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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2013 October 11 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

D. J. SOUTHWOOD, *President*
in the Chair

The President. Today we award the Society's Gold Medal for Geophysics, which this year is presented to Professor Chris Chapman for his outstanding personal and collaborative research in geophysics. He has been one of the world's leading theoretical seismologists for the past 40 years. His early papers are still standard reading and his software is widely used, to the point where even subroutine names are known. A sequence of influential papers on layered media and wave progression led to the WKBJ seismogram algorithm and Maslov–Chapman seismogram, and arguably to the first working formalism for a full wave inversion. Later, Professor Chapman worked on anisotropy in shear-wave coupling, which led to the so-called error Born generalization of linearized Born scattering. Work in the '90s ranged from ground-breaking anisotropic tomography work to finite-difference modelling. His work with Thomas Jordan on the nature of waveform-inversion sensitivity kernels, publications on basic ideas such as linear slit-interface reflections, applications of extended ray theory, and even a new layer-matrix-reflectivity algorithm all appeared while Professor Chapman was working at Schlumberger. Most recent is a novel moment-tensor source decomposition, which expresses the interplay between dipoles, volume-source effects, anisotropic elastic properties, and radiation patterns insightfully for both reservoir microseismics and deeper studies. So Chris, let me present you with the Gold Medal. [Applause.]

The next item on the agenda is the Eddington Medal. The Eddington Medal is awarded to Professor James Binney from the University of Oxford in recognition of his fundamental and enduring contributions to galactic astrophysics. His research has developed critical and influential insight into the roles of rotation and anisotropic velocity in the flattening of elliptical galaxies, the modelling and consequences of cooling flows in clusters of galaxies, the use of the local kinematics to infer the dynamical and chemical evolution of the disc of the Milky Way galaxy and the influence of radial mixing, resonant excitation and maintenance of bulges and warps, mass modelling of galaxies, and accretion onto galaxies. He has written several books, of which *Galactic Dynamics*, written with Professor Scott Tremaine and now in its second edition, is a pedagogical masterpiece and has been so for 25 years. The rigour of Professor Binney's approach to science, the skill with which he combines mathematical analysis, numerical simulation, physical insight, and observational constraints, and his ability to generate novel and exciting ideas, and to recognize when an idea may need to be abandoned, have combined to make him one of the leading theorists of his generation and a truly worthy recipient of the Eddington Medal. [Applause.]

Now we come to the Winton Capital award to Dr. Katherine Joy. She is awarded the 2013 Winton Capital award for geophysics for her pioneering research unravelling the impact history of the inner Solar System through the study of lunar samples, including both lunar meteorites and Apollo samples. Dr. Joy's personal commitment to this is clearly evidenced as she is undertaking work in extreme environments, such as Iceland and Antarctica. Dr. Joy's work combines laboratory geochemical analysis of Moon samples with the analysis of spacecraft data, most notably from the ESA *SMART-1* mission. Her research at the Houston Lunar and Planetary Institute enabled her and her colleagues to identify probable fragments of the lunar basin-forming impactors. That has allowed the source population of asteroidal impactors in the early Solar System to be better constrained. I think this all together makes Dr. Joy a very worthy recipient of the Winton Capital award. [Applause.]

We now move to the scientific programme. The next speaker will be the Winton Capital award winner, Dr. Joy. The floor is yours: 'Unravelling the temporal history of the Moon'.

Dr Katherine Joy. Impact bombardment is a fundamental and ubiquitous Solar System process. It is the action of collisions of planetary bodies such as planet debris, asteroids, icy bodies, and small fragments of those materials. Collisional rates were higher early in Solar System history, when more material existed from accretional debris. However, impacts to the Earth and other Solar System bodies are still happening at the present day (for example, just a few months ago the Russian Chelyabinsk meteor explosion was international news), and it is important that we understand impact-bombardment causes and effects.

Impact bombardment can occur on all scales. It can be catastrophic — such as giant impacts that were responsible for planetesimal disruption. However, it is also a process of creation — new worlds forming from old ones. Without giant impacts we wouldn't have the Earth that we flourish on today. Impacts are the creators of new planetary crusts and have a role in creating environments where life can flourish.

