aurorae. The elderly Lord Kelvin (formerly William Thomson), a dominant figure in the British scientific elite, always maintained that the apparent connection was an impossibility, on the assumption that the Sun's magnetic energy would be radiated like light. The authors admire Maunder for holding firm to a contrary opinion.

The next major stage in solar-terrestrial research was the theoretical work on the physics of the corona by eminent mathematicians such as Sydney Chapman, who also provided the first satisfactory theory of solar corpuscular radiation. The crowning achievement of that age was Eugene Parker's brilliant solution of the supersonic solar wind in 1958—a year that, coincidentally, had just witnessed the launch of the first Earth satellites. The final chapters of the book summarize the present highly-complex picture of the Sun and its environment, including Earth and its magnetosphere, brought about by advances in physics and technology and by research in space. Such studies contribute to the much larger problem of climate change and global warming, and are therefore of the utmost scientific importance. These intricate discussions, intended for solar physicists and climatologists, are quite accessible to less specialist readers of this book. The text is mathematics-free, with much of the technical information supplied through illustrations taken from the literature.

The book is a rich tapestry of scientific information and wide-ranging historical narrative, into which is woven the personal story of Walter Maunder and his wife Annie. The Maunders were among the most experienced eclipse observers of their day, and were active promoters of amateur astronomy in Britain. As regards the solar-terrestrial connection, the authors see Maunder as "clearly a man ahead of his time" and his wife as a collaborator who brought the benefit of a university training to an unusual and devoted partnership. They are here given an honoured place in the annals of the Sun.

Mary Brück

Stargazer. The Life and Times of the Telescope, by Fred Watson (Allen & Unwin, Sydney, 2004), x+ 342 pp., ISBN: 1-86508-658-4, cloth, A$35, 140 × 204 mm.

With a well-thumbed copy of Henry C. King's *The History of the Telescope* occupying pride of place in that part of my library dealing with telescopes and their history, I was curious to see how Fred Watson's new work, *Stargazer. The Life and Times of the Telescope*, measures up. Fred was one of my colleagues at the Anglo-Australian Observatory (AAO), and in Australia is a legend for his ability to very effectively communicate the excitement of astronomy at a public level. Some would see him as the Patrick Moore of Australian astronomy! But he is more than that: he is a highly-respected researcher, and is Astronomer-in-Charge at the AAO Coonabarabran site, home of the 3.9-metre Anglo-Australian Telescope. After leafing through the 350 pages making up *Stargazer*, I was not disappointed. Once more Fred has done an admirable job, in the process bringing the history of the telescope to life.

Most astronomical histories start at the beginning and work their way chronologically towards the present day, but in *Stargazer* Fred begins his survey with a chapter on "Power Telescopes", which recounts a major conference on telescopes and instrumentation for the new millennium that was held in Munich in March 2002. The discussion largely focussed on existing and planned telescopes in the >6.5-metre class, and these surely provide a sobering technological comparison when measured against the pioneering refractors and reflectors of the seventeenth century.

Early refractors and their scientific predecessors occupy the next five chapters, and along the way we visit the writings (and experiments?) of Thomas Digges and William Bourne, examine competing claims for the invention of the first telescope, and survey Galileo's marvellous observational record. The invention of the reflecting telescope is also steeped in controversy, and Fred expounds on this in Chapters 7 and 8 before discussing the work of Gregory and Newton. After examining all the evidence, Fred is moved to suggest that "If Newton is to be called the 'father of the reflecting telescope', then surely Cavalieri must be its godfather." Cavalieri's interesting role is discussed on pages 134-135.

Subsequent chapters, in the main, chart familiar territory and familiar names—Ramsden, the Dollond dynasty, Hadley, Short, William Herschel, Sir James South and the 'Great Equatorial', Grubb Lassell, Nasmyth and Lord Rosse—before adopting a more parochial tone with an account of the "Heartbreaker", the Great Melbourne Telescope. That chapter ends with what for me must be the saddest image in the whole book, a photograph showing all that remains of the Great Melbourne Telescope after the devastating fire that swept across Mount Stromlo Observatory on 18 January 2003.

