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Summary of the RAS Specialist Discussion Meeting on

IS A MOON NECESSARY FOR THE CO-EVOLUTION OF THE BIOSPHERE OF ITS HOST PLANET?

A biosphere could be thought of as being the part of a planet that is capable of supporting life. A question which is often asked is: "is a moon necessary for generating the conditions for life to become established on a planet?" For example, it is known that our Moon affected tides more strongly on the early Earth. This in turn generated a more cyclic environment which may have colluded in the formation of life on Earth. Is this moon–planet interaction a typical product of the planetary accretion, or does the Earth exhibit an unusual combination of properties that were necessary preconditions for the emergence of life?

One of the first of its kind, this meeting, held on 2013 March 8 in the Geological Society Lecture Theatre, was designed to describe and evaluate the importance of a moon in relation to the co-evolution of the biosphere of its host planet. Bringing together speakers with a wide range of different perspectives, it was expected that the outcome would be a wider understanding of a moon's relevance. There were approximately 35 participants, predominantly moon specialists who ensured a lively debate and exchange of ideas.

The meeting began with a presentation on 'The Moon and the origin of life' by Dr. Chris Benn, of the Isaac Newton Group of Telescopes, La Palma. He started by noting that several observed features of the Earth's environment are to some extent anthropically selected, *i.e.*, they have been crucial for the development of observers on Earth. Obvious examples are the luminosity and stability of the Sun and the radius and circularity of the Earth's orbit about it. The wide-ranging effects of past interactions between the Earth and its unusually-large Moon have inspired speculation that the Moon might also have played a crucial role in the origin and development of life on Earth. Several specific mechanisms have been suggested: (*i*) the presence of a large moon in Earth orbit suppresses changes in the Earth's obliquity — such changes would have severely affected the climate, possibly inhibiting the evolution of complex life; (*ii*) the impact which created the Earth–Moon system would have removed any primordial atmosphere, reducing the risk of Earth ending up with an atmosphere hostile to life's chemistry, as is that of Venus;

(*iii*) prolonged heating from that impact might also have played a role in the generation of the Earth's unusually strong magnetic field, which shields the Earth and its atmosphere from the solar wind; and (*iv*) the (probably) larger tides raised by the Moon at the time of the origin of life would have resulted in wetting/drying cycles in extensive tidal pools. It's thought that those cycles may have been crucial for encouraging polymerization and for fostering the long sequences of chemical reactions which must have preceded the dawn of life. The basic message of this talk was that, although at present we can only speculate about the relative likelihood of specific mechanisms, the Moon 'may' have played a crucial role in the origin of life and the evolution of intelligent observers on Earth, which has potential consequences for (*i*) the way we judge otherwise-unlikely hypotheses about the origin of the Moon, and (*ii*) the strategies we deploy in future searches for life-bearing planets.

Dr. René Heller of the Leibniz Institute for Astrophysics, Potsdam (AIP), was the next speaker, and his presentation was entitled 'Exomoon habitability constrained by illumination and tidal heating'. Essentially René discussed the relevance of exomoons around their host planets, which is especially important as, to date, most extrasolar planets being detected are gas giants, of which some are within the habitable zone (HZ). This raises the question: what are the surface conditions of the exomoons of such extrasolar planets? Although observations of the surfaces of exomoons will not be possible at any time in the near future, what can be speculated is that these exomoons will be subject to illumination both by the associated star and reflected light from the host planet, as well as being affected by tidal heating due to the gravitational effect of star and the planet. That increases the real possibility of the existence of potential planets/satellites that might harbour life elsewhere in the Universe (see also *Astrobiology*, **13**, 18, 2013).

Dr. David Waltham, of the Royal Holloway, University of London, gave a presentation entitled: 'The Moon's influence on Earth habitability: myths and uncertainties'. He then introduced a note of caution regarding widely held opinions on the impact of the Moon on Earth habitability. Specifically, he showed computer simulations demonstrating that the effect of tidal slowing of the Earth's rotation is more important than the effect of stabilization through increased tidal torque and, hence, that our large Moon actually very nearly destabilizes our axis. He also discussed the consequences of tidal resonance in Earth's ocean basins and how that may mean that tidal amplitudes were actually smaller when our planet was young than now.

Professor Jacques Laskar of Astronomie et Systèmes Dynamiques IMCCE, Observatoire de Paris, France, presented 'The Moon's stabilizing effect on the Earth's obliquity'. Currently the Earth's obliquity is 23°·25 on average, with an oscillation period of around 40 000 years and amplitude of 1°·3. Moreover, the Earth's rotational axis is stabilized over long time periods due to the Earth being orbited by a single moon which is significantly massive. In addition, he pointed out that the Earth will enter a chaotic period (in approximately $1{\cdot}5 \times 10^9$ years from now) whereby the obliquity will wander haphazardly between 0° and more than 85° as the Moon recedes from the Earth. Obviously this will have a devastating effect on global seasons and possibly the evolution of life.

Professor Nathan Nelson of the University of Tel Aviv, Israel, spoke about 'Life under the Sun'. The Earth's particular distance from the Sun, its specific size, and the absence of violent stellar events were all necessary requirements for both the origin and evolution of life on Earth. One of life's 'inventions' on Earth was the process of photosynthesis. This process has been governing the evolution of life on Earth for over $3 \cdot 5 \times 10^9$ years and thus has been determining the composition of the atmosphere and the Earth's surface. Almost all organisms on Earth receive energy directly or indirectly from oxygenic photosynthesis carried out by plants, green algae, and cyanobacteria, by which life has been sustained on Earth since its inception.

The final session was the questions and answers from the audience. It was a lively debate with the general consensus of opinion being that the Earth–Moon coupling was necessary for both the origin and evolution of life on Earth. Moreover, large moons may be useful pointers in the search for life-bearing planets in the Universe.

The organizers (Dr. Sohan Jheeta, Dr. Martin Dominik, and Professor Nigel J. Mason) extend their thanks to the Royal Astronomical Society for its hospitality and for hosting this specialist discussion meeting. — S. JHEETA.

FALLING THROUGH AN EARTH TUNNEL — AN INFORMAL HISTORY OF A CLASSIC PROBLEM

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A chance encounter with an old physics problem results in a search to find its origins, and in the process an historical 'whodunit' unfolds. Following numerous twists and serendipitous turns, however, the paper trail ultimately leads to the pen of the 5th Astronomer Royal, the Reverend Nevil Maskelyne.

A chance beginning

It all began by accident on an otherwise ordinary day while I was working on a completely unrelated research project. I found myself looking over a copy of *The English Mechanic* journal for 1924 March 28. There, by chance, I came across a short note by S. A. Swan, BSc (Eng.) in the 'Replies to Queries' section. The note related to Query No. 128 by 'Cymro' which appeared, upon further investigation, in the 1924 March 7 issue of the journal. Cymro's question ran, "if two points on the surface of the Earth, taken to be a uniform sphere of radius *a*, were connected by a straight smooth tube, prove that a particle introduced into the tube at one extremity and there released would move in the tube with simple harmonic motion. In what time will the particle arrive at the other extremity of the tube; also what will be its velocity at the middle point of the tube?"

Many readers, I am sure, will immediately recognize this question — it is a classic, and variants can be found in many (many) introductory textbooks on differential equations and particle dynamics. The answer for the travel time is $T = \pi\sqrt{R/g} = 42{\cdot}23$ minutes, half of the minimum time to orbit the Earth's circumference, and the velocity at the mid-point is $\sin(\theta_0)\sqrt{Rg}$, where θ_0 is the half-angle subtended at the centre of the Earth by the chord that the tunnel makes across its interior, R is the Earth's radius, and g is the surface acceleration due to gravity. A full solution to the problem is provided, for those in need, in *Physics by Example*[1], by W. Rees (problem 46). I recall working through just such a question while an undergraduate student at the University of Sussex in the late 1970s. Cymro's question is the generalized (more interesting perhaps) version of the problem that is typically encountered in the form of a hole, or tunnel, being excavated from Earth's North Pole to South Pole along its spin axis. The question is clearly a flight of fancy — the Earth is neither homogeneous, nor solid through and through, nor static, nor spherical, and the sheer mechanics of constructing such a tunnel are well beyond human capacity — the list of difficulties is, indeed, almost endless. All the practical issues aside, however, upon seeing the question again, that old classic, I started thinking about who first penned the question, and what was its historical context. At this beginning stage, I will distinguish between the two versions of the Earth-tunnel question: let Q1 be the version in which the tunnel is diametrical, cutting right through Earth's core, and let Q2 be the version where the tunnel is a chord cutting across Earth's interior but missing the very centre.

Turning to the ubiquitous Internet it did not take long to find a good number of websites concerned with both questions Q1 and Q2, and specifically with their application to the so-called gravity-train[2] concept for which, in principle, the travel time between any two points upon the Earth's surface is just 42·23 minutes. Many of the web-page accounts seemed to me quite appalling, offering no clear explanations, no useful references, no context, and more often than not either riding roughshod over or completely misinterpreting historical detail — Thomas Carlyle's quip that *history is the distillation of rumour* had run, as far as I could tell, rampant upon the Internet. Well, I know I am not the first person to decry the faults and fallacies of the blogosphere. *Caveat emptor*. For all my searching, however, I was no closer to learning who might have first posed the Earth-tunnel question.

The origin of Q1

It was common practice in *The English Mechanic*, as in most magazines and journals at the time of interest, to use a pseudonym to both ask and answer questions. I have not been able to determine who Cymro was, other than presumably someone of Welsh origin, since, with a subtle piece of word-play, Cymro is the Welsh word for Welshman. In his answer to query 128, Mr. Swan was somewhat unusual in (apparently) using his actual name and stating his professional credentials. A second response to Cymro's question in the 1924 March 21 issue of *The English Mechanic* was provided by a correspondent simply identified as CP no. 1 — again, a *nom de plume* of unknown attachment. Usefully for my search, however, CP no. 1 points Cymro in the direction of Tait & Steele's book, *A Treatise on the Dynamics of a Particle*[3]. This classic text ran to many editions, but was first published in 1856. A study of Tait & Steele's treatise soon revealed their version of Q1 under the exercise, "supposing the Earth a homogeneous spheroid of equilibrium, the time of descent of a body let fall from any point P on the surface down a hole bored to the centre C, varies as CP, and the velocity at the centre is constant". At this stage, we have moved some 68 years back in time from Cymro's query. Interestingly, I found several variants on the classic Q1 question, some much more complicated, within Tait & Steele. One exercise, for example, asks, "the Earth being supposed a thin spherical shell, in the surface of which a circular aperture is made, if a particle be dropped from the centre of the aperture, determine its velocity at any point of the descent". This question has the nice twist, of course, that the motion in the interior will be constant and non-oscillatory, since the gravitational force inside of a shell is always zero. In the situation where the shell is very much thinner than the diameter of the particle, the velocity inside of the shell will correspond to that produced by the particle falling through its own diameter (at the surface of the shell) under a constant (surface) gravitational force — accordingly $V = 2\sqrt{rg}$, where r is the radius of the particle — this is the extreme opposite of the particle falling under constant gravity through a sphere or radius R, where the velocity at the centre will be $V = \sqrt{2R/g}$. When the shell has a finite thickness, then the question is considerably more challenging [see Rees[1], problem 45]. Unlike Q1 and Q2, the "particle moving inside of a spherical shell" problem can be easily traced back to Newton's *Principia* (first published in 1687), specifically drawing upon results established in *Book I: The Motion of Bodies*.

Tait & Steele include a much more formidable version of the classic Q1 question in the form of the following exercise, "the Earth being supposed to be at rest, and to consist of concentric spherical strata with densities varying gradually from the centre to the surface, investigate the law of density according to which a particle let fall from the mouth of a diametrical pit would perform oscillations exactly similar to those of a simple pendulum oscillating through 45° on each side of the vertical". There is nothing simple about this question since it explicitly indicates that the small-angle approximation to pendulum motion does not apply, and accordingly the answer will have to incorporate a discussion of elliptic integrals. The form and technicality of this latter question is consistent with its being some form of advanced-examination question, and indeed, in their preface Tait & Steele specifically note that many of the questions within their text are taken from College (that is Cambridge University) Examination Papers. Edward Routh in his *A Treatise on Dynamics of a Particle*[4] (published in 1898) also used College and Tripos questions within his text, and while the Q1 Earth tunnel question is included in his discussions (in Chapter 2, article 124), it is only presented as an 'exercise'. On the more contemporary side, Rees[1] indicates that his problem 46 (our Q2) is based upon a University of Birmingham examination question.

In addition to using college examination questions within their text, Tait & Steele also acknowledge their debt to various earlier works. They specifically mention *The Mathematical Principles of Mechanical Philosophy, and their application to the theory of Universal Gravitation*[5], by John Henry Pratt (published in 1841), and *A Treatise on Analytical Statics with numerous Examples*[6], by Isaac Todhunter (published in 1853). In Todhunter we find, in his Chapter XIII on 'Attraction', the following query: "to find the attraction [gravitational force] of a homogeneous spherical shell of small thickness on a particle placed within it". There the equation of force is developed (showing, in fact, that it is zero), and after some further development of the problem, a corollary is added stating that, "thus inside a homogeneous sphere the attraction varies as the distance from the centre $\frac{4}{3}\pi r\rho$, where ρ is the density of the sphere and r is the displacement from the centre". Todhunter's result, in modern terms, can be cast in the form of the second-order differential equation $d^2r/dt^2 = -\frac{4}{3}\pi r\rho$ which is the equation for simple harmonic motion and the essential starting point for answering our classic Q1 and Q2 questions. Todhunter does not directly ask the reader to consider a particle moving through a tunnel bored through the Earth, but the ingredients of the question are certainly there within his text. We find no mention either of Q1 in Todhunter's highly comprehensive two-volume work, *A History of the Mathematical Theories of Attraction and the Figure of the Earth*[7] (published in 1873). Pratt's text is altogether more formidable in tone and content than those presented by Tait & Steele and by Todhunter. While we do not find Q1 directly in Pratt, we do find a much more complex version of the question in his Chapter VIII, where the dialogue considers the proposition (number 174), "to find the attraction of a homogeneous body differing but little from a sphere in form, upon an internal particle". While Pratt develops his answer in terms of the eccentricity $e = (1 - \text{polar radius/equatorial radius})$, it is clear from his equations that when $e = 0$, the attractive gravitational force will vary linearly with distance from the centre (leading to the same results and equations as developed by Todhunter). Pratt comes very close to asking Q1 in his proposition 232, which seeks to show "a body falls towards a centre of force the intensity of which varies directly as the distance of the body from the centre: required to determine the motion". This is clearly a generalized question, and Pratt goes on to show that the equation of motion will be $d^2x/dt^2 = -\mu x$, where μ is "the magnitude of the force at a distance unity from the centre of force". Finally, Pratt solves the differential equation for the displacement x showing that the solution is simple harmonic. To his solution he adds the comment, "hence a body passes through the centre and stops at a distance on the other side equal to the original distance. From this point it will return to its original position and continually oscillate over the same space: the time of oscillation from rest to rest is $\pi/\sqrt{\mu}$. It is remarkable that this is independent of the initial distance of the body from the centre of force". At this stage in the investigation, while the essential details of the classic interior Earth oscillator question Q1 can be found as early as 1841 (Pratt's text), the first writing of the question still dates to Tait & Steele's text of 1856.

While not directly referenced in any of the texts so far discussed, another early classic of its kind is William Whewell's, *An Elementary Treatise on Mechanics*[8] (first published in 1836). Whewell is more directly concerned with motion under constant gravitational force, and draws the reader's attention to Galileo's work, stating that "Galileo assumed that gravity is a uniform force, and proved by experiment that for this uniform force the velocity increases in the proportion of the time". Again, in modern terms, this is Newton's equation of motion with $V = U + at$, where V is the velocity at time t, a is the constant acceleration, and U is the initial velocity at time $t = 0$. Under those conditions a particle let fall in a tunnel drilled diametrically through the Earth would arrive at the centre with velocity $V = \sqrt{2Rg}$, where g is the acceleration due to gravity at Earth's surface and R is the Earth's radius. As pointed out by Routh in his 1898 *Treatise*[4] (article 182, exercise 2), this situation nicely unites an infinite-fall problem with that of a finite-fall problem, in that the impact speed of a particle falling from infinity to the Earth's surface (the erstwhile escape velocity) is the same as the velocity acquired by the particle, starting at rest, falling from Earth's surface to its centre under constant gravity. Whewell does not consider our Q1 problem specifically, but he does have a surface variant which asks, "a person drops a stone into a well, and after t seconds hears it strike the water; to find the depth to the surface of the water". The nice little twist here is to include the time required for the sound wave to reach the top of the well.

Still not having pushed the date at which the Earth-tunnel question was first posed in print beyond 1856, the next step in my search was purely serendipitous. I chanced to come across a four-volume set of books[9], *The Mathematical Questions Proposed in The Ladies' Diary, 1704–1816*, by Thomas Leybourn (published in 1817). Until coming across Leybourn's works I was not familiar with *The Ladies' Diary or Woman's Almanack*, to give the full title to this annual publication[10,11]. Founded by John Tipper in 1704, *The Ladies' Diary* featured material relating to the calendar, sunset/sunrise times, and Moon phases for the year, along with recipes, medical advice, and short stories. Consistent with its subtitle "containing new improvements in arts and sciences and many interesting particulars: designed for the use and diversion of the fair sex", however, *The Ladies' Diary* also posed scientific and mathematical questions — there was even an annual prize question. Leybourn clearly recognized the great educational value in the kinds of questions being asked in *The Ladies' Diary*, as well as in the responses to them by the readers (many of whom appear to have been men), and accordingly decided to bring them all together under one cover in a series of books. To my great delight I soon found within Leybourn a reference to question number 784, as posed in *The Ladies' Diary* for the year 1781. The question reads, "if a ball be let fall from the surface down a perforation made diametrically through the Earth, it is required to find its velocity and time of falling to the centre, and to any given point, with other circumstances of its motion, abstracted from the effect of the Earth's rotation, and on the supposition that the Earth is a homogeneous sphere 8000 miles diameter". In one fortuitous swoop, the date of the first positing of the classic Q1 question was pushed back another 75 years.

Question 784 in *The Ladies' Diary* was posed under the *nom de plume* of Terricola — which translates from the Latin to 'a dweller upon the land'. A connection and hint to whom Terricola might be is provided by the identity of the then-editor of *The Ladies' Diary* — this being none other than the renowned mathematician Charles Hutton. The exact link, however, is provided by T. T. Wilkinson[12], who notes that Terricola was the *alter ego* of none other than Nevil Maskelyne, Astronomer Royal from 1765 to his death in 1811. The context of our classical Q1 connection is now made a little clearer in that it was Maskelyne's surveying work at Schiehallion Mountain in Scotland, in 1774 (with results calculated by Hutton and published in 1778) that provided the very first practical estimate for the mean density, and thereby the mass, of the Earth[13–15]. In 1780, however, when the Q1 question would have been submitted, Maskelyne, then some 15 years into his tenure as Astronomer Royal, would have been primarily involved in the supervision of observational work at Greenwich Observatory, as well as organizing Board of Longitude chronometer trials. Additionally, at that time, Maskelyne was directly involved in editing the second edition of the *Requisite Tables* (published in May 1781) to accompany the *Nautical Almanac and Astronomical Emphemeris*[13]. As far as I can tell, the origin of Q1 is not directly related to any specific work being conducted by Maskelyne at the time that it was submitted — its origin, therefore, may be located even further back in time than 1780. However, we suggest that Maskelyne, as a gifted mathematician, may have thought of his question, and used (as they were intended) the problems appearing in *The Ladies' Diary* as a light-hearted amusement and pleasant mental diversion — a break, as it were, from his more arduous 'official' work.

The 1782 issue of *The Ladies' Diary* indicates that the correct solution to question 784 was provided by Mr. Rob Phillips of St. Agnes (this presumably being an actual name rather than a pseudonym). The solution presented by Phillips is cast in the terms of "fluents" and "the fluxion of the time", but his answer does not show mathematically that the motion will be simple harmonic. In a corollary, however, Phillips argues on purely physical grounds that the ball "will oscillate forward and backward continually". We know nothing of detail about Mr. Phillips, other than he was an active respondent to questions posed in *The Ladies' Diary* for the years 1779, 1780, 1781, and 1782. With respect to Maskelyne, under the appellation of Terricola, he penned two additional prize questions (although no mention is given as to what the prize might be — other than presumably bragging rights). In the 1795 edition of *The Ladies' Diary*, Terricola's question reads, "suppose the whole terraqueous globe taken as a sphere, should be instantaneously turned into a uniform elastic aeriform fluid, whose particles repel one another with a force which is to that which those of air repel one another, as the density of the one to the density of the other, it will expand itself either to a finite or infinite extent, still preserving the form of a sphere. It is required to determine the force of gravitation tending towards the centre, and also the density, at any given distance from the centre; supposing the mean density of the Earth to be 3825 times that of air at the surface of the Earth". As well as being a decidedly tricky question, it is interesting to note in passing that Maskelyne is supposing a mean density for the Earth of order 4686 kg/m^3, which is low by modern standards (where a value of 5515 kg/m^3 is more appropriate) but is about 4 per cent higher than the value (4500 kg/m^3) deduced by Hutton from the 1774 Schiehallion experiments. As to the answer, Amicus (meaning *friend*; the pseudonym of the renowned mathematician, and reviewer of the mathematical manuscripts submitted for publication to the Royal Society of London, Reverend Charles Wildbore[16]) informs us that the gravitational force will vary as $2c/r$, where r is the distance from the centre and the constant $c = 3825/(2\pi R^2)$, where R is the Earth's radius. The density ρ at any point r is given by $\rho(r) = (R/r)^2/3825$.

The 1801 prize question posed by Terricola reads, "a quantity of matter being given, it is proposed to determine the figure of a solid of rotation made up of it, which shall have the greatest possible attraction on a point at its surface". Once again, Amicus provides an answer, with the curve of rotation being described by the equation $y^2 = (a^4x^2)^{1/3} - x^2$, where $a^3 = 15m/4\pi$ with m being the mass of material enclosed by the figure of rotation. It is of passing interest to note that Amicus — the Rev. Wildbore — was the editor (from 1780 to the time of his death in 1802) of the *Gentleman's Diary, or the Mathematical Repository* (founded in 1741), and he set the prize question that appeared posthumously in *The Ladies' Diary* for 1803. Much earlier in life, Wildbore contributed many mathematical questions to, and additionally answered those posed in, Benjamin Martin's journal *Miscellaneous Correspondence*[17,18]. The only question qualitatively similar to Q1 that I have found in Martin's *Miscellaneous Correspondence* was posed by "Rev. Mr. T-H-" in the 1757 October issue. The question reads, "suppose a bullet fall down eternally in this manner, the first minute 20 miles, the second 19 miles, the third 18 [and] 1/20 miles, and so onward in the same geometrical progression, how far will it fall in all whole eternity?". This infinite geometric series sums to 400 miles, as correctly deduced by Mr. R. Peckham and printed in the 1757 December issue of the journal. This question, while obviously contrived, is somewhat odd in that the bullet is falling and at the same time slowing down, coming eventually to a hovering, Zeno's paradox-like, stop, with each successive increment in distance getting smaller and smaller, but never technically becoming zero. Presumably, grossly to over-analyze the question, the bullet, in parallel with *Revelations* 9:2, is falling into the bottomless pit reserved for lesser demons and was accordingly being slowed by the foul-gas and smoke emanating from therein.

The origin of Q2

As noted earlier, rather than provide a complete answer to Cymro's question in *The English Mechanic* (our Q2), CP no. 1 points Cymro in the direction of Tait & Steele's (1856) book, *Dynamics of a Particle*[3]. Indeed, CP no. 1 indicates, correctly, that the answer to Cymro's question lies in their proposition number 48, which states that, "a particle constrained to move in a straight line, is acted on by a force always directed to a point outside of the line and varying directly as the distance of the particle from that point, to determine the motion". The text then goes on to show that the equation of motion is $d^2x/dt^2 = -\mu x$, where μ is the accelerating force on the particle at unit distance away from the centre of attraction, and this, again, is the equation for simple harmonic motion. Tait & Steele do not specifically ask the reader to solve an exercise cast in the form of Q2 directly, even though the solution methodology lies latent within the text. Interestingly, in his response to question 128, CP no. 1 comments that, "if Cymro will state his difficulty perhaps I can help him"; this suggests that CP no. 1 did not see the question as being a particularly novel or difficult one.

While Tait & Steele's text does not pose Q2 directly, it is posed in *An Elementary Treatise on the Dynamics of a Particle and of Rigid Bodies*[19], by Sidney Loney (first published in 1909). Specifically, Loney asks the reader to solve the following exercise: "assuming that the Earth attracts points inside it with a force which varies as the distance from the centre [this is equivalent to saying that the Earth is a homogeneous sphere], shew that, if a straight frictionless airless tunnel be made from one point on the Earth's surface to any other point, a train will traverse the tunnel in slightly less than three-quarters of an hour." Unlike Cymro's version of Q2, Loney does not specifically ask the reader to show that the train must undergo simple harmonic motion. This question may have been an examination question since Loney comments in the preface to his text that the book has been written for science and engineering students and "junior students of mathematical honours".