Scars of past impacts can be found on all planetary bodies — rocky and icy, water covered and not — and at all scales, from huge basins hundreds of km in diameter (*e.g.*, the 2300-km Hellas basin on Mars) to the microscale (*e.g.*, micron-scale-sized craters found on individual lunar glass beads). The number, size, shape, and products of these scars provide us with information about ages of planetary surfaces (using superposition relationships), the structure and stability of planetary crusts (thermal history, their brittle/ductile nature, ice-relaxation effects), and the nature of the impactors that caused them.

The Moon is an archive of impact cratering in the Solar System throughout the past 4·5-billion years. It preserves this record better than larger, more complex planets like the Earth, Mars, and Venus, which have lost their ancient crusts through geological reprocessing and water/climate weathering action. Evidence for the lunar impact record comes from both surface morphology and geophysical information derived from remote-sensing missions (*e.g.*, the number and size of basins and craters, relative ages of those structures), and the lunar-sample collection, which provides evidence of timing from isotopic resetting of rocks and impact-melt crystallization events, and the nature and sources of bodies impacting the lunar surface.

The lunar-impact record itself is often controversial, with several different models of past impact-bombardment flux. It is generally agreed that rates of impacts were high immediately after the Moon's formation at ~4·5 Gy. All of the Moon's large impact basins were formed between this time and ~3·8 Gy. However, the duration and magnitude of basin formation is not well known. It may be that there was a sudden spike in bombardment between ~3·9 to 3·8 Gy when many basins formed (this is known as the lunar-cataclysm hypothesis), or it could be that there was a period of late heavy bombardment lasting from ~4·2 to 3·8 Gy. Constraining this record is vital as it is important to realize that whatever happened on the Moon could have happened on the Earth by up to about 17 times more, affecting our atmosphere and biosphere and maybe having important implications for the onset and proliferation of life.

There are lots of lines of evidence to consider and debate. One limitation of datasets we are using is that samples returned by the Apollo missions were all collected from the nearside of the Moon, and were influenced in one form or another by the large Imbrium-basin-forming event at 3·85 Gy. That event, thus, may have biassed our sample record. The good news is that we have the new lunar-meteorite collection which provides us with new samples of lunar-impact events from all over the Moon's surface. This is the focus of my current research project in Manchester where we are using argon age-dating methods to measure the ages of small rock fragments in lunar meteorites.

In addition to determining the temporal impact record, it is important to understand the sources and causes of bombardment, and investigate if the sources of projectiles have changed with time. Evidence provides geochemical constraints for dynamical models of the Solar System, and insights to the transfer of meteoritic material throughout the Solar System and delivery of volatiles and organics to Earth.

Chemical signatures of material accreting to the Moon have been detected generally in the form of highly siderophile elements (HSEs) and volatile elements. Mature lunar regoliths exposed to space for tens to hundreds of millions of years have ~1·6% to 3·4% added siderophile-rich material with average CI/CM-like carbonaceous chondritic meteorite compositions. HSE analyses of individual lunar-basin impact melts imply projectile compositions similar to both chondritic and differentiated bodies that are interpreted to be asteroids rather than comets or Kuiper Belt objects. Fragments of impactors in the lunar regolith provide direct evidence of the types of small bodies striking the Moon. Ancient samples, which provide insights to projectile delivery in the latter stages of the basin-forming epoch (3·8 to 3·5 Gy), contain projectile fragments that appear to be primitive asteroid-like bodies that are compositionally dissimilar from meteorites being delivered to Earth at the present day. In younger samples (<2·5 Gy) there are more diverse types of impactors, suggesting a possible change in projectile (asteroid types) delivered to the inner Solar System with time. Work is on-going to investigate a wide range of Apollo regoliths with different ages to test these hypotheses.