The second half of the nineteenth century, with its advances in optical design, mountings, accessories (such as the spectroscope) and the emergence of what Fred refers to as the 'Big Refractor', is discussed in Chapter 14, which leads to a chapter on the twentieth-century quest for increasing aperture and the emergence of the reflecting telescope and various catadioptric designs as the research instruments *par excellence*. Given his intimate association with the 1.2 metre Schmidt at Siding Spring mountain, it is no surprise that Fred devotes a number of pages to Schmidt telescopes and that "... eccentric ... ill-adjusted genius names Bernhard Voldemar Schmidt, who solved the problem of wide-angle photography with fast focal-ratio telescopes." (page 258). Chapter 14 ends by introducing the 4 metre class telescopes, thereby providing a natural link to the very first chapter of this fascinating book.

But *Stargazer* does not end there. In focussing all-to-briefly on radio astronomy and space-based telescopes and satellites in "Walking With Galaxies. Towards the Half-Millennium", Fred takes us beyond the realm of the visible, and above the light and other pollutants of the Earth's atmosphere, before indulging in some amusing crystal ball-gazing in the final chapter, which is conveniently set in the year 2108 (and as such celebrates the telescope's first half millenium)! It is a nice way to end a book, but for those wanting more, there are 33 pages of notes, sources and references, followed by a useful glossary and a master list of the world's largest ground-based optical and infrared telescopes that were operating or near operational in 2004.

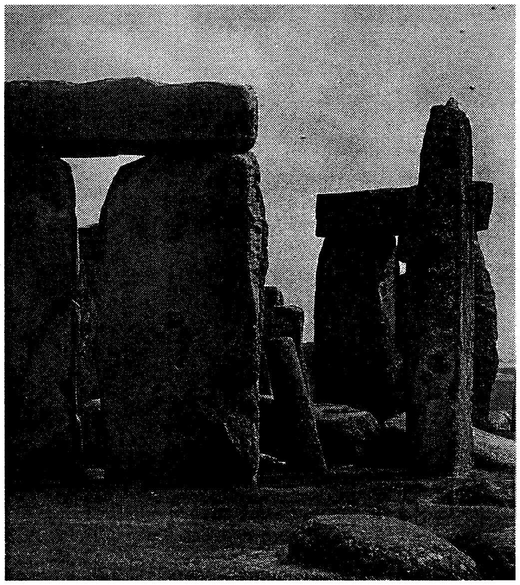
If you have an interest in telescopes and want to enjoy the interplay of technology and human drama, then this book is a 'must' for your library. I can do no better than quote from Patrick Moore's Foreword, where he says that Fred Watson "... has a marvellous story to tell, and he has done so in his unique way. This is a book which will be enjoyed by beginners and specialists alike ..."

Wayne Orchiston

Stonehenge Earth and Sky, by Gerald S. Hawkins and Hubert A. Allen, Jr (Wessex Books, Salisbury, 2004), 48 pp., ISBN 1 903035–24-4, paperback, £5.99, 158 × 250 mm.

If you have not been to Stonehenge and most probably never will, then this is your chance to see and learn about this New Stone Age monument. Crammed into this little book is most of what you need to know about Stonehenge together with some fifty illustrations. It is divided into sections, most of which are four pages with those on the midsummer and midwinter a little longer. Amongst these are: What is Stonehenge? What was the Purpose? How old is Stonehenge? Builders and Designers; The Station Stones; Carvings and Numbers; and What the Ancient Greeks Said.