While there is no specific linkage between the two authors, in addition to the publication of Loney's text in 1909, famed popularizer of astronomy, Camille Flammarion, published a remarkable Earth-tunnel-related piece[20] in *The Strand Magazine* for September of that same year. Indeed, Flammarion's article explores the idea of actually constructing a tunnel to Earth's core, with the rationale behind the project being both scientific and utilitarian. The scientific benefits would relate to a detailed understanding of the Earth's interior, while the utilitarian spin-offs would be the extraction of useful ores and precious metals, as well as the opening-up of an "inexhaustible source of heat". After outlining the methods by which the Earth tunnel might be constructed (*via* the labour of soldiers and standing armies — thereby "distracting them from the art of war" — and even with the requisition of convicts and the unemployed), Flammarion eventually concludes that, "such a shaft, of course, is beyond the bounds of possibilities". Helpfully to my quest, however, Flammarion provides a small amount of historical background to his Earth-tunnel essay, noting specifically that French mathematician Maupertuis had considered the very same idea in the 18th Century (we shall pick up on this thread in the next section). Additionally, Flammarion also considers the question, "what would happen to a body [by which he meant a human body] falling into such a shaft".

Correctly, Flammarion notes that the body would "continue to describe a series of oscillations like a new kind of pendulum". It is clear from Flammarion's account, however, that he has failed to realize, or at least address, the effects of Earth's spin, which would drive the human-bob into the side of the tunnel, and the additional damping effects that would result from any air in the tunnel. Nonetheless, Flammarion correctly states, as the ideal tunnel problem requires, that the Earth-crossing time would be forty-two minutes.

Further back in time

One of the earliest, if not the first, 'sensible' thought experiment involving an object passing through the Earth was outlined in a letter, dated 1679 November 28, from Isaac Newton to the then-Secretary of the Royal Society of London, Robert Hooke (see ref. 21 for this correspondence). Newton's letter concerned "a fancy of my own about discovering the Earth's diurnal motion". The experiment outlined required a displacement measurement of a small "bullet" let fall through a height of "20 to 30 yards". Newton argued that the bullet would fall to the East of the point directly below the release location (as determined by a plumb-line). Indeed, inspired by Newton's letter, on 1680 January 16, Robert Hooke and his long-time friend Harry Hunt, set out for Garraway's Coffee House and "tryd fall of bullet in the hall"[22]. Hooke believed that the experiment "was very certaine" and immediately wrote to Newton explaining that his experimental idea was indeed, sound. It took Newton 11 months to reply to Hooke's letter, commenting in a rather terse note, dated 1680 December 3, that "I am indebted to you thanks"[21]. Had Newton in his original letter simply outlined his ideas for the Earth-rotation experiment all might have gone well between him and Hooke; however, Newton in grand philosophical fashion imagined what would happen to the falling bullet if it could pass unimpeded through Earth's interior under the influence of gravity alone. To this end he drew the path of the bullet as a spiral coming to rest at Earth's centre. Realizing that Newton had made a blunder, and that the unimpeded bullet would actually 'orbit' about the centre of the Earth, Hooke made the great mistake of correcting his taciturn correspondent. The ensuing rivalry between Hooke and Newton has been well documented[21,23], but the more happy result of their one collaboration (Newton the theorist and Hooke the skilled experimenter) opened the doorway for at least the reasoned approach to considering thought experiments about objects passing through the Earth, although at this stage in history no actual tunnel was being invoked.

Galileo, in his 1632 *Dialogue Concerning the Two Chief World Systems*[24], directly addressed the question of what should happen to a cannon ball dropped into a tunnel cut through the Earth's centre. Indeed, the discussion is brought up on three occasions, on days one and two of the *Dialogue*. "Arguing with a certain latitude", Salviati explains on day two of the *Dialogue* that the motion of the cannon ball can be likened to that of a pendulum, with the velocity gained by the cannon ball in its descent being sufficient to carry it through the Earth's centre and then, decreasing in speed thereafter, it would ascend to the Earth's surface on the other side of the tunnel. Galileo's physics, as espoused by Salviati, is technically incorrect, since he assumes a constant gravitational acceleration, but his intuition is good and he essentially blunders his way to the right answer. Sagredo seems convinced, but Simplicio, as ever, errs on the side of caution and sticks, albeit tentatively, to the Aristotelian approach that would see the cannon ball come to a stop at Earth's core. Galileo can offer no mathematical description of the cannon ball's motion, and he even provides an incorrect numerical sequence to illustrate how its velocity should change. In his later, perhaps more famous, rather than infamous, work, *A Dialogue Concerning Two New Sciences*[25], published in 1638, Galileo, while still discussing the variable motion of projectiles and the pendulum, makes no mention of cannon balls falling through Earth-crossing tunnels. This, of course, makes sense, since in the *Dialogue Concerning the Two Chief World Systems* Galileo is attacking the Aristotelian physical perspective, while in his *Dialogue Concerning Two New Sciences* he is attempting to establish new physical facts *via* experimentally verifiable methods — at this end stage in his life and career, Galileo has no wish to invoke thought experiments to further his cause.

Isaac Newton, unlike Galileo, was ever ready to contemplate fanciful thought experiments of one kind or another, and in addition to his 1679 correspondence with Hooke such idealizations can be found throughout his *Principia*[26]. In *Book III: The System of the World*, for example, in proposition XIX, problem III, Newton considers the question, "to find the proportion of the axis of a planet to the diameters perpendicular thereto". There, an interior Earth canal of water is considered, starting at the North Pole, descending to the Earth's centre and then rising to the surface again at the equator.

Swiss mathematician Leonhard Euler invoked the idea of an Earth-crossing tunnel on several occasions, over a thirty-year time interval in fact, in his various writings. Indeed, in 1727 when applying for a vacant professorship at the University of Basel[27], Euler presented for review his thoughts relating to the question as to what would happen if a stone were dropped into a tunnel that cut through the Earth, and he specifically described what would take place at Earth's centre. Euler argued that three outcomes were possible: (*i*) the stone would come to rest (this is the Aristotelian expectation so strongly attacked by Galileo[24,25]); (*ii*) it would proceed beyond the centre (this was Galileo's physically reasoned result); or (*iii*) it would instantaneously return from the centre to the surface point at which it had been dropped. Rather surprisingly, Euler stated that result (*iii*) would apply. He didn't get the professorship at Basel. In his 1727 application Euler offered no specific reasons for his choice of option (*iii*), but he returned to the topic in 1736, in his classic treatise[28] *Mechanics, or the science of motion set forth analytically*. There, in proposition 32 of Chapter 3, he considers the question: to find the speed of a body falling through a given interval, towards a gravitating centre when acted upon by a force that varies as some power n of the distance from the centre. To that proposition, Euler deduced that the velocity will be infinite at the centre, and concludes in (272) Scholium 2 that having reached an infinite speed there is no physical reason to suppose that the body must proceed beyond the centre, arguing that the body could go anywhere; he therefore invokes his earlier option (*iii*) and argues for an instantaneous jump from the centre back to the starting point. Euler's reasoning essentially returns to and even draws upon the interpretation and meaning of infinity adopted by the ancient Greek Philosophers — it being a potentiality that always has room for further actions. Acknowledging, however, that the situation is far from clear, Euler writes, "here we have to be more confident in the calculation than in our judgment and to confess that we do not understand at all the jump if it is done from the infinite into the finite".

Euler, some 26 years after writing his treatise on *Mechanics*, invoked the use of an Earth-crossing-tunnel thought experiment in a new text bringing together a series of letters concerning various subjects in physics and philosophy[29]. In letter LXIX, dated 1760 August 29, Euler is concerned with describing the "true direction and action of gravity relative to the Earth", and uses the idea of an Earth-crossing tunnel to illustrate the point that upwards and downwards "would suddenly change their signification" at the Earth's centre. In the following letter, dated 1760 August 30, Euler again introduces the idea of an Earth-crossing tunnel and writes, "a body penetrating into the bowels of the Earth loses its gravity, in proportion as it approaches the centre", and that, "a body at the very centre must entirely lose its gravity, as it could no longer move in any direction whatsoever". While still confident in his mathematics, Euler sidesteps the issue of infinite velocity in his letters, and simply writes, "it is true, such a project [the Earth tunnel] could never be executed, as it would be necessary to dig to the depth of 3956 English miles, in order to discover what would be the result". Euler died in 1783, and never publicly renounced his solution, requiring infinite speed and an instantaneous jump at the centre, to the stone dropped into an Earth-crossing tunnel.

Moving beyond the level of the thought experiment and the uncertainties of Euler, French mathematician Pierre Louis Maupertuis, while President of the Prussian Academy of Science in Berlin, called for the physical construction of a tunnel to the Earth's core in his *Lettre sur le progres des sciences*[30], published in the summer of 1752. This idea was just one of a whole list of proposals that Maupertuis thought deserved royal patronage and funding, and which would advance science and human knowledge. Sadly for Maupertuis, however, he had earlier made an enemy of Voltaire, who, realizing an easy target when he saw one, took it upon himself to mercilessly lampoon and ridicule the ideas outlined by the Berlin Academy's President. Writing in his satirical work, *The Diatribe of Dr. Akakia*[31], a pamphlet published in 1752 November, Voltaire writes, "we must further inform him [Dr. Akakia, *i.e.*, Maupertuis] that it will be extremely difficult to make, as he proposes, a hole that shall reach to the centre of the Earth (where he probably means to conceal himself from the disgrace to which the publication of such absurd principles has exposed him). This hole could not be made without digging up about three or four hundred leagues of Earth; a circumference that might disorder the balance of Europe". In his 1909 *Strand Magazine* article Flammarion simply advocates dumping the material removed from the tunnel into the sea to make a new country[20]. Well, Voltaire was no scientist, and much of what he ridiculed in his attacks on 'Dr. Akakia' later became active and indeed important areas of research and scientific investigation, but he did destroy Maupertuis, who resigned his Presidency and died a broken man in 1759.

Some final comments

Tunnelling into the Earth has been one of humanity's age-old preoccupations, either for the extraction of minerals or for the construction of transport routes. The theoretical consideration of how one might actually tunnel all the way to Earth's core, however, is of fairly recent genesis. Indeed, as recently as 2003, David Stevenson[32] proposed a "mission to Earth's core", in which a detector, placed within a 10^8 to 10^{10} kg surrounding mass of liquid-iron alloy, would migrate to Earth's centre over the course of about a week. In terms of physical tunnelling motivated by scientific investigation, the record is currently held by the Kola Superdeep Borehole Experiment which prior to its close in 2005 reached a depth of just over 12 km. The deepest oil-extraction boreholes extend a little deeper than the Kola experiment. The longest transport tunnel runs for 57 km under the Swiss Alps.

While the question concerning the motion of a particle falling through a hypothetical, diametrically-bored Earth tunnel has an ancient heritage, it has been suggested that a very much scaled-down, experimental version of the problem could be utilized to extract a measure for the universal gravitational constant G, as outlined by David Berman and Robert L. Forward[33] (pioneer of interstellar space-exploration studies and noted science-fiction author) in 1968. Both Berman and Forward worked, at the time of their publication, for Hughes Aircraft Company, and they attributed the idea behind their case-study to Jesse DuMond (CalTech) — DuMond specialized throughout his long and distinguished career in the measurement of fundamental physical constants. The proposed mini-Earth experiment would be conducted in space, with the aim of measuring the oscillation period of a small ball-bearing introduced into a tunnel cut through a large, homogenous test-sphere. By measuring the period of oscillation T for the ball-bearing, the gravitational constant is given by the equation $G = 3\pi/(T^2\rho)$, where ρ is the density of the test-sphere. The main problem that the authors identified in their proposal was that of manufacturing a truly homogeneous and spherical test-sphere — not only that, it was also realized that to determine a high-precision result a complex correction to the gravitational field pertaining to the test-sphere would be required in order to account for the extracted bore-hole material. A similarly constructed spacecraft-style experiment was later proposed by Warden and Everitt[34] in 1974, not to quantify G this time but rather to test the veracity of the equivalence principle. In their experiment two coaxial cylinders, made of different materials, would be monitored in order to look for any anomalous motion of the inner cylinder relative to the outer one.

In conclusion, then, as far as I have been able to ascertain, having searched nearly 400 years into the past, the now classical question — a ball falls through a tunnel drilled through the centre of the Earth, our Q1 question — first appeared in print form in 1781, in *The Ladies' Diary and Woman's Almanack*, and was posed by 5th Astronomer Royal Nevil Maskelyne. In contrast, the more generalized version of the question, in the form of Q2, in which the ball passes through a tunnel excavated so as to miss Earth's centre, appears to have first seen publication in 1909, as a student exercise, in Sidney Loney's *An Elementary Treatise on the Dynamics of a Particle and of Rigid Bodies*[18]. Having thus brought these specific first-appearance dates forward, the author now dutifully awaits corrections with respect to possible earlier publication dates — in particular it seems highly likely that Q2 was first posed much earlier than 1909.

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SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 233: HD 17922, HD 78899, HD 103613, AND HD 160934, WITH A NOTE ON HD 113449

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The four stars came to attention in different ways and are of different characters. HD 17922 is a 7^{m} F star, single-lined, with an orbit of moderate eccentricity (0·33) and a period a little less than a year. The $7^{m}\cdot6$ object HD 78899 is an 'identical-twin' pair of solar-type stars in an orbit whose eccentricity of 0·62 is extraordinary in relation to the shortness of its orbital period of almost exactly 20 days. HD 103613 is a triple system whose combined absolute magnitude is about $2^{m}\cdot5$. Its primary is probably an early-F main-sequence star (it seems too blue to be a slightly evolved star like Procyon), and has a low-eccentricity orbit with a period of 5000 days (nearly 14 years). The secondary object is itself a single-lined binary, considerably fainter than the primary, and exhibits an orbit with an eccentricity close to 0·5 and a period of 23 days. HD 160934 is a young late-K dwarf which was brought to attention by its chromospheric activity and photometric instability arising from spottedness, characteristics probably related to its rapid rotation ($v \sin i \sim 16$ km s^{-1}). It has proved to have an orbit with a period close to 10 years and the high eccentricity of 0·65. It has been resolved directly on the sky by the *Hubble* telescope and later observers, and a 'visual' orbit that is in very good accord with the spectroscopic one has been given for it; the secondary is about type M0 V but is not apparent in radial-velocity traces.

A final section of the paper offers a comparison of the orbit of HD 113449 given in Paper 212 of this series with spectroscopic and astrometric ones determined elsewhere.

Introduction

Among the objects treated in this paper, two are among those proposed as 'over-luminous F-type stars' by Suchkov (*cf* ref. 1) a dozen years ago through the ΔM_{c_0} criterion advanced by Suchkov & McMaster[2]. The actual luminosities of those stars, derived from the *Hipparcos* parallaxes, are significantly higher than those estimated from Strömgren *ubvy* photometry. Discrepancies of that sort were suggested to arise from duplicity of the objects concerned. A radial-velocity survey[1] by the present writer of about 100 bright F stars thus identified by Suchkov as over-luminous demonstrated that many of them are in fact double, although a substantial minority seem not to be. One of the numerous 'successes' in that programme — objects that did indeed turn out to be double-lined, so there were two objects contributing to the luminosity measured by *Hipparcos* although the photometry was unaffected or actually influenced in the opposite sense[3] — was HD 103613, a triple system described below. More ambiguous is the case of HD 17922, featured in the section immediately succeeding this one. It did indeed prove to be binary, but appears to be only single-lined. Although it can be admitted that photometry is sensitive to duplicity that is not apparent spectroscopically, so the companion whose signature is not seen in radial-velocity traces may yet influence the photometry, its duplicity seems unlikely to be responsible for more than a minor part of the 'over-luminosity' indicated by Strömgren photometry.

The duplicity of HD 78899 was discovered by White, Gabor & Hillenbrand[4] in the course of a seemingly somewhat random lot of observations of stars that were of something like solar type; the star was placed on the Cambridge spectroscopic-binary observing programme when the writer belatedly noticed that reference in early 2013. The late-K dwarf star HD 160934 was put on the programme immediately after Pandey *et al.*[5] drew attention to it in 2002 as a chromospherically active photometric variable and remarked that it had already been reported[6] to have a rotational velocity of 16·4 km s^{-1} — very high for such an object. The suspected implication that the rotation was locked to motion in a short-period binary orbit proved to be unfounded, but a relatively leisurely orbit has now been documented, superseding two premature efforts.

HD 17922

HD 17922 is a 7^{m} star that is to be found in Perseus, about 3° north-preceding Algol. It features in the *Supplement to the Bright Star Catalogue*[7], a fortunate circumstance that caused it to be classified on the MK system by Abt[8], who gave its type as F8V. The *HD* type[9] is F5, and a classification of F5V was made by Sato & Kuji[10]; but F8, which was already given in the David Dunlap Observatory (hereinafter DDO) survey[11] of the +25° to +30° declination zone (but without a luminosity classification), is more consonant with its colour index. The only ground-based measurement of that index itself seems to be one made in 1964 by Eggen[12], who gave the photometry as $V = 6^{m}\cdot86$, $(B-V) = 0^{m}\cdot53$, $(U-B) = 0^{m}\cdot06$.

In an earlier paper[13], Eggen included HD 17922 as a member of the 'Hyades group', which he found to require it to have a 'group parallax' of 29·5 arc-milliseconds. Later, in 1970 and again in 1992, Eggen[14,15] reaffirmed his assignment of the star to the Hyades group (or "supercluster" as it had become in the last paper[15]); the 'group parallax' had declined slightly, to 28·5 milliseconds*, in 1970. The parallax originally published[16] by *Hipparcos* was 19·45 ± 1·04 milliseconds, but it somehow changed by nearly 4σ and became slightly less precise, at 23·38 ± 1·11 mas, when the same data were subsequently re-reduced[17]. A further review of the *Hipparcos* data, taking into account the orbital motion demonstrated below, might now be profitable.

Whether the space motion really agrees closely with that of the Hyades is another matter that could bear being discussed anew, although the radial velocity determined below does not represent a large change from what was believed previously. Eggen[12–15] necessarily relied upon the mean velocity of +25·5 km s^{-1} found in the DDO survey[11]. That value was assigned a 'probable error' of 1·0 km s^{-1}, which, so far from identifying HD 17922 as a spectroscopic binary, was the fourth-smallest of the 20 values in the relevant column on its page of the *DDO Publications*; the paper does not give the individual measurements but only the mean. Eggen's 1964 paper[12] sets out to devote a major section to the DDO results, and includes an 8-page Table VII entitled 'Main sequence stars in the zone +25° to 30° declination', in which we might hope to learn something interesting about HD 17922. Unfortunately, although the actual DDO work embraced the whole 24 hours of right ascension, Eggen's table starts only at about HD 40 000, so for present purposes it is a disappointment.

There is in any case considerable doubt about the reality of the 'moving groups' that featured so largely in Eggen's numerous publications. A small investigation[18] of one such group by the present writer led to a suspicion that it was, to put it crudely, largely a fabrication. Moreover, a sample of 15 stars drawn from Eggen's listings of the 'Hyades Moving Group' itself was included in a survey by Boesgaard & Budge[19] of lithium abundances as a function of photospheric temperature (upon which those abundances for members of the Hyades, and of other clusters of comparable age, exhibit a remarkably strong and characteristic dependence). Those authors found that "the plot of Li abundance versus temperature for the Hyades Moving Group resembles a

* The writer apologizes for an error that he made in ref. 1, where he quoted a distance of 28 pc in place of a parallax of 28 mas; the distance corresponding to the latter is 35 pc, less (but still) discordant with the *Hipparcos* distance of 51 pc.

TABLE I

Radial-velocity observations of HD 17922

Except as noted, the observations were made with the Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
1988 Nov. 6·95★	47471·95	+22	$\overline{13}$·462	−3·5
1989 Oct. 5·03★	47804·03	25	$\overline{12}$·503	−1·5
1991 Jan. 8·81★	48264·81	45	$\overline{11}$·948	+18·5
2000 Feb. 21·92	51595·92	22·8	0·392	−0·9
Oct. 17·11	834·11	16·7	1·139	+0·1
Nov. 8·12†	856·12	18·3	·208	0·0
13·06	861·06	18·6	·223	−0·2
13·97	861·97	18·8	·226	−0·1
19·98	867·98	19·2	·245	−0·2
30·01	878·01	20·6	·277	+0·2
Dec. 13·97	891·97	21·8	·320	+0·1
27·97	905·97	23·1	·364	+0·2
2001 Jan. 6·88	51915·88	23·8	1·395	+0·1
25·84	934·84	25·6	·455	+0·3
Feb. 6·93	946·93	25·9	·493	−0·3
9·88	949·88	26·7	·502	+0·3
Mar. 12·86	980·86	28·9	·599	+0·3
June 29·09	52089·09	26·7	·938	−0·5
July 6·07	096·07	25·1	·960	−0·2
16·11	106·11	22·5	·992	+0·3
26·12	116·12	19·7	2·023	+0·3
Aug. 17·12	138·12	16·7	·092	+0·3
25·13	146·13	16·1	·117	−0·3
Sept. 30·05	182·05	18·7	·230	−0·3
Oct. 12·14	194·14	20·3	·268	+0·2
Nov. 5·02	218·02	22·8	·343	+0·5
Dec. 21·98	264·98	26·1	·490	0·0
2002 Jan. 15·89	52289·89	28·7	2·568	+0·8
28·74	302·74	29·0	·608	+0·2
Feb. 16·86	321·86	29·8	·668	−0·2
Mar. 7·78	340·78	31·3	·728	+0·4
Apr. 3·83	367·83	31·6	·812	+0·1
Aug. 29·17	515·17	20·2	3·274	−0·1
Sept. 28·07	545·07	23·2	·368	+0·2
Oct. 18·07	565·07	24·6	·431	−0·1
Nov. 7·06	585·06	25·7	·494	−0·5
2003 Jan. 5·02	52644·02	29·5	3·678	−0·6
17·89	656·89	30·6	·719	−0·2
Feb. 13·87	683·87	31·4	·803	−0·1
19·85	689·85	31·2	·822	−0·3
Mar. 15·86	713·86	30·0	·897	+0·2
Apr. 4·85	733·85	25·1	·960	−0·3
Aug. 31·12	882·12	24·7	4·425	+0·2
Oct. 8·10	920·10	27·3	·544	−0·1
18·08	930·08	28·0	·575	−0·1
Nov. 4·08	947·08	28·9	·629	−0·3
27·01	970·01	30·8	·701	+0·3
Dec. 15·97	988·97	+31·5	·760	+0·2

TABLE I (*concluded*)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O – C) km s⁻¹*
2004 Jan. 14·99	53018·99	+31·0	4·854	−0·1
Feb. 8·90	043·90	27·9	·932	+0·2
22·88	057·88	23·8	·976	0·0
Mar. 1·88	065·88	21·4	5·001	+0·1
Sept. 5·12	253·12	28·5	·588	+0·1
21·14	269·14	29·5	·638	+0·1
Oct. 6·11	284·11	30·4	·685	+0·1
Nov. 5·04	314·04	31·5	·779	+0·1
Dec. 16·89	355·89	29·5	·910	+0·4
31·93	370·93	25·7	·958	+0·1
2005 Jan. 22·00	53392·00	19·1	6·024	−0·3
30·97	400·97	17·2	·052	−0·3
Mar. 17·84	446·84	17·8	·196	−0·2
2006 Mar. 3·80	53797·80	20·9	7·296	0·0
Sept. 8·15	986·15	30·6	·887	+0·4
Oct. 27·05	54035·05	18·3	8·040	+0·1
Dec. 6·12	075·12	17·2	·166	0·0
2007 Sept. 15·14	54358·14	18·0	9·053	+0·5
2008 Feb. 12·83	54508·83	27·1	9·525	+0·1
2010 Nov. 27·03	55527·03	29·8	12·718	−1·0
2012 Jan. 10·91	55936·91	21·0	14·003	−0·2
13·84	939·84	20·1	·012	−0·2
Sept. 18·14	56188·14	+31·4	·791	−0·1

*Objective-prism measurement[20]; weight 0.
†Observed by Lopez-Santiago *et al.*[23]; wt. 0.

scatter diagram. There is no clearly defined Li–temperature trend at all." Their paper concludes with the sentence, "The Hyades Moving Group does not appear to be a cohesive or coeval group linked with the Hyades, as judged by the scatter in the Li and the Fe abundances."

The literature does not offer for HD 17922 many other entries that are of interest here. Fehrenbach *et al.*[20] gave three radial velocities that were made by their objective-prism method and are not of service here, although they are included in Table I. Wichmann, Schmitt & Hubrig included the object in a paper[21] on 'Nearby young stars'; they found a significant Li I line (E.W. 99 mÅ), and gave an undated radial velocity of +24·6 km s^{-1}, stating also that their velocities had "errors typically 2–3 km s^{-1}", and a $v \sin i$ of < 10 km s^{-1}. Nordström *et al.*[22] gave the radial velocity as +26·9 ± 3·1 km s^{-1} as a mean of two *Coravel* measurements, and $v \sin i = 10$ km s^{-1}. The large uncertainty of the former value indicates a significant discordance that identified the object for the first time as a spectroscopic binary, and implies that the two individual velocities would be represented by the selfsame expression as the mean; the dates, however, are not available. There is just one dated radial-velocity observation in the literature, in a paper[23] whose abstract, only, was printed in *A&A*; it is included in Table I here, adjusted by +0·8 km s^{-1} in an effort to match the idiosyncrasy of the Cambridge zero-point normally adopted in this series of papers.