The global view of impact cratering is also changing. Missions that are currently in orbit around the Moon are returning very-high-resolution images and geophysical datasets of the lunar surface. They are allowing us to determine the size and structure of lunar craters as never before, helping to reassess the impact record and improve crater-counting statistics. One such tool that is contributing to this effort is the web-based 'Moon Zoo' citizen-science project (see http://www.moonzoo.org/) which enables people at home to measure and count lunar craters and identify interesting surface features. Such crowd-sourcing initiatives will both address lunar-impact scientific questions and engage the public with lunar and planetary science.

There is a lot of research still needed to decipher fully the bombardment history of the inner Solar System. The ancient and accessible nature of the lunar surface makes it an ideal place to study impact-cratering processes. Future sample analysis and manned and unmanned missions to the Moon should address these high-priority scientific questions.

I am very grateful to Winton Capital for their generous prize money, and to the RAS committee who made the award. My research over the last ten years has benefitted from collaborators in the UK at Birkbeck College, University College London, the Natural History Museum, the Rutherford Appleton Laboratory, the University of Manchester, and in the US at the LPI, NASA–JSC, and the University of Hawaii. Thank you especially to Professor Ian Crawford (Birkbeck) for his help and support throughout my career.

The President. One or two quick questions?

Rev. G. Barber. The Russian samples from the Eastern side of the Moon — do they show the spike at 3·8 billion years?

Dr. Joy. That is a good question. They show the ages of the lava flows from which they were collected. There are very few impact materials within the lunar samples. It's mostly the lava flows that they date. I don't think anybody has actually identified specific impact events in that particular record, so it is mostly the large hand-specimen samples that were collected by the Apollo astronauts themselves. If we go back and sample the Moon and we send any kind of robotic sample-return missions, we need to be really smart about where we send them and whether we send them to an impact-ejecta sheet rather than a lava-flow site, or to one of the central-peak areas to collect debris.

Rev. Barber. So the lava flows are more recent?

Dr. Joy. They are much more recent, yes.

The President. Thank you very much — that was excellent. [Applause.] Now I am very happy to introduce another Winton Capital award winner: the astronomy award winner, Dr. Baojiu Li from Durham University, presenting 'Numerical simulations for theories of dark energy'.

Dr. B. Li. Our current standard theory of gravity is General Relativity (GR), which was proposed by Albert Einstein in 1916. It predicts that the geometry of space and time is determined by the distribution of matter and also determines the motion of matter. The theory has had tremendous success in various aspects and, most surprisingly, successfully predicted the expansion of the Universe, which was confirmed by observers later. Since then, Einstein's theory has become the foundation of modern cosmology. GR is a very complicated theory, and its equation that governs gravity is highly nonlinear. Exact analytical solutions have so far been found only in a small number of ideal situations, while computation is needed in most other cases.

It then raises the question as to why cosmologists are seriously considering alternatives to GR as the underlying theory of gravity. There are two main reasons. First, to explain the recent observation that our Universe is undergoing an accelerated expansion in the framework of GR, one has to incur the so-called 'dark energy', which appears to generate repulsive gravity and contributes to over 70% of the energy budget in the Universe; this is clearly not a favourable idea for many. Second, GR has so far been accurately tested only in small systems such as our Solar System, and assuming that it applies also to the largest scales of the Universe is an idea which, again, makes many people feel uncomfortable. For those, amongst other, reasons, it remains important to study alternatives to GR and put such theories to rigorous tests in the cosmological context.

There are many alternative theories to GR, and many more are being proposed. But most of them are of little concern to cosmologists, who are more interested in the so-called 'screened' gravity theories, which keep GR's success at small scales while attempting to cure its failure at cosmological scales. This means that any deviation from GR is 'screened' at scales either larger than the Solar System or below sub-millimetre, where current tests of gravity are still inconclusive. There are currently two known mechanisms which can achieve this: the chameleon mechanism and the Vainshtein mechanism. In both cases gravity can deviate noticeably from GR for a single isolated matter particle; however, when that particle is put in a large collection of other particles, the deviation is strongly suppressed at either long (longer than sub-millimetre, such as in the chameleon case) or short (shorter than the scale of galaxies, such as in the Vainshtein case) distances. In other words, gravity may be different from what GR prescribes, but we cannot detect this simply because we are surrounded by too much matter.