Stonehenge Earth and Sky begins with a description of the more than four thousand-year old monument and definitions of some of the terms used – trilithons, sarsens, Heel (Hele) Stone, and Aubrey Holes (discovered by John Aubrey in 1666). The latter consisting of fifty-six holes arranged in a circle some two hundred and ninety feet in diameter and sixteen feet apart. Their purpose seems to be unknown. With no written records at the period of Stonehenge's construction, archaeological diggings have only revealed negative results – no pottery, no gold ornaments, only a few human skeletons, and some ashes from cremations. It is therefore the stones and their arrangements which strongly suggest a religious significance.



The tall, thin Stone 56, the largest ever to be shaped in ancient Britain (over 50 tons). The tenon knob on the top matched a mortise hole in a lintel.

Radio-carbon dating puts the earliest human presence around 7000 BC, far older than Stonehenge which was constructed from 3200 for the ditch at the south entrance to 1720 BC for the Y and Z holes dug around the monument. Remains from human sacrifice have been found near the north-east entrance dating from 2270 BC.

The arrangement of the stones at Stonehenge gives a view of the rising Sun, on the summer solstice, at its most north-easterly position just to the left of the Hele Stone or between it and its missing companion. Naked-eye observations by the Stonehenge people created alignments that were accurate to one degree, an impressive feat in Neolithic times. Not only the Sun, but also the Moon's positions were taken into consideration at Stonehenge. These alignments were put forward by Hawkins (1963) and occurred in the 'High' year of the Moon which had intervals of 19, 18, and 19 years adding up to 56, the Stonehenge cycle.

The book ends with this short paragraph:

> Stonehenge in 3000 BC was a bold and grand endeavour to join earth and sky, to lock the patterns of the sun and moon in post, stones and archways. Even when the work was ended around 1700 BC its fame continued to be recognised far afield, across the ages of time, into the present day.

It is unfortunate that the senior author Gerald S Hawkins passed away before the publication of the book. However, if you are looking an inexpensive succinct description of Stonehenge then this little book is the answer. For if we apply the words of Barnard (1927) "one picture is worth ten thousand words" then *Stonehenge Earth and Sky* is worth half a million words.

John Perdrix

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Journal of Astronomical History and Heritage

It is with a heavy heart that I pen this, my first and last, editorial in the *Journal of Astronomical History and Heritage*. I have been advised by my medical practitioners to cease active work, hence the lateness of this issue. I have enjoyed spending the past twenty years editing and publishing the *Australian Journal of Astronomy* and this publication. All this would not have been accomplished without help, and in particular I should like to thank Wayne Orchiston, Ivan Nikoloff, and Martin Yates, as well as members of The Editorial Board, the authors, the referees, and the subscribers.

It is pleasant to reflect on the range of papers which have appeared in the journal since its founding in 1998. All continents—except Antarctica—have been accommodated; in chronological terms, papers have spanned the interval from the third millennium BC to the twentieth century of the present era; and thematically, most of the main areas of history of astronomy have featured at one time or another.

I am pleased to announce that the *Journal of Astronomical History and Heritage* is to continue. As from Volume 8, Number 1 (i.e. June 2005) it will be published by the Centre for Astronomy, James Cook University, Townsville, Queensland 4811, Australia. Dr Wayne Orchiston will continue as Editor, but for the time being will remain at the Anglo-Australian Observatory in Sydney (e-mail: wo@aaoepp.aao.gov.au).

Whilst the loss of the tasks associated with the journal's production will be keenly felt, it is very pleasing to know that JAH^2 will continue and that there are sufficient papers in hand or promised to fill the next three issues.

Finally, I have been asked on many occasions what the little bird is which appears at the end of each article and on the title page. It is a perdrix. My motto, and that of Astral Press, "Iucundi acti labores" ("the remembrance of difficulties overcome is pleasing"), is particularly significant at this difficult time.

C'est le cœur lourd que je vous écris mon premier et dernier éditorial pour le *Journal of Astronomical History and Heritage*. Mon médecin m'a annoncé que je devais cesser de travailler, d'ou le retard de ce numéro. J'ai été heureux de passer vingt années a éditer et publier le *Australian Journal of Astronomy* et le *Journal of Astronomical History and Heritage*. Ceci n'aurait pas été possible sans la collaboration de plusieurs et je voudrais remercier spécialement Wayne Orchiston, Ivan Nikoloff, et Martin Yates, ainsi que les membres du Comité Editorial, les auteurs, ceux qui ont fourni des renseignements, et les souscripteurs.