The same paper also gave a $v \sin i$ of 11·48 ± 0·70 km s^{-1} and a Li I equivalent width of 93·8 ± 0·8 mÅ.

An orbit was presented for HD 17922, on the basis of the first 22 of the Cambridge radial-velocity measurements listed in Table I, in the Suchkov paper[1]. That paper was written after an observing campaign that had lasted only two years, so the orbits of objects whose periods were too long to be followed completely round in a single observing season inevitably had seriously impaired phase distributions (if indeed they could be determined at all). For that reason, all objects whose periods were longer than 280 days — half of the 24 stars for which orbits could be given in the paper[1], plus several for which they could not — were retained on the observing programme with a view to the provision of fully reliable orbits subsequently. HD 17922, with a period given then as about 315 days (319 is the revised value now), was one of those stars. The number of observations has now tripled, to 67; the period is about ⅞ of a year, so the phases migrate round the calendar in a cycle of seven years during which there are eight revolutions of the orbit, making it easy to achieve a quasi-uniform phase coverage in such an interval of time. The orbital elements are presented in Table II below, which also shows the preliminary elements proposed in the Suchkov paper[1]; the corresponding velocity curve is plotted together with the data points in Fig. 1. The one serviceable measurement in the literature[23] was not utilized in the solution of the orbit, but is included in the table and figure and actually agrees 'exactly' with the orbit.

TABLE II

Orbital elements for HD 17922

Element	*Preliminary*[1]	*Final*
P (days)	314·8 ± 1·6	318·93 ± 0·08
T (MJD)	52113·4 ± 2·9	52746·6 ± 1·1
γ (km s^{-1})	+24·74 ± 0·13	+24·75 ± 0·04
K (km s^{-1})	7·26 ± 0·19	7·58 ± 0·07
e	0·310 ± 0·013	0·336 ± 0·007
ω (degrees)	117 ± 4	109·0 ± 1·5
$a_1 \sin i$ (Gm)	29·9 ± 0·8	31·32 ± 0·29
$f(m)$ ($M_\odot$)	0·0108 ± 0·0009	0·0121 ± 0·0003
R.m.s. residual (km s^{-1})	0·24	0·31

Of course the two values for T refer to different epochs of periastron; they differ by 633 days, which evidently represents two cycles of the orbit, but is not straightforward to interpret as it is also affected by the difference in the values of ω. It is admitted that the honest juxtaposition of the two sets of elements here does rather draw attention to the fact that they do not agree as well as might be hoped, particularly in respect of the period, for which the preliminary value is seen to be adrift by more than 2·5 times its asserted standard deviation. K, e, and ω all differ by between 1·5 times and twice the standard deviation of their differences. Although the discrepancies are a bit disappointing, they should probably not be viewed in too cynical a light, but merely as a reminder of something that we ought already to know, *viz.*, that formal standard deviations are apt to be optimistic when the bases upon which they are computed — which implicitly include an assumption that the data are well distributed in phase — are not fulfilled. In the preliminary orbit[1] of HD 17922, about one-third of the cycle is altogether innocent of data points.

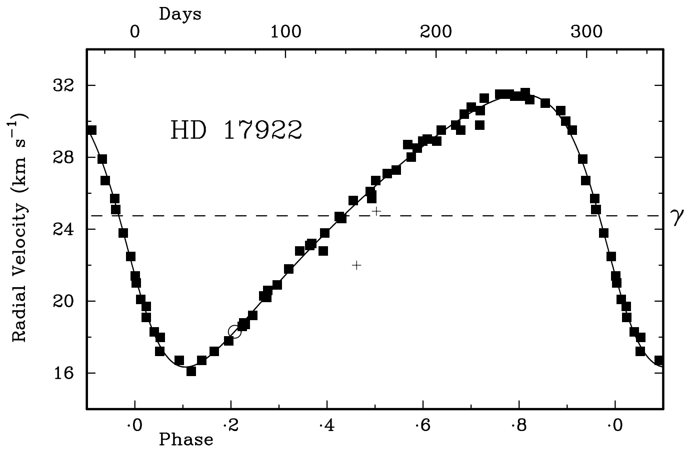

FIG. 1

The observed radial velocities of HD 17922 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The Cambridge observations, upon which the orbit is exclusively based, are plotted as filled squares. The two plusses are two of the three objective-prism measurements[20], the third being far off the top of the diagram at +45 km s^{-1}. The open circle represents the single measurement referred to by Lopez-Santiago *et al.*[23], which was not utilized in the orbital solution although it is obviously in full agreement with it.

The mass of the observed component of the binary can be reliably estimated from its spectral type at 1·2 $M_{\odot}$★, enabling the mass function to be interpreted as demonstrating that the minimum mass for the secondary is about 0·27 $M_{\odot}$, corresponding to that of a star well down the M-dwarf sequence, about M4; it is far from surprising that it has not been seen in the radial-velocity traces. Those traces also provide rotational-velocity estimates, which repeat very well from one trace to another – the extreme range of the $v \sin i$ values (which are quantized in ½-km s^{-1} steps) among the 67 traces is only from 8½ to 11½ km s^{-1}. The formal mean is 9·90 ± 0·10 km s^{-1}, but the rather cavalier disregard, in the derivation of rotational velocities from the traces, of other potential sources of line-broadening makes it unwise to claim an external accuracy better than 1 km s^{-1}, so the result obtained here is best represented as 10 ± 1 km s^{-1}.

HD 78899

This $7^{m}·6$ star is to be found in Ursa Major, in the second-most-northerly of the south-preceding corners of the constellation (of which there are no fewer than six!), very close to the midpoint of the triangle θ, ι, κ UMa that delineates the Bear's left forepaw in the classical constellation pictures.

There is surprisingly little information about the object in the literature. Sky-survey pictures (including the one reproduced as a thumbnail illustration in the *Simbad* bibliography) show a faint companion star about 20″ distant in a position angle of about 62°; Metchev & Hillenbrand[24], who carried out an adaptive-optics survey for close companions, noted a still fainter one at about 8″

★Such a mass is also tolerably representative of the choice of 14 values that confront the bemused reader of a table referred to in ref. 23.

and 77°. They also gave the seemingly conflicting properties of a spectral type of K2 V and a mass of 1·1 $M_\odot$ for HD 78899. White, Gabor & Hillenbrand[4] took an échelle spectrum and reported that the star is double-lined; they gave equivalent widths for the lines of the two components for Hα (appreciably different from one another), Ca I λ6717 Å (a factor of 1⅓ between the components), and Li I (a factor of more than four). Nordström *et al.*[22], however, reported a mean of +13·7 ± 0·1 km s^{-1} from two radial-velocity observations obtained about a year apart; the actual dates are not available. Evidently the system did not appear double-lined to them. The projected rotational velocity was given as 7 km s^{-1}.

The things that are known about HD 78899 with reasonable certainty are its parallax and its photometry. The former[17], of 27·96 ± 0·78 arc-milliseconds, corresponds to a distance modulus of $2^{m}\cdot77 \pm 0^{m}\cdot06$. The latter is known only from *Tycho*: the *Tycho 2*[25] V and $(B - V)$ are $7^{m}\cdot65$ and $0^{m}\cdot80$, respectively. From the V magnitude and the modulus we obtain the M_V as $+4^{m}\cdot88$.

It was only in 2013 February that the writer belatedly noticed that HD 78899 had been discovered to be double-lined[24]; but no orbit had been proposed for it, so he put it on the Cambridge observing programme. The star's place in the sky is convenient, as it passes just south of the Cambridge zenith; the later observations, however, had to be made in the dusk at increasingly heroic westerly hour angles, reaching more than six hours in the case of the final one. The object demonstrated its duplicity anew at the very first observation, when the radial-velocity trace exhibited two mutually similar 'dips' a long way — about 80 km s^{-1} — apart. Such a trace is illustrated in Fig. 2.

The system soon proved to have a short period, almost exactly 20 days, but nevertheless to have a very eccentric orbit. There are now 35 Cambridge observations, all reduced as double-lined, to be entered in Table III and used in the determination of the orbit. It is unusual for the writer to offer an orbit on the basis of just a single season's observations, which in this case cover barely four months. All the same, they define the orbit, whose elements are given in the informal table below, just as well as many longer campaigns have done in the cases of stars that have more leisurely orbits, for many of which it is necessary for reasons of human longevity to be content with seeing just a single orbital cycle*. The orbit is plotted in Fig. 3.

P	=	20·0053 ± 0·0022 days	$(T)_4$	=	MJD 56398·8603 ± 0·0034
γ	=	+15·87 ± 0·04 km s^{-1}	$a_1 \sin i$	=	10·471 ± 0·024 Gm
K_1	=	48·61 ± 0·10 km s^{-1}	$a_2 \sin i$	=	10·502 ± 0·024 Gm
K_2	=	48·76 ± 0·10 km s^{-1}	$f(m_1)$	=	0·1146 ± 0·0008 $M_\odot$
q	=	1·0030 ± 0·0028 (= m_1/m_2)	$f(m_2)$	=	0·1156 ± 0·0008 $M_\odot$
e	=	0·6220 ± 0·0011	$m_1 \sin^3 i$	=	0·4611 ± 0·0027 $M_\odot$
ω	=	275·45 ± 0·14 degrees	$m_2 \sin^3 i$	=	0·4597 ± 0·0027 $M_\odot$

R.m.s. residual (unit weight) = 0·31 km s^{-1}

Owing to the exactitude with which the period approximates to an integral number of days (20 days), uniform phase coverage of the orbit could be obtained on a microscopic scale only after a lapse of time nearly sufficient to allow the 20 phasings corresponding to night-time at the observer's longitude to

*Indeed, the writer's observing programme includes many stars for which he cannot hope to see even *that*; deciding how late he dare leave it before writing up what he knows about them has some analogies with a game of Russian roulette! — but arrangements exist to make unpublished material accessible to interested parties if the game is lost.

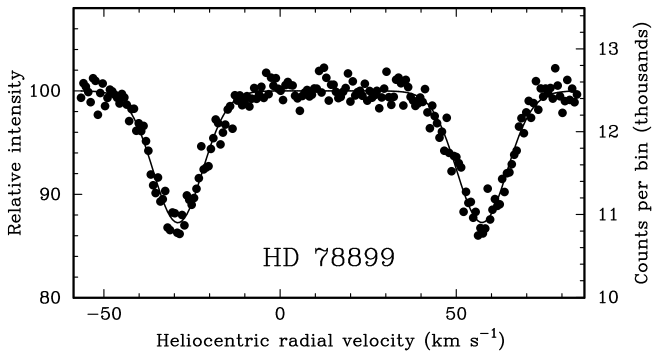

FIG. 2

Cambridge radial-velocity trace of HD 78899, obtained on 2013 April 15 and illustrating the twin dips at nearly their maximum separation (phase ·95).

migrate by one day — something like ten years — or alternatively by additional observations made systematically elsewhere at suitably distributed longitudes (a rather unrealistic proposition). Already, however, observations are available at every one of the 20 currently accessible phasings, and infilling between them could do nothing more than improve (or, much more readily, spoil!) the already-agreeable cosmetic appearance of the orbit shown in Fig. 3.

In the derivation of the orbit, all observations were given the same weight except for four obtained on the nights adjacent to the cross-over of the velocities near phase ·5, when the two dips were very heavily blended together, and one

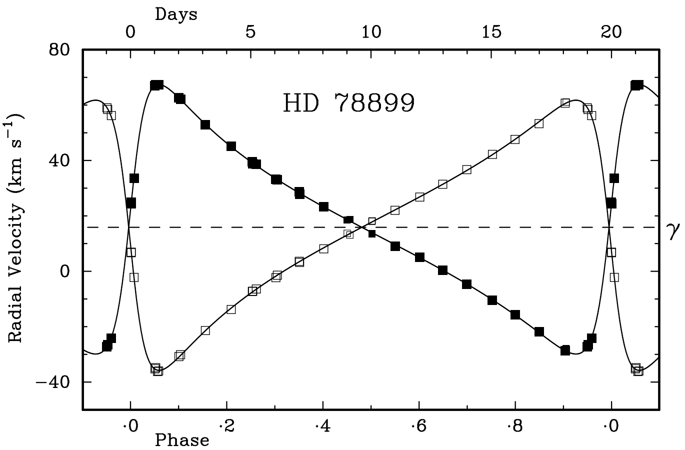

FIG. 3

As Fig. 1, for HD 78899. The agreeably uniform spacing of the points in terms of phase happened unavoidably as a consequence of the exactitude of the period to the integer number 20 days. All the observations are from the Cambridge *Coravel*, and indeed all were obtained in just half of a single observing season in 2013 February–June. Smaller symbols at days 9, 10, and (less obviously) 18 denote measurements given half-weight in the solution of the orbit.

TABLE III

Radial-velocity observations of HD 78899

All the observations were made with the Cambridge Coravel

Date (UT)		MJD	Velocity Prim. km s^{-1}	Velocity Sec. km s^{-1}	Phase	(O–C) Prim. km s^{-1}	(O–C) Sec. km s^{-1}
2013 Feb.	15·03	56338·03	−24·1	+56·2	0·959	−0·3	+0·5
Mar.	5·97	356·98	−27·8:	+61·1:	1·906	+1·1	+0·4
	12·03	363·03	+45·2	−13·8	2·209	+0·3	−0·5
	13·07	364·07	+38·6	−6·4	·261	+0·4	+0·1
	13·95	364·95	+33·1	−1·4	·305	0·0	0·0
	29·01	380·01	+67·4	−36·0	3·058	0·0	−0·2
	30·96	381·96	+52·9	−21·4	·155	−0·2	+0·1
Apr.	1·91	383·91	+39·5	−7·3	·253	+0·3	+0·2
	2·90	384·90	+33·2	−2·3	·302	−0·2	−0·6
	5·87	387·87	+18·5:	+13·7:	·450	−0·2	+0·6
	6·91	388·91	+13·5:	+18·2:	·502	−0·4	+0·4
	15·86	397·86	−27·3	+59·0	·950	−0·1	−0·1
	16·87	398·87	+24·9	+6·7	4·001	+0·5	−0·6
	17·97	399·97	+67·3	−36·2	·056	0·0	−0·5
	18·86	400·86	+62·6	−30·8	·100	0·0	+0·2
	27·86	409·86	+9·1	+22·0	·550	−0·5	−0·1
	29·85	411·85	+0·4	+31·4	·649	0·0	0·0
	30·85	412·86	−4·6	+36·7	·700	0·0	+0·3
May	1·92	413·92	−10·5	+42·2	·753	−0·2	+0·1
	2·86	414·86	−15·6	+47·6	·800	+0·2	0·0
	3·86	415·86	−21·8	+53·3	·850	+0·3	−0·7
	4·95	416·95	−28·7	+60·7	·904	0·0	+0·1
	5·90	417·90	−26·5	+58·4	·952	+0·3	−0·2
	6·88	418·88	+24·4	+6·9	5·001	−0·2	−0·2
	7·00	419·00	+33·6	−2·2	·007	−0·6	+0·3
	7·87	419·87	+67·0	−35·2	·050	+0·1	+0·1
	8·94	420·94	+62·0	−30·1	·103	0·0	+0·3
	11·94	423·94	+38·9	−7·2	·254	−0·2	+0·2
	13·88	425·88	+28·8	+3·6	·351	+0·5	+0·2
	15·99	427·99	+18·7:	+13·1:	·456	+0·6	−0·5
	16·90	428·90	+13·6:	+18·0:	·502	−0·4	+0·2
June	2·90	445·90	+27·9	+3·2	6·351	−0·3	−0·3
	3·91	446·91	+23·2	+8·1	·402	0·0	−0·4
	7·91	450·91	+5·0	+26·8	·602	+0·1	−0·1
	16·91	459·91	+67·3	−34·8	7·052	+0·2	+0·7

other (the second) observation, which was severely curtailed by cloud and would perhaps be better disregarded altogether; those five have been attributed half-weight. It will be noticed from the table that, although the mass ratio is determined to within three parts in a thousand, it is still not possible to identify with certainty which component is the primary. The rotational velocities of the two stars are also found to be identical, the formally computed mean for each of them being $5·3 \pm 0·4$ km s^{-1}.

As far as the observations described here can tell (and despite the differences asserted by White *et al.*[4]), the system is composed of a pair of extraordinarily similar stars. Each must have an absolute magnitude very close to $5^{m}·6$, corresponding normally to a spectral type of G8 V, a type that is also exactly consonant with the observed colour index. The writer is therefore inclined to dismiss as unlikely the type of K2 V asserted (from no clear origin) by Metchev & Hillenbrand[24], and his admittedly subjective doubts about the spectral inequalities reported by White *et al.*[4] (an overlapping syndicate) are inevitably reinforced. Despite the proposal by the former authors of a mass of 1·1 $M_\odot$ (whether for one star or two is not clear from their paper), it seems reasonable here to postulate masses of about 0·85 $M_\odot$, representative of the values commonly accepted for stars of type G8 V, for both components. Then, from the values, found from the orbital elements, of about 0·46 $M_\odot$ for $m_{1,2} \sin^3 i$, we obtain $\sin^3 i \sim 0{\cdot}54$, $\sin i \sim 0{\cdot}81$. The rotational velocities of the stars, freed from the $\sin i$ factor, thus become about 6·5 km s^{-1}. Rotation of a G8 main-sequence star, whose radius is about 0·88 $R_\odot$, in the 20-day orbital period would require an equatorial velocity of 2·2 km s^{-1}; in an orbit with the eccentricity of this one, 0·62, however, the pseudo-synchronous rotation period[26] is shorter than the orbital one by a factor of about 4·4, leading to an expected equatorial velocity close to 10 km s^{-1}. That is decidedly greater than the 6·5 km s^{-1} implied by observation; it is rather tantalizing that the two values should be of such similar order, but the underlying numbers are sufficiently definite that there does not seem to be any plausible way of reconciling them. It is necessary to conclude that the stars are both rotating at only about two-thirds of the angular velocity demanded by pseudo-synchronism. Experts in stellar theory and evolution may be able to assess whether there is a possibility that the orbital eccentricity could 'recently' have increased from the value (about 0·52) at which the observed rotational velocities would be pseudo-synchronous, on a time-scale too short for them to follow.

It is of interest that the combination of short period and high eccentricity in the orbit of HD 78899 places it outside the limiting envelope of the points shown in Fig. 1 of the the most recent synopsis[27] of the papers in this series — an e–($\log P$) diagram with 291 data points plotted, each representing one orbit published in the papers numbered 1–200 of the series. The highest eccentricity previously found in these papers for a binary with a period less than HD 78899's 20 days is[28] that of HD 4271, with $e \sim 0{\cdot}53$ at a period of only 11·4 days. HD 83509[29] has an eccentricity of 0·6232 ± 0·0014 — the same as that of HD 78899 within their uncertainties — at a period of 25·6 days; but decidedly higher eccentricities of 0·67 and 0·73 have been found[30,31] for HD 113638 and HD 213014 B at periods of 27·86 and 27·85 days, respectively.

HD 103613

HD 103613 is quite a bright object, brighter than the seventh magnitude, located in a visually rather barren region of the sky in southern Ursa Major. It is (just) within the North Galactic Pole field, as defined by Yoss & Griffin[32] as the area at $b > +75°$; those authors published a comprehensive catalogue of photometry and radial velocity for all the late-type (G5–M) *HD* stars in that field, but HD 103613 did not qualify for inclusion because its *HD* type[33] of F5 was too early.

V magnitudes close to $6^{\rm m}{\cdot}75$ have been given for the star by a number of authors[34–36,21], mostly in a context of Strömgren photometry; only one[36] of the papers has a $(B-V)$ value, $0^{\rm m}{\cdot}337$, and *that* one is none too readily retrieved because it is not listed in the *Simbad* bibliography, but it is referred to by Eggen[37] who misprinted its reference. Feltz[34] deduced from the Strömgren indices an M_V of $+3^{\rm m}{\cdot}6$ for HD 103613; Hill *et al.*[35] obtained $3^{\rm m}{\cdot}3$. Of course they did not know that they were dealing with a multiple star. One might surmise that the admixture in HD 103613 of the light of the principal F star with that of a relatively faint companion further down the main sequence would cause the Strömgren indices to under-estimate the luminosity, because (*a*) the adulterated spectrum may broadly mimic that of a slightly later and less-luminous star[3], and (*b*) a direct effect of the duplicity is to raise the luminosity of the system modestly above what would be appropriate to a single star.

The parallax[17] gives the distance of the system as 70 ± 3 pc, and shows the true absolute magnitude to be $+2^{m}\cdot45 \pm 0^{m}\cdot10$. Although *Hipparcos* did not recognize the star as double, Fabricius & Makarov[38] managed subsequently (2000) to resolve it from the archive of *Hipparcos* records, deducing a separation of $0''\cdot154$ between components having magnitudes of 7·003 and 9·127 on the '*Hp*' system, which is not too far from *V*. It is hard to believe that the magnitudes are good enough to warrant three decimals, but at least we can accept that they indicate a ΔV near to $2^{m}\cdot1$. Those authors also gave a position angle, to a hundredth of a degree, as if there were absolutely no motion during the three years that *Hipparcos* was watching. What galvanized them into trying to resolve the system from the *Hipparcos* records may well have been the discovery in 1997 by Mason *et al.*[39], from speckle interferometry with the Mount Wilson 100-inch reflector, of its 'visual' duplicity. They (Fabricius & Makarov) particularly remarked, concerning another system, also first resolved by Mason *et al.*, among the small number of retrospective *Hipparcos* resolutions that they achieved, that *that* system was "of particular interest" because it had shown a change of position angle of 36° in the six years between the middle of the *Hipparcos* era and the Mason *et al.* observation. The corresponding change in the position angle of HD 103613 appeared to be only about 8°, although Mason *et al.* had put forward, on the basis of their single observation and a whole set of crude but reasonable assumptions, an indicative orbital period of only 15 years — which will shortly be seen to be remarkably near the truth. The only explanation that comes to mind for the smallness of the apparent change in position angle is that the innate ambiguity in the speckle measure concealed a change that was really 180 ± 8 degrees*.

There are radial velocities for HD 103613 in the literature, but they are not helpful for present purposes. Adamson *et al.*[40] made eight measurements with the DAO 72-inch reflector and a photographic spectrograph giving a reciprocal dispersion of 78 Å mm^{-1} and an r.m.s. velocity error of 12·2 km s^{-1} per observation! Their result is given only as mean, of $-7\cdot9 \pm 4\cdot3$ km s^{-1} (their quoted standard error is slightly smaller than the $12\cdot2/\sqrt{7}$ that one might expect). Fehrenbach *et al.*[20] made four measurements by their objective-prism method; they agree with one another unusually well, giving a mean of $-22 \pm 1\cdot5$ km s^{-1}, but are still not serviceable here. Finally, Nordström *et al.*[22] referred to a table giving one measurement made with the Haute-Provence *Coravel*, which gave a result of $-18\cdot0$ km s^{-1} with the extraordinary standard error of 8·0 km s^{-1}; despite there being only one observation the star is flagged as a spectroscopic binary, which presumably must have been divined by those authors from the 'dip' being double. They also listed a $v \sin i$ of 10 km s^{-1}.

HD 103613 was placed on the Cambridge radial-velocity programme in 2000, when it was included in a list of 'over-luminous' F stars that was provided privately by Suchkov. Quite soon afterwards, that list formed the basis of a substantial publication[1] showing that the Suchkov–McMaster criterion of 'over-luminosity'[2] did indeed — as had been claimed — often identify stars whose luminosity was enhanced by duplicity. One of the 'successes' in that connection was HD 103613, which was found at the first observation to be double-lined, though with very unequal components. Already at the second observation, the radial velocities showed it to be a triple system: the two 'dips' in the traces did not move in anti-phase with one another. The main one proved

*Dr. Mason has kindly confirmed that reversing the p.a. of his syndicate's speckle measure does indeed permit a 'visual' orbit, with elements close to those of the spectroscopic one given here, to be derived.