It is worthwhile to dig deeper into these two classes of theories, and introduce the key concepts which help us understand how they work. In the case of chameleon theories, because deviation from GR is suppressed at long distances, only the deviations produced by particles within a shell near the edge of a spherical body (such as a star) can propagate far enough to be felt by an observer outside the body: if the shell is thin enough (known as the thin-shell régime), such as in the cases of the body being very massive or located in a very dense environment, the deviation from GR is negligible. In the case of Vainshtein theories, in contrast, there is a radius, known as the Vainshtein radius, inside which GR is recovered; the Vainshtein radius is of order of a millimetre for single isolated atoms, but grows quickly when more particles are added into the system, which helps suppress any difference from GR.

The fact that gravity behaves in very distinct ways for isolated and collective particles indicates that such theories are much more nonlinear in GR. For example, in a mildly complicated theory from the Vainshtein family, which theorists call the 'quartic Galileon gravity', the equation contains cubic powers of second-order partial derivatives. Finding exact solutions to such equations is almost impossible, and numerical simulations are so far the only known tool which can accurately track the complex nonlinear physics and follow its effects in cosmology. My past work has been focussed on studying such cosmological effects, from linear to nonlinear regimes.

Understanding the nature and theoretical properties of the different theories can dramatically help test such theories. In the case of the Vainshtein class, we know that the Sun's Vainshtein radius is about 10 billion times larger than its size; this number reduces to 10 for galaxies and of order one for typical galaxy clusters. This shows that deviations from GR are expected to be found on cluster scales or beyond. For example, by studying the effects of Vainshtein gravity on the power spectrum of the cosmic microwave background (CMB), we were able to place several-order-of-magnitude-stronger constraints on the theory than previous studies gave. On-going work about the gravitational lensing of the CMB photons is expected to improve this constraint further by one order of magnitude, or even rule out the simplest and most natural version of this theory. Our recent numerical simulations for such studies also revealed interesting behaviour in galaxy clusters.

The chameleon class of theories has a different story. Typically, as in the Vainshtein case, first-order deviations from GR can be found on cluster scales, which can be used to constrain the theory, as we have predicted in a series of studies. However, such constraints are generally moderate, and much stronger constraints can be achieved in a different way. To see this, remember that the chameleon deviation from GR is determined by a thin shell, whose thickness depends on the environmental matter density: if the environmental density is low, the shell can be as thick as the radius of the spherical body, causing strong deviation. If a thin shell has developed for the outside larger body, it is likely to be developed for the inside smaller body as well, and *vice versa*.

Stars reside in galaxies, which have a dark-matter halo surrounding them, and the dark-matter haloes themselves are in superclusters or voids, a situation much like that of a matryoshka. Therefore, whether a star has a thin shell depends on the behaviour of gravity in its large-scale environments. If the star has no thin shell, then it feels non-standard gravity and, because stars rely on gravity to maintain their hydrostatic equilibrium, can follow a very different life path. Strong constraints come out for two reasons: firstly, the Sun could have no thin shell in such theories, which means that local tests of gravity can already rule out much of the parameter space; secondly, the different stellar evolution due to the deviation from GR can seriously affect various properties of galaxies and therefore the spectroscopic observables.

Such an exciting prospect, however, poses great challenges to our studies. Because astrophysical observables on small scales depend on the behaviour of gravity on very large scales, we need very-high-resolution simulations to make accurate theoretical predictions, and we need better modelling of the gravitational effects in the stellar and galactic evolutions. All these will be left as future works.

Hopefully, in the next few years, there will be further major progress in these regards. By then, we will be more confident on what observational evidence has to say about the fundamental theory of gravity. In this regard, we would have the Universe as a wonderful laboratory for gravity.

The President. Quick questions?

Mr. J. C. Taylor. How about these alternative theories? In amongst all the algebra, are there adjustable parameters there?

Dr. Li. Yes there are, in some of these theories; for example, in the chameleon class of theories, if you consistently reduce these parameters then you will go back to GR. In other classes that is not guaranteed, for example in the Vainshtein theories, even if you change these parameters to zero still you don't have GR. That is why you can put strong constraints or even rule out these models.