C'est un plaisir de réfléchir sur la portée des papiers qui sont apparus dans le journal depuis sa fondation en 1998. Tous les continents – sauf l'Antarctique – y étaient incorporés; en termes chronologiques, les papiers ont embrassé la période du troisième millénaire avant Jésus-Christ jusqu'au vingtième siècle de notre temps; et, thématiquement, la plupart des époques principales de l'histoire de l'astronomie ont étés adressés à un temps ou un autre.

Je suis heureux de vous annoncer que le *Journal of Astronomical History and Heritage* va continuer. A partir du Volume 8, Numéro 1 (c'est-à-dire juin 2005) il sera publié par le Centre for Astronomy, James Cook University, Townsville, Queensland 4811, Australia. Dr Wayne Orchiston continuera dans son rôle d'éditeur, mais pour le moment il restera à la *Anglo-Australian Observatory* à Sydney (son email est wo@aaoepp.aao.gov.au).

Malgré qu'il me manquera beaucoup de participer aux travaux de la production du journal, je suis ravi de savoir que JAH^2 va continuer et qu'il y a suffisamment de papiers remis ou bien en main pour remplir les trois prochains numéros.

Finalement, on ma souvent demandé quel est l'oiseau qui figure à la fin de chaque article et sur la page de titre. C'est une perdrix. Ma devise, et celui de *Astral Press*, est la suivante: *«Iucundi acti labores»* — «le souvenir des difficultés surmontées fait plaisir» — ce qui signifie énormément de choses pour moi en ce moment.

È con cuore addolorato che scrivo il mio primo ed ultimo editoriale nel *Journal of Astronomical History and Heritage*. I medici mi hanno consigliato di cessare la mia attività lavorativa da quest'ultimo numero. È stato per me un piacere trascorrere questi ultimi vent'anni facendo l'editore e lo stampatore dell'*Australian Journal of Astronomy* e di questa rivista. Non avrei potuto far questo senza aiuto: in particolar modo desidero ringraziare Wayne Orchiston, Ivan Nikoloff e Martin Yates, e insieme tutta la redazione, gli autori, i *referees* e i sottoscrittori.

È piacevole pensare alla serie di articoli apparsi nella rivista fin dalla sua fondazione nel 1998. Tutti i continenti – eccetto l'Antartico – hanno qui trovato posto; in termini cronologici, gli articoli hanno spaziato dal terzo millennio avanti Cristo al ventesimo secolo dell'era attuale, e dal punto di vista degli argomenti, sono state trattate di volta in volta le principali aree tematiche della storia dell'astronomia.

Ho il piacere di comunicare che il *Journal of Astronomical History and Heritage* continuerà ad uscire. A partire dal Volume 8, N° 1, (cioè Giugno 2005) sarà pubblicato dal Centre for Astronomy, James Cook University, Townsville, Queensland 4811, Australia. Wayne Orchiston continuerà ad essere Editore, ma attualmente rimarrà all'Anglo-Australian Observatory in Sydney (e-mail: wo@aaoepp.aao.gov.au).

Pur sentendo profondamente la perdita dei compiti legati alla produzione della rivista, è di grande soddisfazione per me sapere che JAH^2 continuerà e che ci sono articoli sottoposti o promessi in quantità sufficiente da completare i prossimi tre numeri.

Infine, mi è stato chiesto in molte occasioni che uccellino è quello che appare alla fine di ogni articolo e sulla copertina: è una pernice (*perdrix*). Il mio motto, e quello dell'Astral Press, "Iucundi acti labores" ("il ricordo delle difficoltà superate è gradevole") è particolarmente significativo in questo difficile momento.

John Perdrix

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