TABLE IV

Radial-velocity observations of HD 103613

All the observations were made with the Cambridge Coravel but those obtained in 2000–2002 have been weighted ⅓

Date	*MJD*	*Velocity* A	*Velocity* Ba	*Phase*	*(O–C)* A	*Phase*	*Computed vel.* outer	*Computed vel.* inner	*(O–C)* Ba
		km s⁻¹	*km s⁻¹*	*(outer)*	*km s⁻¹*	*(inner)*	*km s⁻¹*	*km s⁻¹*	*km s⁻¹*
2000 Feb. 20·08	51594·08	−3·6	+6·6	0·317	−0·4	0·282	−6·5	+12·8	+0·2
Apr. 8·00	642·00	−4·0	+2·2	·327	−0·5	2·361	−6·2	+7·9	+0·6
Dec. 2·27	880·27	−5·1	−19·2	·374	−0·3	12·698	−5·1	−12·0	−2·1
2001 Feb. 14·05	51954·05	−5·1	—	0·389	+0·1				
Mar. 5·06	973·06	−5·5	−19·4	·393	−0·2	16·723	−4·7	−13·6	−1·1
Apr. 28·96	52027·96	−5·6	+17·7	·404	0·0	19·105	−4·5	+23·3	−1·1
May 28·92	057·92	−5·1	+0·4	·410	+0·6	20·405	−4·3	+5·2	−0·5
June 24·91	084·91	−6·2	−12·5	·415	−0·3	21·576	−4·2	−4·7	−3·6
27·92	087·92	−6·8	−16·7	·416	−0·9	·706	−4·2	−12·5	0·0
30·92	090·92	−5·2	−22·7	·417	+0·7	·836	−4·2	−20·7	+2·2
Nov. 1·23	214·23	−6·6	+14·3	·441	−0·1	27·186	−3·7	+19·3	−1·3
Dec. 12·23	255·23	−7·5	−19·6	·449	−0·8	28·965	−3·5	−14·0	−2·1
14·28	257·28	−7·0	+15·2	·450	−0·3	29·054	−3·5	+19·4	−0·7
15·19	258·19	−7·0	+19·5	·450	−0·3	·093	−3·5	+23·3	−0·3
20·22	263·22	−6·6	+6·7	·451	+0·1	·311	−3·5	+11·0	−0·8
22·16	265·16	−6·4	−1·4	·451	+0·3	·395	−3·5	+5·8	−3·7
30·18	273·18	−8·0	−17·7	·453	−1·2	·743	−3·4	−14·9	+0·6
2002 Jan. 1·18	52275·18	−7·5	−25·6	0·453	−0·7	29·830	−3·4	−20·3	−1·9
2·17	276·17	−7·9	−26·8	·454	−1·1	·873	−3·4	−22·4	−1·0
3·13	277·13	−7·2	−25·0	·454	−0·4	·915	−3·4	−22·3	+0·7
31·16	305·16	−7·1	+18·1	·460	−0·2	31·131	−3·3	+22·5	−1·1
Feb. 4·16	309·16	−7·1	+6·5	·460	−0·2	·304	−3·3	+11·4	−1·6
7·06	312·06	−6·8	+0·7	·461	+0·2	·430	−3·3	+3·7	+0·2
14·10	319·10	−6·7	−16·5	·462	+0·3	·735	−3·3	−14·4	+1·1
16·05	321·05	−6·9	−22·5	·463	+0·1	·820	−3·2	−19·7	+0·5
21·15	326·15	−7·1	+14·7	·464	−0·1	32·041	−3·2	+17·1	+0·8
23·13	328·13	−6·4	+19·6	·464	+0·6	·127	−3·2	+22·7	+0·2
23·98	328·98	−7·0	+16·3	·464	0·0	·164	−3·2	+20·7	−1·2
27·05	332·05	−6·9	+6·9	·465	+0·2	·297	−3·2	+11·9	−1·7
Mar. 1·07	334·07	−7·1	+2·9	·465	0·0	·385	−3·2	+6·4	−0·3
2·07	335·07	−6·8	+1·2	·466	+0·3	·428	−3·2	+3·8	+0·5
10·04	343·04	−7·0	−20·0	·467	+0·1	·774	−3·2	−16·8	0·0
29·00	362·00	−6·5	−10·3	·471	+0·7	33·597	−3·1	−5·9	−1·3
30·06	363·06	−7·3	−11·9	·471	−0·1	·643	−3·1	−8·6	−0·2
Apr. 4·01	368·01	−6·6	−23·3	·472	+0·6	·857	−3·1	−21·7	+1·5
4·95	368·95	−7·0	−24·9	·472	+0·2	·898	−3·1	−22·7	+0·9
5·97	369·97	−7·0	−21·9	·472	+0·2	·942	−3·1	−19·4	+0·6
6·96	370·96	−6·1	−12·0	·473	+1·1	·985	−3·1	−6·0	−3·0
11·00	375·00	−7·3	+17·0	·474	−0·1	34·161	−3·0	+20·9	−0·9
24·97	388·97	−6·2	−17·9	·476	+1·1	·767	−3·0	−16·3	+1·4
May 1·97	395·97	−7·3	+19·3	·478	0·0	35·070	−3·0	+22·1	+0·1
29·89	423·89	−6·8	+11·9	·483	+0·7	36·282	−2·9	+12·9	+1·9
July 3·91	458·91	−8·2	−22·2	·490	−0·6	37·801	−2·7	−18·5	−0·9
2003 Jan. 11·17	52650·17	−8·7	+21·3	0·529	−0·4	46·098	−2·1	+23·3	+0·1
30·08	669·08	−8·7	−23·9	·532	−0·3	·918	−2·1	−22·1	+0·3
Feb. 22·07	692·07	−8·4	−23·0	·537	+0·1	47·916	−2·0	−22·2	+1·2
Mar. 1·13	699·13	−8·3	+14·1	·538	+0·2	48·222	−2·0	+16·8	−0·8
16·98	714·98	−8·3	−24·9	·542	+0·3	·910	−1·9	−22·5	−0·5
28·01	726·01	−8·1	+6·0	·544	+0·5	49·388	−1·9	+6·2	+1·7
Apr. 6·97	735·97	−8·6	−21·7	·546	0·0	·820	−1·9	−19·7	−0·1
16·94	745·94	−8·7	+12·9	·548	−0·1	50·253	−1·8	+14·8	0·0
17·93	746·93	−8·6	+11·0	·548	+0·1	·296	−1·8	+12·0	+0·9

TABLE IV (*concluded*)

Date	*MJD*	*Velocity* *A* km s⁻¹	*Velocity* *Ba* km s⁻¹	*Phase (outer)*	*(O – C)* *A* km s⁻¹	*Phase (inner)*	*Computed vel.* *outer* km s⁻¹	*Computed vel.* *inner* km s⁻¹	*(O – C)* *Ba* km s⁻¹
2003 May 6·00	53765·00	−8·6	+20·7	·552	+0·1	51·080	−1·8	+22·8	−0·3
6·90	765·90	−8·6	+20·6	·552	+0·1	·119	−1·8	+23·0	−0·6
19·92	778·92	−8·6	−12·6	·554	+0·1	·684	−1·8	−11·1	+0·3
Dec. 8·27	981·27	−9·3	+1·1	·595	0·0	60·462	−1·3	+1·9	+0·5
2004 Feb. 26·10	53061·10	−9·8	−22·2	0·611	−0·4	63·925	−1·2	−21·6	+0·6
Mar. 1·07	065·07	−9·5	+22·6	·612	−0·1	64·097	−1·2	+23·3	+0·5
Apr. 14·99	109·99	−9·4	+17·0	·621	+0·1	66·046	−1·1	+18·2	−0·1
May 7·00	132·00	−9·6	+1·1	·625	−0·1	67·001	−1·1	+1·3	+0·9
Nov. 14·26	323·26	−9·9	+9·2	·664	−0·3	75·298	−1·0	+11·8	−1·6
Dec. 26·25	365·25	−10·1	+24·2	·672	−0·5	77·120	−1·0	+22·9	+2·3
2005 Jan. 19·21	53389·21	−9·9	+21·8	0·677	−0·3	78·159	−1·0	+21·0	+1·8
Mar. 25·03	454·03	−9·4	−12·9	·690	+0·2	80·971	−1·0	−11·7	−0·2
May 10·94	500·94	−9·4	+3·0	·699	+0·2	83·006	−1·1	+3·9	+0·2
12·02	502·02	−9·4	+18·2	·699	+0·2	·053	−1·1	+19·8	−0·5
June 27·91	548·91	−9·3	+21·9	·709	+0·2	85·087	−1·1	+23·1	−0·1
July 17·89	568·89	−10·0	−18·0	·713	−0·5	·954	−1·1	−16·9	+0·1
Dec. 17·25	721·25	−8·6	−6·3	·743	+0·6	92·564	−1·4	−4·0	−0·9
2006 Feb. 16·08	53782·08	−8·8	+16·9	0·756	+0·2	95·203	−1·5	+18·2	+0·3
Apr. 8·98	833·98	−8·6	+1·1	·766	+0·2	97·454	−1·7	+2·3	+0·5
May 15·96	870·96	−8·9	+18·6	·773	−0·2	99·059	−1·8	+20·7	−0·3
June 27·91	913·91	−8·7	−24·5	·782	−0·2	100·922	−1·9	−21·7	−0·9
July 2·93	918·93	−8·3	+19·4	·783	+0·2	101·140	−2·0	+22·1	−0·7
2007 Apr. 5·02	54195·02	−7·1	+19·9	0·838	0·0	113·117	−3·1	+23·0	0·0
May 18·94	238·94	−6·5	+8·8	·847	+0·4	115·023	−3·4	+11·0	+1·2
June 10·96	261·96	−6·5	+5·7	·852	+0·2	116·021	−3·5	+10·4	−1·2
July 7·91	288·91	−6·6	+13·8	·857	−0·1	117·190	−3·6	+19·0	−1·6
Nov. 16·23	420·23	−6·6	−29·5	·884	−1·0	122·887	−4·4	−22·7	−2·4
2008 Jan. 25·17	54490·17	−5·4	−26·5	0·898	−0·3	125·922	−4·8	−21·9	+0·3
Apr. 23·98	579·98	−4·2	−25·2	·916	+0·3	129·818	−5·4	−19·6	−0·2
May 18·96	604·96	−4·5	−29·0	·921	−0·2	130·902	−5·6	−22·7	−0·7
Dec. 7·29	807·29	−3·2	−17·4	·961	−0·4	139·679	−6·8	−10·8	+0·3
2009 Feb. 17·03	54879·03	−2·1	−25·0	0·975	+0·2	142·792	−7·3	−17·9	+0·2
Apr. 7·95	928·95	−2·0	−22·6	·985	0·0	144·957	−7·5	−16·2	+1·1
Apr. 29·92	950·92	−1·6	−31·0	·990	+0·2	145·910	−7·7	−22·5	−0·8
May 23·92	974·92	−1·5	−25·0	·995	+0·2	146·952	−7·8	−17·6	+0·4
Dec. 21·24	55186·24	−0·5	+14·4	1·037	0·0	156·119	−8·8	+23·0	+0·2
2010 Jan. 31·13	55227·13	−0·1	−31·9	1·045	+0·3	157·893	−8·9	−22·7	−0·3
Feb. 21·10	248·10	−0·5	−28·5	·049	−0·2	158·803	−9·0	−18·7	−0·9
Mar. 23·02	278·02	0·0	+14·1	·055	+0·2	160·101	−9·1	+23·3	−0·1
Apr. 8·95	294·95	0·0	−29·7	·059	+0·1	·836	−9·1	−20·6	0·0
July 9·91	386·91	+0·2	−29·0	·077	+0·1	164·825	−9·3	−20·0	+0·3
Dec. 12·22	542·22	+0·1	−12·5	·108	−0·2	171·563	−9·5	−3·9	+0·9
2011 May 9·94	55690·94	+0·1	−1·5	1·138	−0·2	178·015	−9·5	+7·7	+0·2
2012 Jan. 13·23	55939·23	−0·3	−26·9	1·188	−0·1	188·787	−9·0	−17·6	−0·3
Feb. 11·12	968·12	−0·1	+6·9	·194	+0·2	190·040	−8·9	+16·8	−0·9
Apr. 15·94	56032·94	−0·3	−30·7	·207	+0·3	192·852	−8·7	−21·5	−0·5
Dec. 2·28	263·28	−1·9	−29·1	·253	−0·3	202·845	−7·9	−21·1	−0·1
2013 Feb. 15·08	56338·08	−1·5	+17·1	1·268	+0·4	206·090	−7·6	+23·2	+1·5
Apr. 15·91	397·91	−2·1	−17·0	·280	+0·1	208·686	−7·3	−11·2	+1·5
May 11·96	423·96	−2·4	−26·0	·285	0·0	209·816	−7·2	−19·5	+0·6
June 3·94	446·94	−2·6	−26·1	·290	−0·1	210·813	−7·1	−19·3	+0·3

to change its velocity only slowly; over the two-year duration of the original survey[1] it exhibited a monotonic decline of about 4 km s^{-1}. The subsidiary dip showed rapid changes, and the paper[1] was able to give preliminary elements for its surprisingly eccentric 23-day orbit. HD 103613 is the fifth interesting spectroscopically multiple system found by the writer in the North Galactic Pole field; the other four were all discussed in Paper 221[41] of this series two years ago. In all five cases there are only two observable components; four of the systems are triple and include one unseen star, but one (HD 117078) is quadruple and we see only the primaries of two binary systems that themselves constitute a wide binary.

There are now 103 radial velocities for HD 103613, all of which have been obtained with the Cambridge *Coravel*; they are set out in Table IV. The system has just about been seen round a full cycle of the outer orbit, which has a period close to 5000 days (nearly 14 years). The γ-velocity of the 23-day sub-system is shown duly to vary in anti-phase with the velocity of the primary star. The sub-system is single-lined: no evidence is seen in the radial-velocity traces of the third component, although the mass of the sub-system is considerably greater than that of the primary — a fact that proves troublesome in the discussion below. A trace that well illustrates the unequal dips appears here as Fig. 4. The orbits have been calculated with a program[42] that solves simultaneously the velocities of both components for the total of 12 elements of the two orbits, *viz.*, the 'grand' Γ-velocity, two amplitudes for the outer orbit and one for the inner one, and two periods, epochs, eccentricities, and longitudes of periastron. The elements are presented in Table V, and the orbits are shown in Figs. 5 and 6.

TABLE V

Orbital elements for HD 103613

Element	*Outer orbit*	*Inner orbit*
P (days)	4988 ± 53	23·05062 ± 0·00033
T (MJD)	55001 ± 109	53385·536 ± 0·033
Γ (km s^{-1})	−4·97 ± 0·04	
K_1 (km s^{-1})	5·00 ± 0·06	23·03 ± 0·14
K_2 (km s^{-1})	4·25 ± 0·16	
q	0·851 ± 0·034	
e	0·103 ± 0·011	0·484 ± 0·005
ω (degrees)	309 ± 8	271·4 ± 0·8
$a_1 \sin i$ (Gm)	341 ± 5	6·39 ± 0·04
$a_2 \sin i$ (Gm)	290 ± 11	
$f(m_1)$ ($M_\odot$)	0·064 ± 0·002	0·0196 ± 0·0004
$f(m_2)$ ($M_\odot$)	0·039 ± 0·004	
$m_1 \sin^3 i$ ($M_\odot$)	0·185 ± 0·010	
$m_2 \sin^3 i$ ($M_\odot$)	0·218 ± 0·009	
R.m.s. residual (km s^{-1})	0·28	0·8

In the solution of the orbits, the variances of the respective components' velocities have been approximately equalized by weighting the velocities of the secondary ⅛. The r.m.s. residual shown in the table above for the outer orbit is that of an observation of 'unit weight', *i.e.*, that of the primary star in the years from 2003 onwards; the one shown for the inner orbit is that of the secondary in the same interval, 'as observed' and not adjusted by any weighting. It is no surprise, therefore, that the two values are related by a factor of about $\sqrt{8}$. In the early part of the observing campaign (2000–2002), observations were made rather frequently in order to determine the short-period for publication

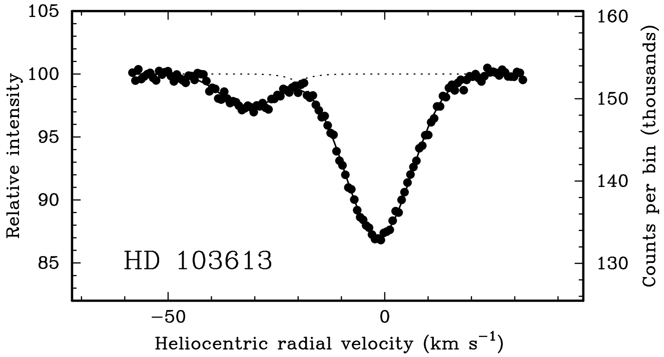

FIG. 4

Cambridge radial-velocity trace of HD 103613, obtained on 2010 April 8 and showing the two very unequal dips at almost the maximum possible velocity separation. All too often they are seriously blended together.

in ref. 1, but subsequent ones, made much less frequently with the main intention of determining the outer orbit whose period is some 200 times as long, have proved statistically to be more accurate by a factor of about $\sqrt{3}$, so the early ones have been weighted ⅓ in this final solution. The difference is possibly owed in part to increased patience on the part of the writer, and correspondingly longer integration times, when the star was observed less often!

Many of the radial-velocity traces show the dips more or less blended together, although in general the observer has refrained from observing the system near single-lined phases. Badly blended traces have been reduced with the widths of the dips and the ratio of dip areas fixed, at mean values obtained from the traces reduced with all parameters 'free'. The primary dip gives a formal mean $v \sin i$ of $9{\cdot}13 \pm 0{\cdot}10$ km s^{-1}, which for the reason given at the end of the section on HD 17922 above would be more realistically quoted as 9 ± 1. The object in the 23-day orbit has no measurable rotational velocity at all; that is a bit surprising, because it must be a star of something like solar type, and a star of solar radius rotating in pseudo-synchronism[26] with the quite eccentric 23-day orbit should have an equatorial velocity of about 6 km s^{-1}, although of course the projected value would be reduced by an amount depending upon the inclination of that orbit (or, more specifically, of the rotational axis, although the concept of synchronism hardly applies except where the rotational and orbital poles coincide).

The mean ratio of dip areas in the many traces reduced 'free' is 1 to $0{\cdot}188 \pm 0{\cdot}004$, which corresponds arithmetically to $1^{m}{\cdot}81 \pm 0^{m}{\cdot}03$ when expressed in terms of stellar magnitudes. According to the 'rule of thumb' adopted in ref. 1, the actual magnitude difference between the components is about 1·15 times as great, *viz.*, $2^{m}{\cdot}06$ (the factor represents the increase in line strength in the stellar spectra towards later spectral types). (We notice the almost embarrassingly close agreement with the ΔHp difference found by Fabricius & Makarov[38]!) Our own estimate of the magnitude difference depends somewhat on the implicit assumption that both stars are on the main sequence, and we cannot be certain that that is true. The total magnitude and joint colour index are, however, closely matched by a combination of the properties of F0 V and G2 V

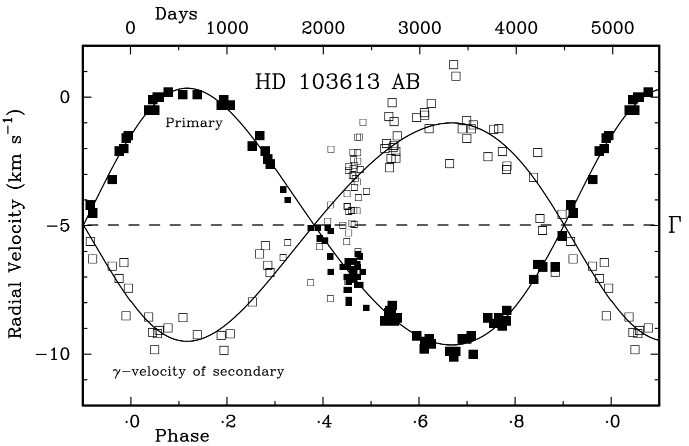

FIG. 5

The outer orbit of HD 103613. All the observations were made with the Cambridge *Coravel*, but those made in the first three years and appearing here between phases ·3 and ·5 have proved to have statistically larger residuals than the later ones and have merited a weighting of only ⅓ in the solution of the orbit. The exceptional number of them in a particular season (2001–02) at about phase ·45 arose from a desire at that time to obtain a good orbit for the short-period sub-system whose single-lined orbit shown in Fig. 6 was published in preliminary form in ref. 1 in 2003.

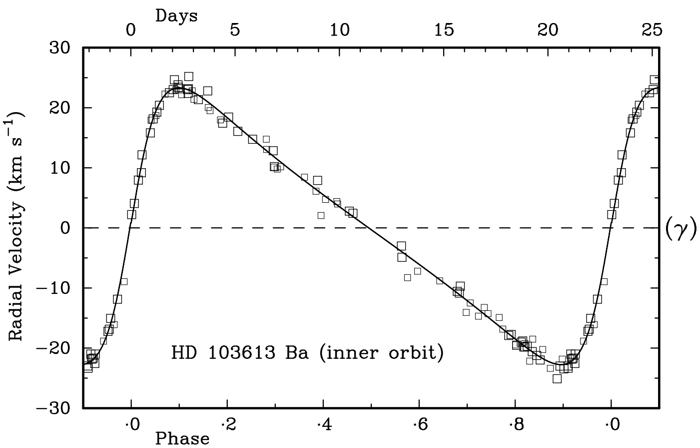

FIG. 6

Orbit of the 23-day single-lined sub-system that constitutes the secondary object in the spectrum of HD 103613. As in Fig. 5, the small symbols represent measurements that were made in the early years of the campaign and needed to be down-weighted in the solution of the orbit. The rather high orbital eccentricity is unusual at such a short period.

stars as found in a table such as that in *AQ*[43], whereas if the brighter star in HD 103613 had begun to evolve, in the way that Procyon has, then the system could be expected to be distinctly redder than it is.

Accepting, then, that the principal star has a type of about F0V, we may assign it a mass of about 1·7 $M_\odot$ and then appeal to the quantity $m_1 \sin^3 i$, that is determined as 0·182 $M_\odot$ from the orbital elements and listed in Table V above, to provide an estimate of the orbital inclination. We obtain $\sin^3 i \sim 0{\cdot}107$, $\sin i \sim 0{\cdot}475$, $i \sim 28^\circ{\cdot}4$; the uncertainty is only of the order of a degree or so, and even *that* stems at least as much from the uncertainty of the initial guess at the mass of the star than from any imprecision in the orbital elements.

So far, so good! But now we come to pay attention to the short-period sub-system. The outer orbit has a tolerably well determined mass ratio of 0·850 ± 0·042, so if the primary star has a mass near the 1·7 $M_\odot$ that we have been obliged to assign to it, then the sub-system must have a mass of 2·0 $M_\odot$. The uncertainty of the mass ratio adds about 0·1 $M_\odot$ to whatever error there may be in the estimate of the mass of the primary. Of the 2·0 $M_\odot$, just half is accounted for by the star whose velocity has been measured and is in the 23-day orbit; the other half must be in its seemingly invisible companion. We can hardly postulate that that is a white dwarf, because the evolution leading to that stage could not leave the companion star in an orbit that had either a short period or a high eccentricity, both of which the observed star assuredly *has*. The only way to make a solar mass invisible in the system is to split it in half and assume that the 23-day secondary object is itself a pair of faint main-sequence stars, which would have to be about type M0 and in an orbit with a period only of the order of a day. That is not an idea with which the author is particularly comfortable, but he sees no more-plausible alternative.

Recapitulating how that difficulty has arisen, we started with the proposition that the mass of what is undoubtedly the primary star is about 1·7 $M_\odot$. Its distance is quite accurately known; it definitely has the absolute magnitude and the colour index of a main-sequence star with a type close to F0, so it has to have the corresponding mass*. Then the orbit shows incontrovertibly that its companion, the 23-day system, has a larger mass, close to 2 $M_\odot$. We can see one of the components, which gives the impression of being another main-sequence star about two magnitudes fainter than the primary and therefore — from the tabulated properties of main-sequence stars — very similar to the Sun. So it accounts for one solar mass and leaves us to look for the other one, which must necessarily be attributed to its unseen companion in the 23-day orbit. There seems no satisfactory way to escape from that conclusion; we seem constrained to suppose that the companion must be a system like Castor C (YY Gem) — something like a pair of early-M stars in a 1-day orbit.

We could try starting the argument from the other direction, with the premise that the total mass of the sub-system should be at most 1·5 $M_\odot$, so that after accounting for the solar-type star whose velocity has been measured we are left with no more than 0·5 $M_\odot$ for a suitably unobservable secondary. Then the known q would require the primary mass to be no more than 1·3 $M_\odot$ — not compatible with either the known absolute magnitude of the system ($2^{\mathrm{m}}{\cdot}45$) or the observed colour index ($0^{\mathrm{m}}{\cdot}337$).

We might also consider the implicit relationship between the mass of the invisible object and the mass function found for the visible one, in terms of the orbital inclination. By re-arranging the expression for the mass function in

*A star of somewhat smaller mass could have the right luminosity if it had begun to evolve. Procyon, for example, has $M_V \sim 2^{\mathrm{m}}{\cdot}68$ and a mass of 'only' 1·5 $M_\odot$. But it is redder than a main-sequence star of the same luminosity, having $(B - V) = 0^{\mathrm{m}}{\cdot}42$. And we would need a more extreme case, with a smaller mass and concomitantly larger and even more unacceptable colour index than Procyon's, before the invisibility of the 23-day secondary would cease to represent a problem.

the form $\sin^3 i = f(m_1)(m_1 + m_2)^2/m_2^3$, we can see that, if $m_1 = m_2 = 1\ M_\odot$, then $\sin^3 i = 4f(m)$ or about 0·08, so $\sin i \sim 0{\cdot}43$, and the projected pseudo-synchronous rotational velocity of the observed 23-day component of the system would be about 2½ km s^{-1}, always on the assumption that its rotational axis is aligned with the orbital angular momentum. But if $m_2 = 0{\cdot}5\ M_\odot$, then $\sin^3 i = 18f(m)$, about 0·35, so $\sin i \sim 0{\cdot}7$ and the expected rotational velocity would be over 4 km s^{-1}, which is in even more unacceptable conflict with observation than the former value.