The President. Thank you very much. [Applause.] And thank you to Winton Capital for the prizes. I think we have very worthy prize-winners.

We now come to the main dish of the session, the Harold Jeffreys Lecture, by Professor Bob White who is presently the director of the Faraday Institute for

Science and Religion in Cambridge. I will truncate the citation I have, as much of what it says is what he is going to talk about today! I will simply introduce Professor White and say that he is going to speak on 'Building the dynamic crust of Iceland by rifting and volcanism'.

Professor R. White. [It is expected that a summary of this talk will appear in a future issue of *A&G*.]

The President. Questions, comments?

Mr. M. Hepburn. It looks as if the hotspot is moving across the mid-Atlantic ridge. Is there any evidence as to which way it is going?

Professor White. Oh yes, it's moving eastwards. It is moving faster than the plate is spreading so it will reach us in Britain in 100 million years or so!

Professor Kathy Whaler. You've shown evidence of magma chambers or sills being relatively persistent features, which is rather different from the way that people had assumed that crust is built in these mid-ocean ridges. Would you put that down to the effect of the mantle plume?

Professor White. No, it's not the mantle plume. If you look at the thermodynamics of melt bodies, the time it takes to cool a body is proportional to the square of its thickness. So if a one-metre-thick sill freezes in a week, and you double the thickness to two metres, it will take four weeks to freeze. If you go to a hundred-metre-thick sill it will take 200 years to freeze. We know that some of these melt intrusions, from looking at Skaergaard and other places on East Greenland that are exposed, can be hundreds of metres thick. So, probably there's a lot of melt sitting around in the crust and it's only a fraction of each one that gets erupted. Probably only a hundredth of the volume in each sill actually gets erupted because the remaining melt is fairly dense. Most of the melt actually gets intruded into the crust and never reaches the surface. Overall the ratio of volcanic rocks extruded from the surface to those intruded into the crust is typically one to five or ten. So there is a lot of melt sitting around in the crust. Beneath Eyjafjallajökull in Iceland, prior to the 2010 eruption, molten rock remained in the subsurface for nearly 200 years since the previous eruption in the 1820s.

Professor D. Lynden-Bell. Are we likely to get some more geysers as a result?

Professor White. Yes, these areas are very active geothermally. One of the things that surprised me on my recent visit to Hawaii was that they were all complaining about the price of gasoline (about a quarter of what we pay!) despite having abundant sunshine for photovoltaic electricity, as well as wind and geothermal energy, but they're hardly touching any of it. There's a huge amount of energy available in volcanic areas.

Professor Whaler. Has there been much gas monitoring on the surface of Iceland? You talked about the CO_2 in the ductile layer. Does much of it make it to the surface?

Professor White. There has been very little gas monitoring in Iceland. It's only very recently that we've realized how important the gas is to triggering microearthquakes. There has been much more gas monitoring on Hawaii and interestingly the plumbing system is such that there is an open vent, which doesn't erupt usually, called Halemaumau, on Kilauea volcano. Then the melt moves underground about 10 km sideways until it erupts at Pu'u'O'o crater. Most of the degassing occurs at Halemaumau from this open vent. There was a really interesting observation recently, where they got a sudden increase of CO_2 at Halemaumau. Then it was followed, about three months later, by an upsurge in eruption volume at Pu'u'O'o. The CO_2 had separated from the melt at about

15-km depth and then moved up faster than the melt which erupted three months later. That is exactly the same timescale as we're seeing at Upptyppingar in Iceland. This gives you information about the permeability of the material the gas is moving through. If you could monitor CO_2 from such volcanoes it might be a good pre-eruptive warning.

A Fellow. You've shown hotspot areas like Hawaii, which is active like Iceland. Is there any significant difference between them and somewhere like Australia, where I understand there have been some uprisings but no volcanoes.