HD 160934

HD 160934 is a young late-K main-sequence star, to be found a few degrees to the north of the asterism constituted by β, γ, ν, and ξ Draconis that marks the head of Draco in the conventional constellation figure. The star is appreciably fainter than the tenth magnitude — extraordinarily faint, for a northern star, to be in the *Henry Draper Catalogue*. A notation in that *Catalogue* shows that the spectrum was observed with the *Metcalfe* telescope, which had an aperture of 16 inches, double that of those generally used for the *HD*. A quick look at the part of the *Catalogue*[44] around the number 160934 shows that there are only nine stars between 160 000 and 161 000 that were observed with the larger telescope. Mystifyingly enough they are not all concentrated in a small area, as would be the case if they all came from one particular objective-prism plate, but range over nearly 120° of declination. The one thing that they all have in common is that they are very red stars, mostly type M (160934 itself is given as Ma) but with two N stars and one R. In the same range of *HD* numbers there are only six objects (three of them *Metcalfe* stars) that are not also listed in the *BD*[45], and one of them is HD 160934.

Although its classification in the *HD* showed HD 160934 to be an M-type star, most such stars that are bright enough to feature in that *Catalogue* are of course giants. It was Vyssotsky[46] who recognized it as a main-sequence star, to which he assigned a type of K8, in the last of the four papers in which he identified M dwarfs (a category to which he admitted very-late-K ones) from McCormick objective-prism plates. In the absence of a *BD* number, and scorning the *HD*, Vyssotsky identified it by its AC[47] number, +61° 27026; he gave it his own number 796. Little further interest was shown in it until it became apparent that it was a young and very active object, through its featuring in surveys of ultraviolet and still-more-energetic photons, including those of *Einstein*[48], *ROSAT*[49–53], and *EUVE*[54–57]. In case we should have difficulty recognizing it, Shara and collaborators twice published (at slightly different scales) a picture[58,59] of the field, centred on the one bright star, HD 160934! Zickgraf *et al.*[60] later muddied the waters by offering a list of no fewer than eight objects, running down almost to twentieth magnitude, that were within the *ROSAT* error box, notwithstanding that it had long been obvious that HD 160934, by far the brightest of the objects, was in all probability the source of the high-energy radiation.

The surface activity that was implied by HD 160934's short-wavelength emission led to its inclusion in a *Catalogue and bibliography of the UV Ceti-type flare stars and related objects in the solar vicinity*[61]; that may account for HD 160934's sometimes being referred to as a flare star (in fact the main heading in the *Simbad* entry for the object is 'Flare Star'), although the writer has not come across any actual reports of flares having been observed from it.

The realization that it is an active object galvanized Weis[62] into obtaining *UBV* photometry of the star; he obtained $V = 10^{m}\cdot28$, $(B-V) = 1^{m}\cdot23$, $(U-B) = 0^{m}\cdot95$. It soon transpired that, while that V magnitude was representative, the brightness was not constant: the chromospheric activity that produces short-wavelength emission right down into X-ray wavelengths is probably also responsible for surface features, that may well be analogous to sunspots, that cause photometric variability. Mulliss & Bopp[63] obtained spectra that showed Hα to be strongly in emission. Later, Remillard was said to have "confirmed"[64] the star as the counterpart of the relevant *Einstein*[48] source simply by finding strong Ca II *H* and *K* emission in the spectrum. The *Hipparcos* 'epoch photometry' appears to show variability of the order of $0^{m}\cdot1$. Henry, Fekel & Hall[65] (at a different time) found variations only of the order of $0^{m}\cdot02$, which were periodic; there was an ambiguity in the period, between 1·842 ± 0·005 and 2·181 ± 0·007 days. Those periods are aliases of one another, being equidistant on either side of 2 days when expressed as frequencies; the authors were inclined to think that the shorter one was the true one. They had spectra, taken on three out of four consecutive nights, that indicated a constant velocity of –26·7 km s^{-1}, so the photometric period evidently did not reflect motion in a binary system; they considered that it represented the rotation period of the star. The spectra were blurred to a significant degree by rotation, for which the syndicate determined a $v\sin i$ of 13 ± 1 km s^{-1}, implying a projected radius of about 0·5 $R_{\odot}$. Apart from the rotational broadening, the spectra appeared analogous to that of 61 Cyg B, whose type is always (29 times noted in *Simbad*!) taken to be K7V*. Fekel[6] subsequently gave a considerably revised value of 16·4 km s^{-1} for the projected rotational velocity of HD 160934, corresponding to $R_{min} \sim 0\cdot6\ R_{\odot}$.

In a paper entitled "Late-type members of young stellar kinematic groups – I. Single stars", Montes *et al.*[68] assigned HD 160934 to the 'Local Association' and relied on Henry *et al.*'s finding of the constancy of its radial velocity over a four-night interval as evidence that it is a single star. Gizis, Reid & Hawley[69], who were engaged in a spectroscopic survey of 'nearby' stars with the Palomar 60-inch reflector, published in 2002 a single radial-velocity measurement of HD 160934, –26·1 km s^{-1}, obtained in 1995. So as not to embarrass them, we should avoid recalling that exactly the same syndicate, working at the same telescope, published[70] in that very year of 1995 a radial velocity of +36 km s^{-1} for HD 160934, albeit one obtained with a seemingly deliberately down-graded spectrograph that they said gave a radial-velocity accuracy of 10 km s^{-1} for the brighter stars on their programme and 15 km s^{-1} for the fainter ones. Their result then on HD 160934, however, *differed by more than 60 km s^{-1}* from the one that they chose to recall in their later paper[69], and the agreement of *that* one with the velocity obtained by Henry, Fekel & Hall[65] provided no evidence of the existence of a companion.

Pandey *et al.*[5] made photometric observations of HD 160934 on five nights during a span of 46 nights, and by fitting a sine curve (and thereby using up all but one of their five degrees of freedom) offered an illusory (but incredibly precise) period of 43·182 ± 0·040 days; they actually pointed out that that period seems long for an active K dwarf, and that Fekel's[6] $v\sin i$ of 16·4 km s^{-1}

*Probably all but one of the 29 'classifications' are actually quotations, and should never have been listed in the 'measurements' section of the *Simbad* bibliography; they include two[66] by the writer more than fifty years ago. The K7 type was actually *defined* by 61 Cyg B by Johnson & Morgan[67] in 1953 and since then nobody has ever dared to dissent!

would imply a maximum period of only 1·85 days for the rotation of a late-K dwarf with "R = 0.6R/odot" [*sic*]. They neglected, however, to mention that their maximum period is close to the one that Henry *et al.*[65] *actually found* for the star.

In the 2004 volume of *Annual Reviews*, Zuckerman & Song[71] proposed HD 160934 for membership of the 'AB Dor moving group' (a bit half-heartedly, since in a column headed *Note* there was a question mark meaning 'questionable membership'). The same column was used for notes of duplicity, but no such note appears for HD 160934; that is the more surprising, inasmuch as the same authors, with a third collaborator, published at almost the same time a paper[72] on "The AB Dor moving group" in which they listed a radial velocity of $-35{\cdot}6 \pm 0{\cdot}7$ km s^{-1} — decidedly different from the one published[6] previously, but with no date given. The same velocity and the same assignment to the AB Dor group were reproduced by Lopez-Santiago *et al.*[73]. A largely overlapping syndicate[74] then included HD 160934 in a paper giving orbits for six late-type spectroscopic binaries. One member of the syndicate (Montes) had previously asked the present writer for his radial velocities of the star, though without warning that there were plans to publish a premature orbit. That was how it came about that the writer's 21 observations, made at an epoch when there was no clear variation of velocity at all, contributed the majority of the data (which were, however, not published) to an orbit of plausible form but terrible uncertainty, with a period given as 6246·2318 days. Most of the other orbits presented in the same paper were equally open to criticism: only two of the six periods, even, were in principle correct. In due course (four years later) Griffin & Filiz Ak[75] felt obliged to offer fresh orbits for the same six stars; five of the new solutions could be claimed as good, but that of HD 160934 (with a period then given as 4000 ± 330 days) could still be considered only as preliminary since the orbit had still not been seen round a complete cycle.

Maldonado *et al.*[76], in a discussion of nearby stars as possible members of kinematic groups, assigned HD 160934 once more to the 'Local Association', which they recalled had been invented by Eggen (*e.g.*, ref. 77); they also explained that the AB Dor group was to be regarded as a youthful sub-set of that Association. In a computer-accessible auxiliary table, they gave ten radial-velocity measurements of HD 160934; although the velocities were given to a hundredth of a km s^{-1}, the timings were given in terms just of observing runs whose dates were noted only as within specific calendar months, of which the observations fell in groups of four, one, and five. More helpfully, Lopez-Santiago *et al.*[73], in a "spectroscopic study of late-type stars: chromospheric activity, rotation, kinematics and age", divulged seven radial velocities, complete with dates, for HD 160934. They were obtained in 2000–2002 at just three distinct epochs (observing runs, evidently), and clearly bracketed a nodal passage — a relatively sharp minimum of velocity in the otherwise leisurely orbit. Those authors also gave a $v \sin i$ value for each observation; though given with uncertainties always very close to 1·00 km s^{-1}, their mutual agreement is a bit better than that would lead one to expect, and the mean is $19{\cdot}1 \pm 0{\cdot}3$ km s^{-1}. There is none too good a consensus between that result, Henry, Fekel & Hall's[65] 13, and Fekel's[6] 16·4 km s^{-1}. The paper[73] also draws attention to a series of plots, visible by computer, showing the *K* line in the various stars discussed. The emission in the HD 160934 *K* line is so intense (rising to an ordinate of 8 on a scale that looks as if it is intended that any continuum that might be visible would be at 1) that the rest of the spectrum appears quite suppressed in the

TABLE VI

Radial-velocity observations of HD 160934

Except as noted, the observations were made with the Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
1995 June 12·38*	49880·38	−26·1	0·320	0·0
2000 Aug. 11·99†	51767·99	−31·5	0·824	+0·7
2001 Apr. 5·22‡	52004·22	−34·9	0·887	−0·2
6·19‡	005·19	−33·5	·887	+1·2
2002 Apr. 24·09†	52388·09	−39·8	0·989	+1·4
25·14†	389·14	−40·6	·989	+0·6
26·14†	390·14	−40·6	·990	+0·6
July 4·94†	459·94	−38·7	1·008	−0·7
5·94†	460·94	−38·4	·009	−0·5
7·05†	462·05	−38·3	·009	−0·5
Dec. 4·76	612·76	−28·4	·049	+1·3
17·72	625·72	−28·4	·052	+0·9
2003 Mar. 3·23	52701·23	−27·4	1·073	+0·3
Apr. 8·14	737·14	−26·2	·082	+1·0
May 29·01	788·01	−26·7	·096	0·0
June 25·00	815·00	−26·5	·103	0·0
July 13·95	833·95	−26·0	·108	+0·3
Aug. 14·95	865·95	−24·8	·117	+1·4
Sept. 13·88	895·88	−27·1	·125	−1·1
Oct. 11·89	923·89	−26·5	·132	−0·5
Nov. 27·74	970·74	−26·9	·145	−1·1
Dec. 15·72	988·72	−26·7	·149	−0·9
28·72	53001·72	−26·4	·153	−0·6
2004 Mar. 2·23	53066·23	−26·5	1·170	−0·8
Apr. 1·16§	096·16	−25·6	·178	+0·1
3·18	098·18	−26·2	·179	−0·5
4·19§	099·19	−26·3	·179	−0·6
5·13§	100·13	−26·2	·179	−0·5
6·11§	101·11	−26·6	·179	−0·9
7·19§	102·19	−26·1	·180	−0·4
May 19·09	144·09	−27·5	·191	−1·8
June 15·05	171·05	−25·1	·198	+0·6
Aug. 8·00	225·00	−26·1	·212	−0·4
Sept. 1·94	249·94	−24·9	·219	+0·8
Oct. 5·85	283·85	−26·1	·228	−0·4
Nov. 13·80	322·80	−24·5	·238	+1·2
2005 Mar. 25·17	53454·17	−25·8	1·274	+0·1
Apr. 22·12	482·12	−24·8	·281	+1·1
May 15·07	505·07	−25·1	·287	+0·8
June 11·03	532·03	−25·3	·294	+0·6
July 21·98	572·98	−24·9	·305	+1·1
Aug. 15·98	597·98	−26·5	·312	−0·5
Sept. 8·94	621·94	−26·5	·318	−0·4
Oct. 4·83	647·83	−25·9	·325	+0·2
Nov. 16·75	690·75	−27·3	·337	−1·1
2006 Apr. 4·16	53829·16	−25·4	1·374	+1·0
Aug. 2·02	949·02	−26·1	·406	+0·5
Nov. 1·82	54040·82	−26·8	·430	0·0

TABLE VI (*concluded*)

Date (UT)			*MJD*	*Velocity* km s^{-1}	*Phase*	*(O–C)* km s^{-1}
2007	July	19·01	54300·01	−27·2	1·499	+0·1
	Nov.	15·77	419·77	−27·4	·531	+0·2
2008	July	13·06	54660·06	−27·8	1·595	+0·4
	Nov.	7·80	777·80	−29·4	·627	−0·8
2009	May	29·08	54980·08	−28·9	1·681	+0·4
2010	May	13·10	55329·10	−29·8	1·774	+1·1
	July	30·03	407·03	−31·5	·795	−0·1
	Oct.	11·83	480·83	−31·8	·814	+0·1
2011	June	8·06	55720·06	−34·6	1·878	−0·4
	July	25·04	767·04	−35·5	·891	−0·6
	Sept.	10·91	814·91	−36·6	·903	−1·0
	Oct.	14·81	848·81	−35·7	·912	+0·5
	Nov.	17·79	882·79	−37·7	·921	−0·9
	Dec.	5·75	900·75	−37·3	·926	−0·1
2012	Apr.	16·16	56033·16	−41·9	1·962	−1·6
		30·11	047·11	−39·3	·965	+1·3
	May	12·12	059·12	−40·0	·968	+0·9
		26·09	073·09	−40·9	·972	+0·2
	June	13·08	091·08	−42·8	·977	−1·4
		29·03	107·03	−40·7	·981	+0·8
	July	15·98	123·98	−42·1	·986	−0·7
		23·03	131·03	−41·8	·988	−0·5
	Aug.	8·97	147·97	−41·5	·992	−0·5
		20·95	159·95	−39·8	·995	+0·8
		30·92	169·92	−39·9	·998	+0·2
	Sept.	13·89	183·89	−39·9	2·002	−0·5
	Nov.	5·81	236·81	−36·1	·016	−0·1
		14·76	245·76	−36·4	·018	−1·0
	Dec.	1·77	262·77	−34·0	·023	+0·3
		16·71	277·71	−32·1:	·027	+1·3
2013	Mar.	14·18	56365·18	−30·1	2·050	−0·5
	Apr.	2·16	384·16	−29·3	·055	−0·3
		21·13	403·13	−28·0	·060	+0·6
	June	3·08	446·08	−27·4	·072	+0·3

* 'Published' observation[69]; weight 1.
† 'Published' observation[73]; weight 1.
‡ Supplied by Dr. D. Montes; weight ¼.
§ Supplied by Dr. D. Montes; weight 1.

plot. There is an asymmetry in the profile, which appears to have a relatively weaker blending component with an intensity rather less than 1 on the red side, just where the *K* emission in the secondary star might be expected to appear at times near the nodal passage to which reference is made above.

A most interesting development occurred in 2007, when Hormuth *et al.*[78] and then Lafrenière *et al.*[79] reported the 'visual' resolution of HD 160934. They were able to see the spectroscopically-discovered system as a double star 'on the sky'. Hormuth *et al.*, in a short paper devoted entirely to HD 160934, described how they resolved the system in 2006 July in the very-near infrared,

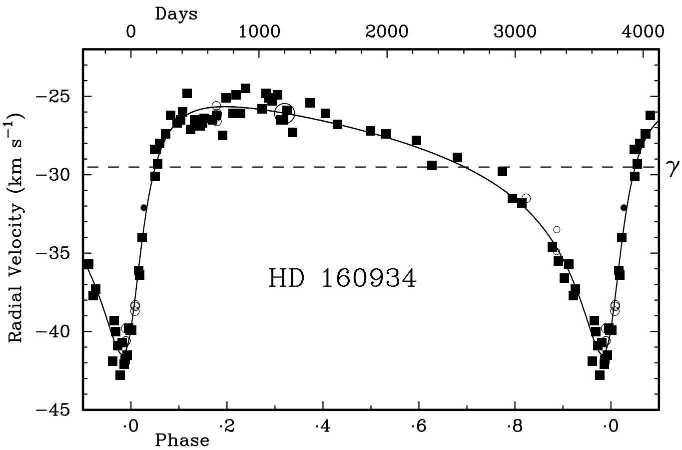

FIG. 7

The observed radial velocities of HD 160934 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. Most of the observations were made with the Cambridge *Coravel* and are shown as usual by filled squares. Ordinary-sized open circles plot measurements made either with the Calar Alto 2·2-m telescope or *INT* on La Palma, and accessible by computer or else provided privately by Dr. D. Montes; the ones in the general vicinity of the velocity minimum refer to the cycle previous to the Cambridge observations at that phase. The large open circle represents the single observation by Gizis *et al.*[69], which also belongs to the earlier cycle.

by short-exposure 'lucky imaging' with the Calar Alto 2·2-m telescope. Moreover, they somehow retrieved a previously unremarked observation, made by the *Hubble* telescope in 1998, that also resolved the binary. Lafrenière *et al.*, though publishing a few months later, actually resolved it before Hormuth *et al.*, in 2005 April, and again in 2006 September, with adaptive optics operating well into the infrared (1·6 μm) on the *Gemini North* 8-m reflector. Evans *et al.*[80] subsequently published three more measurements of HD 160934 as a 'visual' double star, obtained by an aperture-masking technique equivalent to a multiple interferometer, on the Palomar and *Keck II* telescopes. There was then a total of seven resolutions, all confined to an arc of about 110° of the orbit, running from near apastron to approaching periastron; the orbital period was, however, well constrained by the relatively old *Hubble* measurement of 1998, which fell between the more recent measurements of the next orbital cycle. Evans *et al.* were therefore able to compute an orbit; its elements are listed below in Table VII, for comparison with the (actually very similar one) established from radial velocities*. In analogous fashion Evans *et al.* resolved another system whose primary was known to be an active dwarf, HD 113449, and gave a 'visual'

*It is to be hoped that *Simbad* will soon straighten out the 'information' that it volunteers on HD 160934. A conspicuous piece of advice headed "**essential notes**" asserts, "Binary with a separation 0·12″ (4·5AU) and a period of 8·55yr" [then it gives references in its own style to Galvez *et al.*[74], who actually gave the period as 6248·2317 days, and Hormuth *et al.*[78]] "meaning a triple system with the possible low-mass star HD 160934B." The latter name leads to an entry that is headed as a brown dwarf of that name, and has a bibliography of three papers that speculate on the possible existence of such an object. Whoever wrote the 'essential notes' might have made it clear that the visual companion observed by Hormuth *et al.* is the same as the spectroscopic companion, as those authors did in fact suppose, and in any case no brown dwarf is known to belong to the system.

orbit for it in the same table as that of HD 160934. In the case of HD 113449, however, they omitted to mention the spectroscopic orbit[81] already given for the star by the present writer; a comparison of the two orbits is given below (Table VIII) in the final section of this paper.

HD 160934, having been on the Cambridge observing programme from 2002, remained under observation after first Galvez *et al.*[74] (a syndicate that included Montes) in 2006, and then Griffin & Filiz Ak[75] in 2010, published preliminary orbits. Actually, between 2006 and 2010 the star was not observed very often because it was then near apastron and its change in velocity was slow. With the approach of periastron the frequency of observation was appropriately increased in 2011 and particularly 2012, and there are now 67 Cambridge *Coravel* observations to be listed in Table VI, together with those to be found in the literature and five additional ones kindly supplied by Dr. Montes. None of the usual tampering with zero-points has seemed warranted, since all of the observations seem to be systematically homogeneous as they stand. They readily produce the orbit which is plotted in Fig. 7; the orbital elements are presented in Table VII, alongside those of the two previously published spectroscopic sets, those of Galvez *et al.*[74] and of Griffin & Filiz Ak[75], and the 'visual' elements recently given by Evans *et al.*[80].

TABLE VII

Orbital elements for HD 160934

Element	*Galvez et al.*[74]	*Griffin & Filiz Ak*[75]	*This paper*	*Evans et al.*[80]
P (days)	6246·2318	4000 ± 330	3748 ± 8	3764·0 ± 12·4
T (MJD)		52452 ± 11	52429 ± 9	52389·5 ± 64
γ (km s^{-1})		−29·25 ± 0·16	−29·51 ± 0·11	
K_1 (km s^{-1})		7·39 ± 0·22	7·90 ± 0·13	
e	0·8028	0·697 ± 0·026*	0·651 ± 0·010	0·636 ± 0·020
ω (degrees)		220·4 ± 3·2	218·0 ± 2·0	216·0 ± 3·1
$a_1 \sin i$ (Gm)		292 ± 28	309 ± 6	
$f(m)$ ($M_\odot$)		0·062 ± 0·010	0·084 ± 0·005	
R.m.s. residual (wt. 1) (km s^{-1})		0·78	0·75	

*Owing to negligent proof-reading, for which the writer apologizes, this number was printed as 0·26 in the original paper[75].

There is seen to be very good agreement between the new spectroscopic orbit and those elements of the 'visual' one that can be compared with it. The period, for example, which is more than ten years, differs by only 16 days between the two orbits, the standard error of the difference being 15 days. The other three elements which are available from both orbits are well *within* the standard error of their difference. The comparison with the earlier Cambridge orbit[75], which was uncharacteristically preliminary and published then only for completeness in referring to *all* of the six orbits in the paper[74] being commented upon, is less satisfactory: the amplitudes, in particular, differ by almost twice their joint standard deviation. Such a discrepancy is not a cause for undue concern, since it has been remarked above, in connection with HD 17922, that standard errors are liable to be invalidated by poor phase distribution of the observations. That could apply particularly to the amplitude in the present case, because the part of the orbit that had not been seen from Cambridge was the sharp descending node upon whose velocity the derived amplitude would largely depend. The comparison with the only two elements actually presented by Galvez *et al.*[74] is far worse: the discrepancy in the periods is 2498·2318 days — almost two-thirds of the period itself and more than 300 times the standard error of *ours*!

Extrapolating the 'brightness ratios' of the components of the binary at different wavelengths[78] leads to an expectation that their ΔV is about $1^{m}\cdot5$; coupled with the spectral type of K7 for their combined light, that would suggest that their individual types must be very close to K6 and M0, for which a consensus of tabulated main-sequence masses would suggest values close to 0·65 and 0·5 $M_{\odot}$. Such masses are compatible with the mass function found here, yielding a value for it of $0{\cdot}95 \sin^3 i\ M_{\odot}$, whence from the actual value we obtain $\sin^3 i \sim 0{\cdot}885 \pm 0{\cdot}05$, leading formally to an inclination between 70° and 78°. In this matter we have been pre-empted by the 'visual' orbit, which has[80] an inclination of $82°{\cdot}3 \pm 0°{\cdot}8$. Not only is the inclination angle i very sensitive to $\sin i$ when $\sin i$ is close to unity — as it is here — but inasmuch as the masses appear squared and cubed in the expression for the mass function, the latter is very sensitive to *them*, so only very small adjustments to the initial mass estimates would be needed for complete compatibility.

The parallax[17] of HD 160934 corresponds to a distance modulus of $2^{m}\cdot60 \pm 0^{m}\cdot15$, which with the representative apparent magnitude of 10·28 observed by Weis[62] yields an absolute magnitude of 7·68. We can use the convenient table in the *Skalnaté Pleso Atlas*[82] to find that for a double star with $\Delta m = 1^{m}\cdot5$ the total brightness is $0^{m}\cdot24$ brighter than the primary component alone, so the absolute magnitudes of the pair that constitutes HD 160934 should be about $7^{m}\cdot9$ and $9^{m}\cdot4$ (still with the uncertainty of $0^{m}\cdot15$ from the parallax alone). The tabulated[43] M_Vs for types K5 and M0 are $7^{m}\cdot3$ and $9^{m}\cdot0$, respectively, so the types of K6 and M0 suggested spectroscopically would each need to be advanced by one sub-type for agreement with photometric considerations.

The Cambridge radial-velocity traces yield a mean $v \sin i$ value of 16·0 km s^{-1} with an r.m.s spread of 1·8 km s^{-1} per observation, so the standard error of the mean of all 67 traces is only 0·22 km s^{-1}. But (as explained above) to allow for possible systematic error arising from un-considered minor sources of line-broadening, the mean value is not claimed to be accurate to better than 1 km s^{-1}, so the 'official' result is $v \sin i = 16 \pm 1$ km s^{-1}.

Note on HD 113449

HD 113449 is a $7^{m}\cdot7$ star of type about G8–K0 V, which at a declination of –5° culminates at a zenith distance of more than 57° and so is none too attractive an object for observation from Cambridge. All the same, an orbit was published for it in Paper 212[81] of this series in 2010. What was at that time a quite comprehensive run-down on the literature on the star was given in that paper, and no purpose would be served in repeating it here. The star was placed on the Cambridge observing programme in 2003, when it came to the writer's notice as a binary for which astrometric elements had been derived by *Hipparcos*. Not only that, but the star had been found to have an active chromosphere and thus could be expected to be of substantial interest to some sections of the astronomical community. Indeed, soon after the writer's observing campaign began, Fuhrmann[83] actually remarked that "clearly, HD 113449 requires a detailed spectroscopic orbit". Owing to its declination, however, the observing season for it in Cambridge is short, so even though the orbital period is only about seven months it took several years to obtain good coverage of the orbit.

The 2010 paper[81] goes so far as to say that it was "surprising, even astonishing, that … HD 113449 has not had its spectroscopic orbit published already, since it is well-known both spectroscopically and astrometrically to be a binary — indeed, *Hipparcos* determined an astrometric orbit; the desirability of a spectroscopic orbit has been specifically pointed out, and its short period and large amplitude make it an easy subject for such an investigation."

What the writer did not know at the time was that a spectroscopic orbit *had* in fact already been given. A single-page contribution by Cusano *et al.*[84], in the conference-publication series that is an offshoot of the Mexican journal, claimed to know the orbit, although it did not give either the data or the elements, nor even a reference to where they might be found. That publication was received in Cambridge on 2010 May 28, just three days before the one that included Paper 212. It was only when Evans[80] gave the astrometric elements — that have prompted this present item — that the reference (not recorded by *Simbad* or even by Cusano *et al.*[84]) was given to earlier conference proceedings, in a not-particularly-astronomical series, where Cusano *et al.*[85] were a bit more forthcoming, showing a graph of the orbit (and one on a much more open scale of ordinates showing the residuals) and giving its elements. The radial velocities themselves were not published. All available sets of orbital elements — from *Hipparcos*, Cusano *et al.*[85], me[81], and Evans *et al.*[80], are collected together in Table VIII. It is particularly noted that the Evans *et al.* set is not a totally independent solution of the orbit. Its authors *imposed* certain of the elements, noted in the table with asterisks, from the orbit by Cusano *et al.*; they did not refer to mine (and seem not to have considered it at all, otherwise the discrepancies would have given them food for thought).

TABLE VIII

Comparison of published orbital elements for HD 113449

Element	*Hipparcos*	*Cusano et al.*[85]	*Griffin*[81]	*Evans et al.*[80]
P (days)	231·23 ± 1·96	215·9 ± 0·1	216·48 ± 0·06	216·9 ± 0·2
T (MJD)	—	53410·5 ± 1	53845·3 ± 1·0	53410·5 ± 1*
γ (km s^{-1})	—	−1·79 ± 0·02	−0·66 ± 0·07	—
K_1 (km s^{-1})	—	13·40 ± 0·02	13·03 ± 0·10	—
e	0·51 ± 0·21	0·300 ± 0·005	0·261 ± 0·007	0·300 ± 0·005*
ω (degrees)	42 ± 22	114·6 ± 0·5	294·5 ± 1·9	114·5 ± 0·5*
$a_1 \sin i$ (Gm)	—	38·00 ± 0·15	37·46 ± 0·30	—
$f(m)$ ($M_\odot$)	—	0·0467 ± 0·0006	0·0448 ± 0·0011	—
R.m.s. residual (wt. 1) (km s^{-1})	—	0·32	0·38	—

*Fixed

The juxtaposition in Table VIII of mutually conflicting elements seems to call for some comment. The two spectroscopic sets differ in most of the elements by amounts far in excess of what the stated standard errors would lead one to expect. The standard errors given by Cusano *et al.* are wrong. Among the pieces of evidence that encourage that to be stated with such breath-taking confidence are those of the residuals from the orbit, which can easily be read individually from the graph that gives them (although we notice that the points on the actual orbit graph do not seem to reflect them). There appear to be 18 observations; their residuals have been read back from an enlarged copy of the graph and have been found to have an r.m.s. value of 0·32 km s^{-1}. It is hardly likely, therefore, that the standard error of the γ-velocity, let alone K, can be less than $0{\cdot}32/\sqrt{18}$, ~0·076 km s^{-1}; it cannot be the quoted[85] 0·02 km s^{-1}. The uncertainties of the other elements are less directly verifiable by anyone who is not privy to the actual data, but they all appear very optimistic. Evans *et al.*[80] complained that they could not fit their astrometric data to the Cusano period of 215·9 days, which they evidently hoped would be more accurate than anything that they could determine for themselves: they were forced back into making their own determination, which turned out to be 1·0 days (ten standard deviations) different, and incidentally nearer to my own value, though it is seven of *its* standard deviations away from *that*, too. (But I watched more than ten circuits of the orbit, so the resulting period could be expected to be relatively accurate.) A direct comparison of the other elements shows a discrepancy of more than 1 km s^{-1} in the γ-velocities, but that must largely represent a zero-point difference which is admitted and expected. It is more difficult to explain why the ωs differ by 180°, especially when the orbit graph demonstrates at sight that the value asserted by Cusano *et al.* is in the wrong quadrant. Owing to the difference in the definitions of ω in the 'visual' and spectroscopic cases, it is to be expected that the values will be mutually reversed, so the value adopted by Evans *et al.* is reasonable (apart from being mis-quoted).

There would be much to be said, in this and every case, for the publication of the actual data, so that they can be utilized by themselves or pooled with others to assist third parties in pursuing their independent interests in the objects concerned.

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CORRESPONDENCE

To the Editors of 'The Observatory'

Greenwich Recollections

Gilbert Satterthwaite† joined the Meridian Department of the Royal Observatory (RO) in 1952. He came in straight from school as a Scientific Assistant and was very soon one of the regular observers on the *Airy Transit Circle* (*ATC*). There were six of us: in order of experience, Leonard Symms (Head of the Department), Kenneth Blackwell, Aled Jones, Andrew Murray, myself, and Gilbert. We were each issued with an alarm clock, a torch, and keys to Greenwich Park (locked at night) and the RO sites within the park. It was at Greenwich that Gilbert first met his future wife, Valerie Terry, working elsewhere in the RO.

The Department consisted of about ten people, housed in a large office in the South Building. It was the office where Arthur Eddington had sat as a young Chief Assistant (within the memory of one living member of the Department, Frank Jeffries), before the First World War. The normal work pattern for the observers was two or three night duties per week, plus a great deal of arithmetic during office hours to 'reduce' the observations; a computer in those days was a human being. The Sun was also observed at transit every day from Monday to Saturday (we worked a 5½-day week) and on Sundays near each equinox.

On Monday mornings the *Airy* telescope, in a horizontal position, was lifted a foot or two out of its trunnions so that direct observations could be made from the north collimator on the south collimator, both housed within the Airy pavilion. The observer on duty had to stand above and astride the telescope and do the lifting with a crank handle, having first removed the main steps on which he climbed to the top! Meanwhile, Jack Johnson from the workshops would appear, to remove old oil from the bearings and replace it with new clean oil. He had a ready flow of pungent criticism for any young members of staff who displayed any clumsiness or departure from the regular routine.

A special investigation was also in progress on the *Cooke Reversible Transit Circle* (*RTC*), housed in a separate enclosure. It was destined for Herstmonceux, as a replacement for the non-reversible *ATC*, but was exhibiting largish fluctuating errors relative to the collimators which seemed to depend on the *rate of change* of temperature. So we were heating up the pavilion with electric fires and then cooling it rapidly, usually by opening the shutters on a cold night. The investigation required the telescope to be reversed in its bearings from time to time, a task accomplished with the aid of a jack on rails and a turntable well clear of the piers. Eventually the main problem was traced by simple algebra to the two tubes attached to the central cube; for more details see *R.O. Annals* from 1973, *Second Greenwich Catalogue for 1950.0*.

Another special investigation in which Gilbert played a full part was the observations with the *ATC* of a light fixed to an old azimuth mark at Chingford, due north of Greenwich. The old 'assegai' on a concrete plinth used in Victorian times had become obscured by trees, but with the aid of light-keepers and a lamp lit with car batteries provided by the Ordnance Survey we could now take regular observations; and this we did over a month or so in the autumn of 1953, in order to provide an accurately determined N–S reference for the Ordnance

Survey, who in turn could calculate a geodetic longitude for Herstmonceux for ultimate comparison with a (later) astronomical value.

On one occasion the BBC arranged to do a live radio broadcast from the *ATC*, with Symms doing a running commentary on his actions to observe a star at transit. The selected star was δ Ophiuchi. As the moment approached, so did the clouds, but Symms carried on as though the sky was clear: "It's entered the field of view ... it's now reached vertical wire one ...", *etc*. When he heard about this next morning, Andrew Murray said it destroyed his faith in all outside broadcasts; perhaps the Test Matches were not being played at all! From then on, δ Oph was known in the Department as the star that can be seen through cloud.

From time to time the more junior members of the Department wondered aloud about who our 'customers' were. Were our observations just filed away and never used? Leonard Symms would say that the whole procedure of determining accurate time was useful to "the frequency-standards people" at the National Physical Laboratory. So another regular joke in the Department, after a fruitful observing duty on a clear night, was that "the frequency-standards people will be very pleased". Symms chuckled with the rest of us.

The move to Herstmonceux was being done in stages, department by department, and for us it was in the period 1954–55. Aled Jones decided to return to Wales and do a maths degree. One of our first tasks was to get a new photographic zenith tube (*PZT*) up and working. Sir Howard Grubb Parsons staff undertook the installation, with me watching in an 'eyes-on, hands-off' role. After various teething troubles before and after hand-over, the *PZT* produced the first reliable determination of the longitude of Herstmonceux. Gilbert was fully involved in this work. The *RTC* was also moved at about the same time, and housed in a pavilion designed to keep it at an equable temperature.

The scientific assistants did day-release studies in Brighton. It was a busy life, with observing duties, office work, and study. Eventually Gilbert decided that he needed a change and found employment in scientific publishing. But his great interest in astronomy stayed with him throughout a varied career. He obtained a BSc and then an MSc by part-time study; his MSc dissertation on the history of the *ATC* is both scholarly and interesting at the same time. In retirement he volunteered as a guide at the RO, by then part of the National Maritime Museum (NMM), and took a particular interest in the maintenance of the *ATC* in good condition.

The surviving members of the Meridian staff from the Greenwich era had several annual reunions at *The Plume of Feathers* pub near the NMM. In recent years, as Andrew Murray became less mobile, we shifted our venue to *The Ship* pub near his home in Eastbourne. The last such meeting was in 2012 October, shortly before Andrew died (see page 120 of this volume).

Yours faithfully,
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† Gilbert Satterthwaite's obituary is to be found on page 374.

William Herschel's Mirrors

In a memorandum that William Herschel[1] sent to the President of the Royal Society on 1781 November 19, soon after his discovery of Uranus and the award of the Copley Medal of the Royal Society, and prior to the presentation ceremony, he made the following statement regarding his Newtonian telescopes: "...The object speculum of my 7ft telescope has obtained its perfection of figure by making three specula at a time; then by a tryal upon the planets or stars selecting the best of them, and working upon the other two till one of them was better than that which was laid by; which, as soon as better was obtained, became in its turn one of two destined to be worked upon. And tho' this method of perfectioning the telescope has proved extremely laborious, I flatter myself that my endeavours have not been ill bestowed. The Speculum I now use is the best of near two hundred I have finished in that manner...". Herschel's mirrors were cast in speculum metal, an alloy of about ⅔ copper and ⅓ tin.

Why did he choose to work on three mirrors at a time rather than one or two or four or some other number? It may be related to a well-known technique of making a flat block of glass, when an external standard of flatness is not available. It is then necessary to work on three blocks of glass, A, B and C, until 'perfect' fits of A on B, A on C, and B on C are obtained, as judged from the absence of interference fringes. A could give a good fit to B at any displacement if one is a slightly convex spherical surface and the other is a slightly concave spherical surface with the same radius, but C cannot be convex and concave at the same time. This is the reason why three surfaces are necessary. The tests must include checks of fit at different displacements of A against B, *etc.*, to avoid the unlikely case of corrugated surfaces fitting together in a particular position.

The normal strokes of grinding and polishing a mirror tend to produce a spherical surface. The Foucault knife-edge test for spherical surfaces was not described until 1858, but earlier telescope makers may have used some similar null technique. The final stage of polishing is then to 'figure' the mirror into a paraboloid shape. Having done his best with three mirrors, Herschel could then test them on, for example, close double stars and choose the one (A) with the best performance to 'set aside'; in practice, this might mean that it was the one he would use on clear nights. Perhaps he used one of the other two (B and C) to produce a supposedly parallel beam of light from an illuminated pin-hole and performed a null test of some kind on the other, or examined the quality of the image. Successively 'touching up' these two mirrors, reversing their roles if necessary, might produce a situation in which one has been slightly over-figured and the other under-figured. Now bring A into the tests: it cannot be over- and under-figured at the same time. Repeated cycles of work on the three mirrors would ideally bring each to a shape very close to a perfect paraboloid. This reasoning would explain Herschel's practice of working on three mirrors as a group.

It is not claimed here that Herschel had a fool-proof method of converging rapidly to three perfect paraboloids, merely that testing in pairs may have been part of his normal approach towards that aim.

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Reference

(1) See *Occasional Notes of the RAS*, **3**, no. 18, 1956 February, pp. 115–117.

REVIEWS

Stellar Evolution Physics, by I. Iben, Jr. (Cambridge University Press), 2012. **Volume 1: Physical Processes in Stellar Interiors**. Pp. 890, 25 × 19 cm. Price £50/$90 (hardbound; ISBN 978 1 107 01656 9); **Volume 2: Advanced Evolution of Single Stars**. Pp. 605, 25 × 19 cm. Price £55/$95 (hardbound; ISBN 978 1 107 01657 6).

In the 1960s, various groups of astronomers around the world independently took advantage of the advent of accessible computers to begin a systematic numerical study of stellar structure and evolution. Some of the groups were based in research institutes (for example, those led by Rudolf Kippenhahn and Bohdan Paczynski, in Munich and Warsaw, respectively), but those in the USA were mostly at universities (for example, Martin Schwarzschild at Princeton and Icko Iben at Illinois) and perhaps had a slightly different perspective. Kippenhahn published his well-received text-book (with Weigert) in 1996 (with a second edition out this year), and now Iben has put together this comprehensive pair of volumes (advertised as separate books, but the chapter numbers and even the pagination run continuously on from Volume 1 to Volume 2, although each book has its own index). Unlike most recent books on stellar structure, this one is unashamedly a graduate monograph, only suitable for someone with a thorough undergraduate grounding in physics, and preferably some general knowledge of astronomy as well.

It reminds me to some extent of the classic 1968 text by Cox & Giuli, and to my mind has the same principal defect: it provides so much detail that it is hard to see the wood for the trees — this is a reference work, rather than one to read from cover to cover. Another disappointment is that there is no general bibliography, surveying other books in the field; instead, there are selected references in each chapter, listed at the end of the chapter (so, for example, Kippenhahn's book is mentioned nowhere, although there are a few references to papers from his group). In addition, there are quite a few typos, perhaps not surprising in a book of this length (proof-reading must have been a nightmare!); one persistent one is "terrestial" [*sic*] for "terrestrial".

Having said that, my admiration for the book grew as I read it. This is a book by one of the most prolific researchers in the area of stellar evolution, and he has plenty to teach practitioners in the field. The detail provided is impressive, if a little overwhelming in places, and yet he continually tries to emphasize the physical processes behind the detailed mathematics; the English style is always clear and straightforward. He has also adopted the unusual course of splitting the discussion of the background physics: the physics required for evolution to the red-giant tip is presented in Volume 1, while the physics needed for the later stages of evolution is postponed until Volume 2. Thus Volume 1 covers statistical mechanics, thermodynamics, equations of state, opacity, and H-burning nuclear reactions (including neutrinos), while transport processes (diffusion, gravitational settling, electron conduction), weak interactions (including neutrino processes at high densities), and He burning and later nuclear reactions are covered in Volume 2.

The first part, comprising two chapters, is unusual also: it is an introduction to stellar evolution, but it is more like an extended summary of everything that he goes on to justify and derive in the rest of the book, together with the only significant mention of binary-star evolution (is he planning a Volume 3 to cover binaries?). After that, he changes gear in part II and becomes quantitative (although still using, where appropriate, approximate and order-of-magnitude arguments to illuminate the physics). He has his own slant on topics, which sometimes makes the reader stop and think (for example, in his discussion of charge separation and the consequent electric field, and in the use of V for the logarithmic temperature gradient instead of the more usual ∇), but perhaps the most unusual feature is his emphasis (in Chapter 5) on using polytropes to illustrate stellar properties, starting by deriving Poisson's equation and then using gravitational potential as the dependent variable in the Lane–Emden equation (which he doesn't name). The one-zone models that he subsequently uses in that chapter to obtain qualitative results are quite complicated, and I felt that it might have been better in some places to use scaling arguments based on homology (which he doesn't mention). When he comes to discuss opacity, he goes into great detail (137 pages), starting with a full account of the background physics, but then concentrating more on how to use opacity tables (with a plea for more analytic fits for use in evolution calculations) than on how best to calculate the tables from the physical principles.

A long chapter on the equations of stellar evolution and how to solve them concludes the second part of the book. It starts with the derivation of the equations in full generality, followed by a discussion of the fitting-point method of solution and a detailed account of using relaxation techniques to evolve an initial model; this includes a description of how to solve linear equations. In a later chapter, he has an interesting digression on some artificial effects introduced by too small a time-step. The chapter ends with a fascinating personal account of how the computing environment has changed since he began computing in 1960.

The next 320 pages discuss evolution from the pre-main-sequence phase up to the onset of helium burning at the red-giant tip, with one chapter devoted solely to the Sun and the relevant neutrino physics. All are profusely illustrated by his own calculations, some of which have clearly been produced especially for this book, using models of 1, 5, and 25 solar masses as representative. At the end of section 11.1, he devotes a couple of pages to the possible effects on the Earth of the vast growth in the Sun's radius and luminosity as it climbs the red-giant branch; disappointingly, there is no recognition that anyone else has considered this rather well-studied problem, just as elsewhere there is no explicit comparison of his own evolutionary calculations with anyone else's. I felt that these were opportunities missed, although the concentration on his own models does make the book fully self-consistent, and enables him to discuss details that are not usually available in published papers.

Coming to Volume 2, the first three chapters discuss the more advanced physical processes mentioned above, followed by a detailed discussion of He-burning reactions in Chapter 16. Good familiarity with quantum physics is assumed throughout. I had one small query in his discussion of the electric field induced by charge separation: on pp. 952–3 he assumes charge neutrality, estimates the E field, and then finds a (very) small excess (about 1 part in 10^{36}) of positively charged particles — is this real, or just a measure of the uncertainty introduced by the estimate? He doesn't comment.

The next part consists of four chapters giving the results of his calculations of evolution from helium burning to some final stage, again using models for his three representative masses. There is a very detailed account of how a 1-$M_{\odot}$ model starts to burn helium and transfers to the horizontal branch — this 46-page section has no fewer than 51 diagrams! Helium ignition starts off-centre, because of neutrino energy losses, so burning is initially in a convective shell. He discusses whether there are any strong hydrodynamical effects arising from the He flash, and finds that all velocities and accelerations are small. The transition to the horizontal branch (HB) lasts almost 2 million years, and involves six smaller flashes after the main one before the convection zone finally extends right to the centre and quiescent helium burning continues in a convective core. This is followed by a careful discussion of the transition from the HB to the asymptotic giant branch (AGB) and the onset of thermal pulses. The many illustrations make it clear *what* is happening in the pulses, but there is nowhere any straightforward explanation of *why* they occur (although the relevant papers by Schwarzschild & Härm appear in the reference list for Chapter 17, without, I think, being cited in the text).

The following two chapters discuss the 5-$M_{\odot}$ model in similar detail, but again there is no *explanation* for the pulses, or for the blue loop during He-core burning, just a description, and a discussion of how some results depend on the numerical treatment, and of the uncertainties involved. The second of these chapters concentrates on *s*-process nucleosynthesis, in which neutrons are added slowly to certain nuclei to build up heavier ones; the results are illustrated by detailed examples from the 5-$M_{\odot}$ He-shell flashes, comparing pulse and interpulse phases. Interestingly, ^{14}N acts as a neutron absorber (forming ^{14}C), inhibiting the *s*-process in the interpulse phase and restricting the main formation of *s*-process elements to the peak of the flash (p. 1337).

The next chapter follows the 25-$M_{\odot}$ model through He and C burning, but starts with an overview and an illuminating comparison with the evolution of the 1-$M_{\odot}$ and 5-$M_{\odot}$ models. An interesting difference in the 25-$M_{\odot}$ case is that the luminosities from H and He burning are comparable throughout He burning. The detailed discussion stops at the end of C burning, where Iben's numerical model is becoming too cumbersome to describe the later stages without significant re-coding. However, he outlines the later Ne-, O-, and Si-burning phases, based on books by Clayton and Arnett, and various papers by others, and concludes with a brief paragraph outlining the final core collapse that leads to a Type II supernova.

The sixth and last part consists of a single 78-page chapter on the effect of wind mass loss at the top of the thermally pulsing AGB, *via* a superwind whose details are taken from the published literature, both observational and theoretical. Iben then follows the evolution of a 1-$M_{\odot}$ model in great detail through the planetary-nebula (PN) central-star phase and down to its final fate as a slowly cooling white dwarf. Along the way he has a digression into solid-state physics, exploring the equation of state of the material, and estimating when (and why) the centre becomes first liquid and then (after about 5×10^9 years) solid; the whole white dwarf becomes solid after about 10^{10} years. His other emphasis is on the various sources of energy as the surface luminosity decreases by some eight orders of magnitude; once the effective temperature starts to decrease, gravothermal energy (whose contribution has been carefully followed all the way from the main sequence) becomes the dominant source.

He also considers how the post-AGB evolution is affected by exactly when during a thermal pulse cycle the superwind removes the outer layers. The usual situation is for a single PN phase, in which case a layer of hydrogen is left on the surface, which gradually becomes essentially pure hydrogen as the heavier elements diffuse downwards; this accounts for DA white dwarfs. However, in some circumstances one more helium-shell flash can occur as the star evolves to the blue, and this causes the track in the H–R diagram to turn round and head back towards the AGB, producing what is known as a 'born-again' AGB star; he relates these models (and those of others) to observations, *e.g.*, of FG Sge, which is believed to be in this phase. A second superwind then removes the entire H envelope, revealing C at the surface (DC white dwarfs); diffusion gradually removes the carbon, leaving an essentially pure He atmosphere (DB white dwarfs). Finally, he applies the results of the chapter to an estimate of the age of the galactic disc, concluding that theory is not yet good enough to give a definitive value. This provides a challenge for future generations.

This is not a book for the faint-hearted, but it is certainly full of fascinating detail which will repay study by the active researcher. A nice feature of the book is that each chapter begins with a one- or two-page summary of what it contains. Iben has chosen to concentrate almost entirely on the results of his own computations as far as the stellar evolution is concerned, which means that he is able to investigate and describe exactly what happens at various important phases of evolution. The downside of this approach is that the reader is not told, except very occasionally, how his particular models fare in comparison with computations by other people — that is tacitly left as an exercise for the reader. There is also essentially no discussion of the effects of mass loss in phases of evolution before the superwind at the top of the AGB, nor is there any serious mention of effects of rotation (although in that case there has been a recent monograph by Maeder[1], which to some extent complements the present book).

I have a few regrets, then, for what isn't there, but nothing but admiration for what is. Iben has produced a classic monograph, which compares well with classics of the past, and I am pleased to have a copy. — ROBERT CONNON SMITH.

Reference

(1) A. Maeder, *Physics, Formation and Evolution of Rotating Stars* (Springer, Heidelberg), 2009.

Introduction to Astronomical Spectroscopy, by I. Appenzeller (Cambridge University Press), 2013. Pp. 254, 23 × 15·5 cm. Price £65/$110 (hardbound; ISBN 978 1 107 01579 1), £29·99/$50 (paperback; ISBN 978 1 107 60179 6).

This compact handbook, another in the *Cambridge Observing Handbooks for Research Astronomers* series, covers a large area in the field of astronomical spectroscopy. While a number of books on this subject have been published over the years, few if any describe most of the techniques used over the astounding wavelength range (some 13 orders of magnitude) used in modern astronomy. This book's intended readership is those with an astronomy and physics background and uses the terminology and mathematics familiar to those workers.

Chapters one and two are a concise history and discussion of modern uses of astronomical spectroscopy, respectively. The brief discussion of coudé spectrographs and their retirement may be a bit overstated as a number are still in use, some having been fitted with échelle gratings and CCDs. The author then gets down to business with the physics of electromagnetic radiation and diffraction gratings. The heart of the book is concerned with more-or-less traditional near-UV-to-IR spectral regions but also includes discussions of more recent developments such as VPH gratings, grisms, and the use of fibre optics. Just to hint at the large breadth of the topics discussed, there are sections on the basic design of spectrometers, image slicers, atmospheric-dispersion compensators, échelles, multi-object spectrometers, Fabry-Perot techniques, FTS spectrometers, and techniques for the direct detection of photon energies. Besides laying out the basic principles and equations of these various methods, extensive references are given to specific instruments and more detailed descriptions in the literature.

In keeping with the theme of a comprehensive overview of spectroscopy, a chapter is also devoted to efficient planning for observing runs as well as how raw data are massaged to produce useful scientific information. Further chapters discuss principles and practice of UV, X-ray, gamma-ray, radio, far-IR, and sub-millimetre spectroscopy. The materials and techniques for those wavelengths are quite interesting and peculiar to those of us engaged in the optical regions. The clever resourcefulness of investigators in these diverse wavelength regions can be appreciated with developments such as chirp-transform spectrometers, hot-electron bolometers, and grazing-incidence optics.

Lastly, the author delves into an educated forward look into possible new developments in instruments and telescopes for the next decade or two, while admitting that with the rapid development of new technologies heretofore unknown techniques may yet evolve.

Ample photographs, drawings, and plots are included, albeit in black and white, to illustrate the various instruments, physics, and data. One particularly striking example is the effect of increasing spectral resolution on the spectrum of a quasar. The use of black-and-white images doesn't seem a particular handicap; this reviewer remembers the days of examining only B&W plates! This volume is highly recommended for scientists requiring an overview of techniques perhaps outside their main area of research but needing complementary wavelength coverage. It would also be excellent to accompany any course in astronomical techniques. — DARYL WILLMARTH.

Introduction to Planetary Geomorphology, by R. Greeley (Cambridge University Press), 2013. Pp. 238, 28 × 22 cm. Price £45/$85 (hardbound; ISBN 978 0 521 86711 5).

A world-class pioneer of planetary geology, Professor Ronald Greeley was involved in the Apollo programme from 1967 at NASA Ames Research Center and many major planetary missions flown since. He taught at Arizona State University for over 30 years and his later research focussed on studying the processes that shape planetary surfaces in order to understand their geological histories. This book, submitted a month before his death, is a tribute to his life's work at the forefront of this field and his considerable communication skills, gained from his lecturing career and experience in co-authoring 17 other books.

Aimed at both undergraduate students and non-specialists with an interest in the subject, this introduction to planetary geomorphology explains technical terms throughout, making it easy to follow. At first glance, I wondered if colour would have added an extra dimension to the book, but as I read more closely I realized that the carefully selected black-and-white images on nearly every page clarified the individual concepts and processes described in the text and kept the beginner from being distracted.

Following an introduction of objectives and strategy for Solar System exploration, the author examines the various imaging, spectral, and thermal data obtainable from remote-sensing techniques and their analysis and use in planetary geomorphology. Chapter 3 explains the four principal geological processes that shape the Earth — tectonism, volcanism, impacts, and gradation — with good use of images and diagrams to aid understanding. This leaves the reader well prepared for the remainder of the book, in relating those processes to currently known data providing evidence for similar processes found on other moons and planets in our Solar System.

The lunar section seamlessly integrates data and images from the full range of missions, from Soviet and NASA unmanned missions to Apollo, as well as more recent ESA, Japanese, and Chinese contributions, while also considering knowledge gained from lunar meteorites found on Earth. I learnt much about the unmanned lunar missions, particularly the five *Surveyor* landers and *Lunar Orbiter* spacecraft, which set the stage for developing planetary-geology techniques to study, analyze, and interpret geological processes on the lunar surface. Those techniques were then developed and tested further by the Apollo landings.

The chapters are well structured, taking each planetary system in turn, giving a brief history of relevant exploratory missions, and then looking at the four geological processes where present on each planet and moon. From *Mariner 10* and *MESSENGER* data retrieved for Mercury, to *Cassini–Huygens* data collected at the Saturn system, with a wealth of missions to Venus, Mars, and the Jovian system in between, each major planet and moon with evidence of geological processes is discussed thoroughly. The chapter lengths are proportional to our current knowledge, ranging from 32 pages assigned to the Moon to just eight concerned with the intriguing glimpses of the Uranus and Neptune systems caught by the *Voyager 2* spacecraft. Greeley ends with a brief summary of current and future spacecraft missions and mentions the major NASA priorities of Mars sample-return and exploration of Europa, which are not as yet approved.

This is an excellent, enjoyable read for those who come to planetary science without a geological background and who want to pick up a good basic grounding to complement further studies, and also for geologists who want to learn about the application of their subject in a planetary context. — JANE MACARTHUR.

Experimenting on a Small Planet: A Scholarly Entertainment, by William W. Hay (Springer, Heidelberg), 2013. Pp. 983, 24 × 16·5 cm. Price £22/$27·95/€29·91 (hardbound; ISBN 978 3 642 28559 1).

Evidence indicates to any sensible scientist that the Earth's climate is changing. There again, the sensible scientist knows that our climate has always been changing. But nowadays humans are helping. We are breeding like rabbits and increasing the human population by about 1·3% per year; we are happily cutting down forests and increasing the planetary dustiness by farming; contrails from our aircraft are pumping up the water content of the stratosphere; and we are changing the atmosphere by destroying ozone and increasing greenhouse gases such as carbon dioxide and nitrous oxide.

There are two main results. First, our planet is getting warmer. Second, the weather variability is increasing. William Hay concludes that we are both "off on a wild climatic ride into an uncertain future", and that we have passed the point of no return — in essence we can do nothing about it.

Experimenting on a Small Planet is a weighty tome that delves into the basic physics, chemistry, biology, and geology of the mechanics of climate systems. Much is made of the last interglacial, the variability of Arctic and Antarctic ice, the usefulness of ice cores, sea-level variability, the effect of continental drift, the chemistry of water, the physics of clouds, solar variability, climate history, catastrophism, Milankovitch cycles, the efficacy of our greenhouse, carbon cycles, ocean currents and salinity, and anthropocentric influences. The book is accurately aimed at the general scientist, well referenced, and replete with many graphs and illustrations. But in essence it is rather like the infamous curate's egg. I loved the biographical details of the author's varied career that are tagged to the end of each of the twenty eight chapters. I was amused by the suggested cocktails to drink as one progressed through the book. I learned a huge amount about subjects away from my speciality. But as my arm muscles increased daily in strength by having to hold this weighty book, I started to question the need for 983 pages. What has happened to the art of succinct summary? Was précis homework missed at school? Why did not the publisher say "come on Bill, 400 pages are enough — save some trees". And talking of the publisher, whatever happened to proof reading? I might forgive the fact that the text is littered with typos and missing words, but, coming from the north of England, I found a reference to Fred Hoyle as an *American* astrophysicist was going a tad too far. — DAVID W. HUGHES.

The Space Book, by J. Bell (Sterling, New York), 2013. Pp. 528, 22 × 19·5 cm. Price £19·99/$29·95 (hardbound: ISBN 978 1 4027 8071 4).

This book is attractively presented and pleasing to handle, with a weight about it suggesting that there is much of interest to be found within, and with the main content (between the introduction and the end notes) comprising 250 double pages each with text on the left and a full-page illustration on the right, a layout which for me had an immediate appeal.

The purpose of the book, as described in its subtitle, is to set out and summarize 250 milestones in the history of space and astronomy "from the beginning to the end of time", so the first entry is headed "Big Bang" and the last "How will the Universe End?". The great majority of milestones mark historical events; 24 entries cover episodes prior to the 'Birth of Cosmology' *c.* 5000 BC, and 13 cover events that have yet to happen, the last eight of which we shall not be around to see.

The illustrations range from an imaginative depiction of the Big Bang to a dramatic portrayal of a black hole, but take in also a diverse range of more mundane objects including the pyramids of Egypt, photographs of prominent (Planck, Einstein, Hawking) and less well-known (William Lassell, Maria Mitchell) pioneers in the build-up of space knowledge, and a modern reconstruction of the Antikythera calculating machine. All the illustrations are clear and sharp, benefiting from their full-page presentation, and while the subjects are not all of equal interest, the illustrations, taken collectively, are the outstanding feature of this book.

The pages to the left of the illustrations are consistent in providing a date, a heading, and about twenty seven lines of text. Research has enabled the author to provide approximately the same length of wording for each milestone, and since some of the milestones are more important and/or abstruse than others some entries provide less and others more than what might be regarded as the optimum amount of information. There is also a certain amount of repetition, but bearing in mind the basic plan of the book this was inevitable and does not detract from the reader's enjoyment; despite its chronological layout the book is clearly designed to be dipped into rather than read as a continuous narrative.

Some of the text pages have a name or names as a sub-heading — 146 out of 250 to be precise — and I am not comfortable with this. It is inconsistent in an otherwise admirably consistent layout, and more importantly it is confusing. All relevant names are mentioned in the text, in bold type when appropriate, and to give prominence to some as a sub-heading also is not really necessary, besides suggesting an order of importance which is inappropriate. Thus the very first milestone is headed Big Bang and sub-headed Edwin Hubble. The text goes on to mention Hubble's part in perceiving the expansion of space, and the *Hubble Space Telescope*, but makes no mention of the Steady State hypothesis or of Lemaître's and Gamow's pioneering work in developing the 'Bang' concept, and if one had to have a name sub-heading this entry I should have gone for Lemaître and (albeit ironically) Hoyle. I am not arguing against Hubble's importance, but his name in any case appears in six more sub-headings, from 'Doppler Shift of Light (1848)' to 'Dark Energy (1998)', and it is hard to see the logic of this. Many other sub-headed names are equally contentious, and in this one aspect the author has in my view missed his goal.

In other respects the book presents a lively, instructive, and entertaining summary of our knowledge of space, touching lightly upon alpha and omega, the beginning and the end, and bringing to our notice or reminding us of a host of great people and great achievements. Others will have different ideas about what makes up 250 key milestones in the history of astronomy and space, as the author is quick to make clear in his introduction, but I have no criticism in this respect. If I have detected a faint bias towards American achievement I cannot but feel that NASA's amazing exploits go a long way to justify this; but I must protest about the mention, in the milestone dated 1839 and headed 'First Astrophotographs', of John William Draper as an "American doctor, chemist and photographer". Draper was born in Lancashire and studied at the University of London before going to America.

The Space Book is one in a series from Sterling built around the Key Milestone concept, and my concluding compliment to it is that the possibility of the others being equally well presented and illustrated will encourage me to seek them out. — Colin Cooke.

Heart of Darkness: Unravelling the Mysteries of the Invisible Universe, by Jeremiah P. Ostriker & Simon Mitton (Princeton University Press, Woodstock), 2013. Pp 299, 24·5 × 16·5 cm. Price £19·95/$27·95 (hardbound; ISBN 978 0 691 13430 7).

I'll say one thing for cosmology, it is full of unexpected surprises. Over the last one hundred years we have 'progressed' from allegedly being at the centre of a single, slowly spinning Milky Way galaxy to being part of an amazing system of many billions of visible, escaping, star-filled galaxies which actually make up but a small fraction of what is really there. Our Universe has a 'heart of darkness' which, in today's astronomy, is much less accessible than the original 'heart of darkness' — Joseph Conrad's Upper Congo River in Central Africa. Today's mysterious cosmology is replete with dark matter and dark energy, together with a misnamed Hubble's Constant that is not constant but, rather than declining as expected, is actually getting larger, and also a geometrical flatness that not only was always flat but also stays flat into the future. And if that is not mysterious enough, this Universe of ours seems to have been fine-tuned in order to support human life — sentient observers and enquirers.

I enjoyed *Heart of Darkness* hugely. Rare among astronomy books, it was a 'page-turner', an exciting, intriguing, authoritative historical review of past cosmological endeavours coupled with an informed assessment of where we are at the present time. It is accurately aimed at the general reader and non-expert.

Cosmology has always surprised me. Around one in four of the world's astronomers work in this discipline, the vast majority believing in a universe that started apparently *ex nihilo* with a Big Bang that was the beginning of all space, time, and matter/energy. The vast majority of cosmologists also seem to be singing from the same scientific 'hymn sheet' — believing that a ΛCDM flat model works really well for what we see. Ostriker & Mitton perceptively stress that unanimity is not a guarantee of accuracy. This superb book underlines just how much the subject has changed in the last one hundred years. It will also encourage many more astronomers into the field. But I cannot help but think that the next hundred years of cosmology will be just as innovative as the last. — David W. Hughes.

Extreme Cosmos, by Bryan Gaensler, (Perigee, Penguin, New York), 2011. Pp. 240, 21 × 14 cm. Price $16·00 (about £10) (paperback, ISBN 978 0 399 53751 6).

Imagine yourself in a library chock full of books, many informative, some inaccurate, and nearly all fascinating, but they are arranged neither by subject matter nor by author; instead they are shelved by sizes of the volume, colours of the covers, typeface, and year the publisher was founded. Now shrink the library to a single book and the contents from all of human knowledge to astrophysics, and you will have some idea of what you will experience reading *Extreme Cosmos*.

Ten chapters consider the largest and smallest values of temperature, density, size, speed, mass, gravity, electric and magnetic fields and currents, time, and (somewhat strangely) light and sound. Thus white dwarfs appear in four chapters, neutron stars in five, and the Sun in practically every one (often for some intermediate value). The index helps somewhat, but if you need to recheck how neutron stars form, for instance, you will need to look at all ten places where they are mentioned.

Here are two surprising factoids. First, the magnetic field you experience in a typical MRI is more than twice the strength of that in the atmosphere of Babcock's Star, HD 215441 (80 000 *versus* 34 000 Gauss). Second, 20–30% of the human population suffers from autosomal dominant compelling helio-ophthalmic outburst (ACHOO), meaning uncontrollable sneezes upon sudden exposure to bright sunlight. This condition I knew about. (Father described days as having "one blow", "two blow", or "three blow" suns. But the incidence came as a surprise. He had an incompletely penetrant albino gene, with nearly colourless eyes and very white skin, and was the only person I knew who reacted to daylight that way.) Perhaps ACHOOSS (ACHOO Syndrome Sufferers) don't often settle in Southern California. But author Gaensler is an Australian Laureate Fellow at the University of Sydney, where he must experience a good many three-blow suns.

Those of us in "late youth" (as Asimov called it) might be pleased to see that magnetic fields are measured in Gauss. But accelerations come in miles per hour per second, densities in ounces per cubic centimetre, and particle energies in calories. You would have to jog for several minutes to work off the calorie content of the most energetic cosmic ray detected so far. Very large and very small numbers abound, as you would expect for the topic. Some are expressed in words (40 million trillion) some with very many zeros (180,000,000,000, 000,000,000,000,000,000,000° F and 0·000000000000000004% of an inch). These are, incidentally, the temperature very soon after the Big Bang and an upper limit to the size of an electron. The intention is presumably to achieve some combination of familiarity and 'Wow!' A proper test of success would require a second version of the book, using power-of-ten notation and a random assignment of readers to the two editions. I have no idea what the result would be, so meanwhile read and enjoy the version that is available! — VIRGINIA TRIMBLE.

The Milky Way: An Insider's Guide, by W. H. Waller (Princeton University Press, Woodstock), 2013. Pp. 316, 24 × 16·5 cm. Price £19·95/$29·95 (hardbound; ISBN 978 0 691 12224 3).

When I were a lad, it was sometimes a schoolboy trick to give one's address as London, England, Earth, …. the Universe. Of course, at the time, my experience was of, well, just London. But having now read *The Milky Way: An Insider's Guide*, I feel I could, with some pride, say that the Galaxy really is part of my address. This book is superb: written in an engaging, even folksy, style, it would be the perfect primer for a beginning undergraduate wanting an up-to-date overview of stellar and galactic astronomy.

After a couple of scene-setting chapters, including a brief history of our perceptions of the nature of the cosmos — which, nonetheless, do not burden us with the usual baggage of wrong tracks and dead ends that an historical treatment often provides — Waller takes us on a journey among the components of our Galaxy as gleaned from observations right across the spectrum, outlining the structure, scale, and evolution of both the individual components (stars, gas, dust, *etc.*) and the system as a whole. Accompanied by monochrome images and diagrams gleaned from a wide range of 'front-line' sources (fully referenced), plus an excellent colour section, the reader is left in no doubt about the wonderful, intricate stellar community in which we live, from the 'Monster in the Core' out to the satellite galaxies and the streams of gas that link us to them. This breath-taking panorama is concluded with a chapter on the origin of the Milky Way and its companions in the Universe, and another on the prospects for life in the Galaxy, with a good, modern stab at solving the Drake Equation.

The book is rounded out with a first-rate glossary, extensive follow-up notes on certain points raised during the individual chapters (as an editor tasked with paging-up this *Magazine*, I much prefer these to footnotes!), an excellent selection of suggestions for further reading handily grouped by technical level, and a good index.

Any concerns? Just two: on page 22 we are told that William Herschel was financially supported by King George VI (which might have been true if Herschel belonged to the Methuselah club) — but perhaps we can forgive our

American cousins for not keeping up with the complexities of British royalty; and on page 74/76, we are told that "significant results" have only emerged from the UV domain in the last 25 years — as a member of the *IUE* 'Old Comrades' association, I think that could be extended by at least ten years. But otherwise, this completely typo-free volume can be recommended without reservation. — DAVID STICKLAND.

Le Verrier — Magnificent and Detestable Astronomer, by J. Lequeux (Springer, Heidelberg), 2013. Pp. 337, 24 × 16 cm. Price £117/$179/€139·05 (hardbound; ISBN 978 1 4614 5564 6).

Lequeux's detailed account of the professional life of one of France's best-known 19th-Century astronomers is a work of extensive scholarship, and fills admirably a curious void in the literature on world astronomers of renown. Without any doubt, Le Verrier was not only a gifted mathematician but also a very hardworking one. Gaining a position as a teaching assistant in astronomy almost by accident, he turned his attention to the bothersome deviations which the positions of the planet Uranus increasingly exhibited compared to the calculations of the day. Hours and hours of hand-calculations based on the observed perturbations eventually led to his correct prediction of Neptune, and to its discovery by a German. The circumstances that led to the lack of a contribution from the British are discussed in depth, and one senses that the facts have finally been set out with all the dispassion of a scientific researcher. But for Le Verrier all this was only the start; his consequent rapid ascent to world fame and his evident genius led to associations with government and royalty, and — when it became vacant — to the coveted appointment as Director of the pollution-damaged Paris Observatory.

Lequeux recounts Le Verrier's career extensively as through the eyes of colleagues. Le Verrier gave high priority to significant improvements and upgrades to the Observatory's instruments, and to introducing new or bigger ones, with a fervour that was remarkable in a theoretician who had had little to do with observing. But we are also left in no doubt about his pride, haughtiness, and difficult temper, his swashbuckling treatment of protégés of the previous Director, and the readiness (and ease) with which he made enemies of most of those around him. He expected his staff to work at least as hard as he did, and punished those who did not, ultimately fuelling a mass walk-out and a dismissal for the accused Director — it's the stuff of high drama. One can see how Le Verrier's strong preference for astrometry over astrophysics (then in its infancy) set the scene for France's later commitment to the *Carte du Ciel* and further impoverishment in astrophysics, though lack of good observing conditions was a significant factor in preventing France from excelling in astrometry as well as it should. How all this evolved during the presidency of Le Verrier is a fascinating tale, and Lequeux does it full justice.

The book is extremely well written (if the translation is close to the original), and the English text is almost without error. It is richly supplied with 'boxes' of definitions, explanations, and biographical sketches, and well supported with photographs, some of instruments, many of characters who feature in the text, plus a few reproductions of original documents. The book is as thorough a study as can be conceived, and should be in every library as a testament to Paris's struggles with both pollution and people during its formative years on the world astronomical stage. But although there can be no such thing as a completely unbiassed biography, one rather prominent word bothered me considerably, and that was the "Detestable" in the title. It may certainly have summarized the feelings of the majority who brought about his dismissal, but he did have some friends among his colleagues, and his daughter (whose journals are often quoted) never speaks ill of him. It is rare that the abilities, fame, and energy of a Le Verrier also make a good communicator and manager of people, and we can all cite instances of poor leadership without necessarily branding the unfortunate misfit in perpetuity as 'detestable'. It seems a rather harsh epitaph for someone who had achieved significant improvements in French astronomy, won for it substantial funding despite admittedly dictatorial handlings of people and events, and had moreover, in parallel, championed meteorology and established an international weather-forecasting network. But others can make their own judgement; the evidence is all here, and makes excellent reading. — ELIZABETH GRIFFIN.

[See also the review of the original French-language version in **130**, 40, 2010. — Ed.]

Origins of the Expanding Universe: 1912–1932 (ASP Conference Series, Vol. 471), edited by M. J. Way & D. Hunter (Astronomical Society of the Pacific, San Francisco), 2013. Pp. 336, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 826 8).

Vesto Melvin Slipher (1875–1969) of Lowell Observatory measured the first Doppler shift of an extragalactic spiral nebula (M31 of course) in 1912, the year my father was born, so this is not quite ancient history to me. It was truly a Doppler shift, about –300 km/sec, not a cosmological redshift. But a few years later, when he had accumulated a dozen more nebular velocities, all but two were positive, the largest one near 1000 km/sec, exceeding anything found for stars in the Milky Way. His increased sample of 21 redshifts (out of 25 measurements) by 1917, and another 20 in the next decade, made up the data used in attempts to measure the solar (or Galactic) motion relative to those nebulae by Wirtz, Lundmark, Silberstein, Strömberg, Lemaître, Dose, Robertson, and, most famously, Edwin Powell Hubble.

The Slipher velocities and the organizers' feeling that he probably deserved more credit than he usually gets were the motivation for the organization of a 2012 September conference held at Lowell Observatory on, nearly, the centenary of that first, M31, measurement. Twenty-three of the talks from the meeting appear in the proceedings, and every one is worth reading. They include somewhat informal reminiscences from William Lowell Putnam, III (the sole trustee of Lowell Observatory and great-nephew of Percival), and Allen Melvin Slipher (grandson of Vesto Melvin), the latter organized on quite short notice as an after-dinner talk. More formal presentations by noted American historians of astronomy Owen Gingerich and David DeVorkin bring Henry Norris Russell, his diagram, and his influential position in the community into the picture. He was definitely a 'late adopter' of the expanding Universe, as well as of the dominance of hydrogen in stars.

Other presentations requested more credit to the theorists de Sitter and Friedmann, as well as to Slipher, Lundmark, Lemaître, and perhaps others; co-organizer Michael Way asks for less credit to Hubble all around, including for the standard classification scheme of galaxies, the brightness profiles of galaxies, confirmation of the Island Universe hypothesis, and, especially, the velocity–distance relationship, citing 26 pre-Hubbleian papers (though seven are by Silberstein, who wanted to include globular clusters in the velocity–distance scheme, even ones with negative radial velocities).

A provocative question was asked by Joe Tenn: "what else did V. M. Slipher do?" The answer is, early in his Lowell career, the first spectroscopic measurement of the rotation of Uranus; evidence that Venus rotates slowly, and that both interstellar dust (reflection nebulae) and interstellar gas ("stationary lines" in spectra of spectroscopic binaries) exist; observations of night-sky brightness, zodiacal light, and aurorae; and the near-infrared spectra of planets and comets that were the subject of his 1933 George Darwin Lecture for the Royal Astronomical Society. He received an RAS Gold Medal the same year. More than a dozen astronomers before and after him achieved both in the same year (but none since Martin Schwarzschild in 1969). Slipher's later years were largely devoted to administration of Lowell (at which he was not particularly gifted) and management of his own property. For the last eight years of his life, his "companion and secretary" was Frances Wilson, the ex-wife of his brief successor as director of Lowell, Albert G. Wilson (probably better known for the taking of many plates for the Palomar Observatory Sky Survey in 1948–52).

Because the meeting focussed on events between 1912 and 1932, all of the key figures (arguably 32) had overlapping lives, from Scheiner (1858–1913, first spectrogram of M31) to H. P. Robertson (1903–61, of the Robertson–Walker metric, *etc.*); indeed, about half were born between the 1874 and 1886 birth years of my grandmothers. Henrietta Leavitt (of the Cepheid period–luminosity relationship) is the only woman among them.

And now the conflict of interest, and it is a serious one. When the meeting was first announced publicly, I quickly volunteered an abstract for a talk intended to put the 1912–32 period in context, that is, starting with pre-modern pictures of the cosmos that expanded (like the Chinese Pan-ku) and ending with the sorting-out of distances scales that led to Hubble's (improperly named?) constant shrinking from 500 to 74·3 km/sec/Mpc from his time to ours. This was first scheduled as an after-dinner talk, then cancelled because I was briefly very unwell, then written up for the proceedings, and then rejected by two anonymous referees (who wanted roughly opposite things done) and one editor, after a couple of revisions aimed at trying to satisfy them all. Early-modern appears in the chapter by Lucia Ayala, who addresses Descartes, Swedenborg, Thomas Wright, and Kant; and 'now' is the focus of Chris Impey's discussion of very large redshift surveys and their results. If you are curious about the rejected paper, it can be found on arXiv as paper number 1307.2289 under both physics.hist-ph and astro-ph.CO. — VIRGINIA TRIMBLE.

Neutrino Cosmology, by J. Lesgourgues, G. Mangano, G. Miele & S. Pastor (Cambridge University Press), 2013. Pp. 378, 25 × 18 cm. Price £50/$85 (hardbound; ISBN 978 1 107 01395 7).

Neutrino physics is at a very interesting point, with new terrestrial experimental results and cosmological observations putting tantalizing constraints on the properties of neutrinos, as we come close to determining their detailed nature and masses. Recent measurement of the mixing angle θ_{13} from neutrino experiments has been supplemented by limits on the sum of neutrino masses from cosmological observations in the cosmic microwave background (CMB) and, more stringently, through observations of the matter power spectrum derived from galaxy-clustering studies. The sensitivity of CMB and matter fluctuations to neutrino masses is intriguing in that the current limits on total masses are similar from those observations as from terrestrial experiments.

Furthermore, CMB observations hint at the possibility of extra relativistic species, which may be neutrinos; and future cosmological observations, through measurements of CMB polarization and weak gravitational lensing, for example, may be able to measure neutrino masses as well as determine the hierarchy, and their nature as Dirac or Majorana particles. On the ground, neutrino experiments may unambiguously determine the antiparticle nature through neutrinoless double-beta decay. Thus we see that the neutrino field is enormously rich on both fronts, and a text that sets out the theoretical, experimental, and observational status, along with the associated background in particle physics and cosmology, is very timely and relevant. This book does that job, and does it superbly well. It would be unrealistic to expect a book of this size to be able to set out all the physics in a completely understandable way to someone without a strong background in the relevant physics, but the authors do an admirable pedagogical job within that constraint. Particle-physics context is given in some detail, as are relevant aspects of cosmology, including the thermal history and the challenges of modelling perturbations involving massive neutrinos. The signatures of massive neutrinos and of extra neutrino species in cosmological observations are considered in some detail, and the prospects for the future are discussed. In all, this is an authoritative text for anyone who wants to understand neutrino physics from particle and cosmological perspectives, and is highly recommended. — ALAN HEAVENS.

The First Galaxies: Theoretical Predictions and Observational Clues, edited by T. Wiklind, B. Mobasher & V. Bromm (Springer, Heidelberg), 2013. Pp. 429, 24 × 16 cm. Price £117/$179/€139·05 (hardbound; ISBN 978 3 642 32361 4).

The First Galaxies is one of several recent books which attempt to review our current understanding of the rapidly changing research area of astrophysics dealing with the formation, evolution, and impact of the first stars and galaxies to form in the early history of the Universe. Huge amounts of research effort have been invested in this area in recent years, with astronomers pushing the boundaries of what is possible with the latest generation of instrumentation and taking advantage of ever increasing amounts of computer power. As a result, the review provided by this book is both well motivated and timely.

The book is structured into three main sections, beginning with detailed discussions of our current cosmological model, the epoch of cosmic re-ionization and our current theoretical understanding of the formation of the first, metal-free, stars at very high-redshift (*i.e.*, so-called Population III stars). Moving on, the second section of the book provides an excellent summary of recent progress in our understanding of high-redshift galaxies from an observational perspective and discusses what we have learnt about the evolution of the first galaxies and supermassive black-holes from recent theoretical work and the latest simulations. The majority of the third section comprises a discussion of what information can be extracted from modelling the spectral-energy distribution of high-redshift galaxies and what can potentially be learned about high-redshift star formation *via* studies of extremely-metal-poor stars in the local Universe. The final review article in the book summarizes some of the key observational issues currently under investigation and discusses the potential impact of new observatories such as *JWST* and *ALMA*.

Overall, there are several features of this book which would lead me to recommend it, with particular relevance to postgraduate or advanced-level undergraduate students who are beginning their research careers in this area. Firstly, each of the individual review articles is generally well written, by authors who are genuinely leading research in their chosen subject areas. Moreover, because the book comprises only nine articles in total, despite being over 400 pages long, each author has been allowed sufficient space to make a genuine effort to begin with a pedagogical discussion before proceeding to a more detailed description of key issues in current research. Finally, in several cases (*e.g.*, discussions of cosmic re-ionization and the formation of the first Population III stars) there is a reasonable level of overlap in the introductory material presented in the different review articles. Rather than being redundant, I actually found this level of modest overlap beneficial, gaining additional insight as a result of being provided with alternative explanations of the same subject.

As is inevitable with books reviewing the current state-of-the-art, some of the material presented here will date on a reasonably short time-scale. However, over the next few years I think that this volume will serve as an excellent research resource for advanced undergraduate and post-graduate researchers studying high-redshift galaxy formation, either from a theoretical or observational standpoint. — ROSS MCLURE.

The Twilight of the Scientific Age, by M. López Corredoira (BrownWalker Press, Boca Raton), 2013. Pp. 208, 21·5 × 14 cm. Price $25·95 (about £17) (paperback; ISBN 978 1 61233 634 3).

This book (so the end cover assures us) "gives a challenging point of view about science and its history/philosophy/sociology. Science is in decline ... Our society is saturated with knowledge which does not offer people any sense in their lives." It is not the first time that López Corredoira has written in this vein. A previous review (**129**, 32, 2009) concluded that he had a large chip on his shoulder. It has not got any smaller. He starts with some valid points, but expresses them in such impolite language and with unrelenting bigotry that what we have is a one-sided diatribe against just about everything that we and our scientific colleagues stand for. He deplores the way nobody nowadays is "great" like Newton and Einstein; there is nothing left to discover, and all we do is spend increasingly large sums of money on diminishing returns. He uses 'science' synonymously with 'technology', and heaps the faults of the latter upon the former. Money means power, the prime bent of every modern scientist. The worst criminals are the Institutions, their swaggering administrators spending huge amounts for their own ends and doing nothing constructive. Since López Corredoira has tenure at the IAC it amazes me that the Spanish tax-payer supports someone who writes about the awful evil in such places.

If his writings are to be convincing, López Corredoira must present well-balanced, carefully-argued statements. Shrill writing is counter-productive, as any traces of reason become lost upon an irritated reader. The language is appalling; words like "ballyhoo" and "rubbish" describe scientific publications; those outside his own circle are fools, "common" people (*i.e.*, not Newtons or Einsteins) who only have enough intelligence to "worry about food, sex and power". The peer-review system comes in for considerable bashing, while training students by giving them topics to work at is nothing but slavery for the glory of the fame-grubbing staff member. And so it goes on.

When those topics are exhausted, the book then turns to the philosophy of science, but leaves the reader no happier because — despite having a PhD in Philosophy — López Corredoira repeatedly confuses 'truth' with 'knowledge'.

He ends by visualizing a "new humanity" of hugely downsized "science": salaries slashed and facilities shrunk in order to encourage quasi-monastic devotion to research, the scientific *hoi polloi* seeking better careers in worldly occupations which certainly do not help humanity. Has the age of big discoveries really passed? Are the *Hubble* images not worth looking at, or the details of exoplanets not worth the trouble? Should we weaken scientific research, and turn out all those over 50 since they cannot possibly have another new idea by that age? Are discoveries really made only by gentlemen scientists in isolation? Money, he says, should be used to build hospitals, not to fund research. Mister, an isolated person isn't going to find a cure for cancer just by sitting and thinking about it.

I want to call it a terrible book and leave it at that, but the photograph of the Teide Observatory (Tenerife) on the front cover is worrying. It suggests that the author is speaking on behalf of astronomers, though apart from labelling large new telescopes "expensive toys", astronomy gets no mention. Yet I would urge people to look through it, in order to be forearmed should the patient general public get to see it and assume its opinions are widespread. I fear this book will do damage. I sincerely hope it will be avoided. — ELIZABETH GRIFFIN.

Engaging the Heavens: Inspiration of Astronomical Phenomena V (ASP Conference Series, Vol. 468), edited by M. Bolt & S. Case (Astronomical Society of the Pacific, San Francisco), 2012. Pp. 187, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 818 3).

This is a collection of papers arising from the fifth (2008) meeting in the INSAP series and, like the proceedings of the sixth meeting held two years later and published in 2011 (see review in **132**, 42, 2012), offers a wide range of contributions: cultural associations of astronomy, accounts of contemporary art works inspired by astronomy, intriguing by-ways of history, and experience of astronomical outreach. We are used to stunning astronomical images through books, exhibitions, and 'Astronomy Picture of the Day', and it is interesting to see artists' responses. As explained in the *Introduction*, the volume cannot reproduce the conference experience, because many presentations consisted of musical, artistic, and poetic expressions, but descriptions of some made it to the volume to convey the presenters' ideas. One contribution where the author could have given us more is Karen Keck's on the poem *Planetarium*, which would have benefitted from a copy of the poem itself. Pangratios Papacosta discusses an impressive bridge-building exercise: giving students taking science courses, as general education requirements at his college, assignments to integrate art forms with science concepts of their choice. Amongst the historical by-ways, Rolf Sinclair compares the most commonly seen pictures and statues of Newton and Einstein: the former is depicted with reverence and the latter often as an almost comic figure. Sinclair does not offer an explanation — I wonder if some of the high regard for Newton after his death arose from his public role as Warden of the Royal Mint — but he does give us some rarely seen images of Einstein at the height of his powers. I was impressed by the responses to the 2004 transit of Venus described by Chuck Bueter, and intrigued by the influence of Wernher von Braun on Chesley Bonestell's painting of a lunar-landing craft. The most substantial contribution is Michael Shank's sweeping historical survey of the perceptions of Saturn from the earliest times to that of Galileo. The association of Saturn with misfortune is very old: Babylonian astrologers considered birth under Ninurta, its name in Akkadian, to be a bad omen. The melancholic association continued through the ages to Dürer's

Melencolia I and Milton's *Il Penseroso* and, on the way, Shank gives us mythology, astrology, Roman Imperial and Medician politics, medieval medicine, magic squares, and more, with excellent illustrations and copious references. Overall, the book is well illustrated and edited. Anyone interested in outreach should take a look. — PEREDUR WILLIAMS.

Signatures of Life: Science Searches the Universe, by E. Ashpole (Prometheus, Amherst), 2013. Pp. 227, 23 × 15 cm. Price $25 (about £16) (hardbound; ISBN 978 1 61614 668 9).

This is another of those books about unidentified flying objects — UFOs — believed to be of extraterrestrial technological origin. This is *X-File*-like stuff. It also appears to be the product of Ashpole's wishful thinking — lots of soft notions and no satisfactory logic.

Ashpole mixes all this with the topic of SETI, since the days of Struve and Su-Shu Huang as well as Cocconi and Morrison. In some ways this is like mixing H. G. Wells' science fiction *The War of the Worlds* with SETI. The rest of the book is about enterprising apes, unknown life forms, life's machines, big brains, and the robots, *etc.*, touching upon theological as well as philosophical speculatives. Regarding apes, perhaps Ashpole ought to look at the fiction series *Planet of the Apes*, a saga now on DVD.

With emphasis on philosophy and science, it is plausible there exists some form of life on other worlds. This is a profound issue, to be resolved by dedicated scientific enterprises — astrobiology and bioastronomy.

Ashpole underestimates the importance of these enterprises, including SETI. His book demonstrates that he is preoccupied by his belief in the presence of UFOs, in spite of the fact that there is no solid evidence of a visit having occurred. In summary, it is a book from the fringe. Not recommended. — P. CHAPMAN-RIETSCHI.

Feeding Compact Objects: Accretion on All Scales (IAU Symposium No. 290), edited by C. Zhang, T. Belloni, M. Méndez & S. Zhang (Cambridge University Press), 2013. Pp. 389, 25 × 18 cm. Price £76/$125 (hardbound; ISBN 978 1 107 03379 5).

Held in conjunction with the last IAU General Assembly, the well-attended IAU Symposium 290 was dedicated to the subject of accretion onto compact objects. This volume splits roughly into two, with longer papers (talks) in the first three chapters and short, two-page papers (posters) in Chapter IV. Chapters I and II cover "black holes and accretion jets" and "neutron stars and white dwarfs", respectively, with a single eight-page paper on the proposed *LOFT* X-ray mission as Chapter III. Appearing just over 18 months after the symposium, it's not obvious why anyone would wish to read through 104 poster papers

After an introduction to the subject by Andy Fabian at the start of Chapter I, there follows a set of papers concentrating on what variability signatures can tell us and the nature of discs and jets over a very large range in black-hole mass. Linking the inflow through the disc with outflow through jets and winds was a common theme of those papers. Chapter II concentrates on the stellar-mass end of things but with an equally bewildering range of phenomena. Lest anyone think it's all about X-rays, there were some nice results presented from the optical, sub-mm, and radio bands.

IAU Symposia should be well attended, as this was, but also accessible to own by students and postdocs *via* low-cost proceedings, as this is *not*, thanks to the very high price of this hardback CUP volume. If your institution has a suitable subscription you can read it on-line, but otherwise this is one for the library shelf. — PAUL O'BRIEN.

Astronomical Data Analysis Software and Systems XXI (ASP Conference Series, Vol. 461), edited by P. Ballester, D. Egret & N. P. F. Lorente (Astronomical Society of the Pacific, San Francisco), 2012. Pp. 918, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 804 6).

ADASS conferences have been held annually since 1991. They provide one of the few ways in which astronomers can publish their ideas, plans, and achievements in the fields of software development and computer-system design. Since practically all working astronomers are utterly dependent on both the software and the hardware systems that have been designed and implemented by the people who frequent these conferences, the ADASS proceedings deserve to be more widely known. Fortunately the individual papers are all freely available on-line from the adass.org website. Incidentally a new journal called *Astronomy and Computing* provides another much-needed way of publishing papers in this area, but the ADASS proceedings are likely to remain important, not least because of the variety and sheer number of papers they contain, almost 200 in this volume alone.

The Virtual Observatory (VO) remains the single most popular subject area, with 51 papers here. It is good to see that efforts to promote interoperability are now so widespread and that the VO is reaching something approaching maturity. The next largest number of papers is on observatory operations and scheduling systems, which may be helpful to those using particular observatories or designing something similar for other projects.

Of more general interest are the papers on the problems posed by the next generation of observatories, especially because of the large data rates and volumes involved. Space observatories such as *Gaia* and *JWST* are covered here, but the limited telemetry bandwidth from spacecraft means that the computing problems on the ground arise more from complexity than data volume. The data-volume challenges come from terrestrial telescopes. The *LSST*, for example, expects to produce 30 terabytes/night, while the *LOFAR* array will soon be generating 10 terabytes/hour of scientific data, and expects to have an archive of 25 petabytes by the end of 2014. To handle this, the *LOFAR* project has chosen to abandon the FITS format, otherwise well-entrenched in astronomy, adopting instead HDF5 because of its fully hierarchical structure and greater power and flexibility. HDF5 has been used for some years in other branches of physics and biology including Earth-observation science. It will be interesting to see whether other large-data projects follow their lead, or whether the FITS format is capable of meeting the challenge.

Papers on The Grid have dwindled to a couple this year, but Cloud Computing, a rather similar concept, is clearly a rising trend. It was also interesting to see that astronomers are starting to make serious use of arrays of GPUs (graphical processing units) in their quest for ever larger number-crunching power to handle the data deluges. There were 15 papers devoted to their use at this conference.

Since astronomical research often requires comparison with results from earlier epochs, there are many papers on provisions for data archiving. This year some authors have pointed out that it is not enough to preserve the scientific data from past observations, it is also vital to preserve the data-analysis capability, which means preserving old software and the associated documentation as well.

Overall the ADASS conferences continue to be a unique forum for the exchange of ideas, and these printed volumes are a valuable record of what was presented at them. In a rapidly advancing field it is all the same unfortunate that by the time the papers appear in print they are already a more than a year out of date. — CLIVE PAGE.

Star-Craving Mad: Tales from a Travelling Astronomer, by Fred Watson (Allen & Unwin, Sydney), 2013. Pp. 334, 20 × 13 cm. Price AUS$24·99 (about £15) (paperback; ISBN 978 1 74237 376 8).

It seems that Fred Watson and I had the same kick-start into the world of astronomy: a set of 50 picture cards entitled 'Out into Space' to be found in packets of tea way back in the mid-1950s. And although our paths have crossed frequently in the intervening years (at the University of St. Andrews, the Royal Greenwich Observatory at Herstmonceux, and most recently in a restaurant in Oxford), his journey has been, I think, rather more exciting than mine. His career really took off when he headed for Australia where he was involved with *UKST* and latterly a number of high-tech, high-profile, instrumental projects aimed at survey work on massive scales. So where he got the time to take on outreach duties — to the point where he's become Australia's answer to the late Sir Patrick Moore — and to lead 'astro tours' I have no idea.

It is the 'astro touring' that sets the framework for this latest book, in which Fred has taken parties of antipodeans on visits to a number of famous astronomical sites, from prehistoric monuments (Nazca lines, Machu Picchu) in South America, *via* Tycho's observatory on Ven (Hven), to some of the world's major modern observing facilities. And on that framework he hangs a wealth of information on a vast range of topics from the naming of Pluto through to the possibility of multiverses, all explained in an amazingly user-friendly way, with humour and analogies one can readily appreciate. History, astronomy, astrophysics, cosmology — they are all here and are accessible to everyone. And we get quite a few insights into Fred's thoughts on life, the Universe, and everything else. For the interested layman, budding student, and many others, this book will be an inspiration; I found it fun and a wonderful read. — DAVID STICKLAND.

Astronomy 2014 Calendar, edited by T. Dickinson (Firefly Books, Richmond Hill, Ontario), 2013. Pp. 22, 30 × 35·5 cm. Price $14·99 (about £9) (ISBN 978 1 55297 489 6).

Terence Dickinson is a well-known advocate for astronomy in Canada and he has brought together twelve stunning images to create a beautiful — and useful — calendar for 2014. The pictures range from a photo from the *International Space Station*, showing a Perseid meteor entering the Earth's atmosphere, out to the glorious spiral galaxy NGC 3259. Below each picture (when hung on your study wall) is a large-format calendar for the month, with space enough for appointments, *etc.*, together with historical events; for example, in the box for February 18, it is noted that on that day in 1930 Clyde Tombaugh discovered Pluto; the phases of the Moon also appear. To complete the page, the photo is described with an appropriate snippet of astronomical information. A nice Christmas present indeed! — DAVID STICKLAND.

Stargazers' Almanac 2014, by Bob Mizon (Floris Books, Edinburgh), 2013. Pp. 32, 29·5 × 42 cm. Price £14·99/$25 (ISBN 978 086315 945 9).

The now-familiar *Stargazers' Almanac* has appeared in good time for Christmas as a potential present for anyone with an interest in the happenings in the night sky and within a reasonable latitude of 52° North. With one month to an opening, the celestial panoramas to the north and south are shown for an observer shortly before bed time (around 10 pm for mid-month), together with any special Solar System events. Highlighted this year are the contributions of Galileo and Copernicus to the advance of astronomy in the 16th and 17th Centuries. Finally, the 25th anniversary of the BAA's Campaign for Dark Skies is celebrated. — DAVID STICKLAND.

THESIS ABSTRACT

DYNAMICAL ASPECTS OF EXOPLANETARY SYSTEMS

By Giammarco Campanella

The detection of more than 130 multiple-planet systems makes it necessary to interpret a broader range of properties than is shown by our Solar System. This thesis covers aspects linked to the proliferation in recent years of multiple extrasolar planet systems.

A narrow observational window, only partially covering the longest orbital period, can lead to solutions representing unrealistic scenarios. The best-fit solution for the three-planet extrasolar system of HD 181433 describes a highly unstable configuration. Taking into account the *dynamical stability* as an additional observable while interpreting the radial-velocity data, I have analyzed the phase space in the neighbourhood of the statistical best-fit. The two giant companions are found to be locked in the 5:2 mean-motion resonance in the stable best-fit model.

I have analyzed the dynamics of the system HD 181433 by assessing different scenarios that may explain the origin of these eccentric orbits, with particular focus on the innermost body. A scenario is considered in which the system previously contained an additional giant planet that was ejected during a period of dynamical instability among the planets. Also considered is a scenario in which the spin-down of the central star causes the system to pass through secular resonance. In its simplest form this latter scenario fails to produce the system observed. If additional short-period low-mass planets are present in the system, I find that mutual scattering can release planet b from the secular resonance, leading to a system with orbital parameters similar to those observed today.

Finally, I have studied the evolution of low-mass planets interacting with a gas-giant planet embedded in a gaseous disc. The transit-timing method allows the detection of non-transiting planets through their gravitational perturbations. I have investigated the detectability of low-mass planets neighbouring short-period giants after protoplanetary-disc dispersal. — *Queen Mary, University of London; accepted 2013 July*.

OBITUARY

Gilbert Elliott Satterthwaite (*1934–2013*)

Born in Norwich on 1934 May 22, Gilbert soon moved to Dorset where two generations of his family were involved in providing electrical contracting and accounting services to the new army camps and also carrying out conversions of manor houses into staff headquarters. His interest in astronomy began when he was a schoolboy at Weymouth Grammar School and he was taught for a while, and also influenced in later life, by Dr. Arthur F. O'D. Alexander, who was a prominent member of the British Astronomical Association and at that time Director of its Saturn Section. Whilst at school Gilbert sent off a copy of *Worlds Without End* by the Astronomer Royal, Sir Harold Spencer Jones, to the author at Greenwich, who duly signed and returned the volume and he encouraged him to apply for a post at Greenwich. He attended for interview in 1952 to face a series of questions which became harder as the interview progressed. Fortunately, L. S. T. Symms, the head of the Meridian Department, was on the panel and came to his aid, and Gilbert got a job in the Meridian Department as a Scientific Assistant. There he became a regular observer with the *Airy Transit Circle* and was, by chance, the last observer to use that famous instrument before it was retired and replaced by a more modern version when meridian observing was transferred to Herstmonceux in 1954.

While never wavering in his dedication to astronomy, especially its history, nevertheless in 1957 Gilbert left the Royal Observatory and pursued other avenues: first in scientific publishing (with Pergamon Press and later the Geological Society of London and Pitman Publishing); and thereafter displayed his considerable practical skills as Senior Laboratory Technician at Ravenswood Boys School, and finally in the Applied Optics Teaching Laboratory at Imperial College, where he remained an honorary member following his retirement in 2001. In parallel, he gave adult-education and evening classes in astronomy, including lectures at the Greenwich Planetarium, over a period of 32 years. His *Encyclopaedia of Astronomy* was published in 1971, he co-edited the 16th version of *Norton's Star Atlas* in 1973, and he made guest appearances on the BBC.

Gilbert was active in a number of astronomical organizations, particularly the Orpington Astronomical Society (of which he was a founder member, and President at the time of his death), the Society for the History of Astronomy (of which he was a founder member and Chairman from 2004 to 2011), and the British Astronomical Association (which he joined in 1948); he was also an Associate of Commission 41 of the International Astronomical Union (History of Astronomy). His observational interests also included Saturn, and he was the Director of the BAA Saturn Section from 1970 to 1974. His other wide-ranging interests included music, opera, literature, and poetry.

Gilbert obtained his degrees by studying part-time — first a BSc and then in 1995 he was awarded an MSc from Imperial College, London, for his thesis entitled *The History of the Airy Transit Circle at the Royal Observatory, Greenwich*. He once confessed that he was "an Airy nut", often volunteered to show groups of visitors around the *Transit Circle* at Greenwich, and also took a particular interest in the maintenance of the *ATC* in good condition. In 2004 March he helped to organize a meeting to commemorate the 50th anniversary of the last observation made with the *Airy Transit Circle*. It was held at Greenwich on March 30 and is recorded in these pages[1] together with pictures of Gilbert at the *ATC* with three former colleagues and fellow observers.

Gilbert was elected a Fellow of the RAS on 1954 January 8. He died peacefully at his home in Hayes on 2013 July 20 and is survived by his son Martyn and daughter Joy. — R. W. ARGYLE.

[Further memories of the work of the Greenwich transit observers can be found elsewhere in this issue[2], and in ref. 1. — Ed.]

References

(1) R. W. Argyle, *The Observatory*, **124**, 229, 2004.
(2) P. D. Gething, *The Observatory*, **133**, 351, 2013.

Here and There

DAMNED WITH FULSOME PRAISE

Beginning his presentation at Astrofest in 2011, the American lunar scientist Chuck Wood paid fulsome tribute to the contribution to the history of selenography by the B.A.A. Lunar Section. — *JBAA*, **123**, 143, 2013.

AND STAR-FILLED DAYS

... the only infrastructure [in Antarctica] is an orbiting satellite or two, impossible to see in the sun-filled nights of the astral summer. — *Victoria Times-Colonist*, 2013 June 16, p. D9.

FOR THE BEST IN THEIR FIELD

Pasture Institute — Fourth in a list of the top ten of France's scientific institutions on the basis of the *Nature Publishing Index*, p. 20.