Professor White. There is a significant difference and it depends mainly on how thick is the lithosphere (that is, the rigid outer layer of the Earth) that sits above the mantle plume. In Iceland, because it's rifting apart, the lithosphere is very thin and the hot mantle can well up all the way to the base of the crust, so you get a lot of melting. In Hawaii, the lithosphere is about 70-km thick so the mantle can only well up to 70 km, so you get less melting. In Australia, the lithosphere is up to 200-km thick, so the mantle can never well up far enough to go above the melting point as the pressure decreases. So you can get uplift because the lithosphere is underlain by hot, buoyant mantle, but there is not significant melting. Underneath continental blocks you generally don't get much melt from mantle plumes, although there are lots of hotspots in North Africa (such as Tibesti, Hoggar, and other places) where there is very little melting. In contrast, there is a mantle plume that Prof. Kathy Whaler has worked on under Afar, where again there is a rift, a hotspot, and there is a lot of melting. So it depends on how thick that lithosphere lid is because that is what shuts off mantle melting as the mantle wells up to the surface. If you thin the lithosphere lid by rifting you get much more melting.

Mr. Hepburn. When you look at the ore bodies in North Yorkshire, it looks as though they've been shifted by superheated water rather than other fluids. Do you get that as well in your Icelandic places?

Professor White. The geothermal water above active volcanoes is certainly superheated and we've been working round the geothermal area called Krafla in the north of Iceland, another volcanic system. I have a story related to the gung-ho nature of Icelandic drillers. They were drilling in Krafla to try to reach superheated water at 450° C, because they get much more energy out of it than out of lower-temperature steam. When you get to high pressure you can carry more heat in superheated water, which sits above the magma chamber which is at about 1200° C. There is a very thin zone of cracked rock between the magma chamber and the geothermal circulation. So if you could penetrate that layer you could get enormous amounts of energy out. But they didn't bother to do any geophysics first. We'd been doing seismic work there and we knew roughly the depth and position of the magma chamber containing molten rock. But they never asked us where it is. They just went out with their big equipment and drilled a hole. They got down to 2 km and instead of hot water they got molten rock [laughter]. It didn't come out at the surface because it hit the drill stem and clogged it up first. So, being engineers, they pulled the rest of the drill string out, put some explosives down to blast the hole open and drilled again [laughter]. They drilled a side diversion hole and that got bunged up by magma so they eventually gave up.

The President. Thank you very much again. [Applause.] I will close by reminding you that there is a drinks reception in the RAS Library around the corner immediately following. The next A&G Open Meeting will be on 2013 November 8.

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 235: HD 48913, HR 6853, HD 206843, AND HR 8589

By R. F. Griffin
Cambridge Observatories

The four stars have found their way onto the Cambridge observing programme in independent ways. HD 48913 is of interest as a composite-spectrum object, but as it is on the faint side for good spectroscopy with instrumentation available to the writer it is being presented simply as a 'single-lined' binary (the radial velocity of the late-type component, only, can be measured with the *Coravel* spectrometer). It has an orbit of moderate eccentricity, whose period of nearly 23 years is determined within a standard error of only 23 days. HR 6853 is a 6^m metal-deficient giant star, already known to be a spectroscopic binary, which has proved to have a 13-year orbit, again of moderate eccentricity. HD 206843 is very different, having been pointed out as a prospective binary system by Suchkov on the basis of his 'over-luminosity' criterion, ΔM_{c_0}. It seems, after all, not to have an observable secondary component, but it is certainly a binary system, with a period of only nine days and an orbit of small but non-zero eccentricity. Finally, HR 8589 is a 6^m G8 III star in an orbit that, despite having a period of about 11½ years, seems to be exactly circular.

Introduction

The stars treated in this paper are a miscellaneous lot of seemingly single-lined binaries, but are none the less interesting for that. HD 48913 was discovered, repeatedly but independently and with increasing certainty, to be a binary system, first by resolution at a lunar occultation, then by its anomalous Strömgren indices, and finally by actual spectral classification. It seems to be a composite-spectrum system whose primary is a late-type giant. HR 6853 and HR 8589 were both discovered to be spectroscopic binaries by de Medeiros & Mayor[1], who in fact had enough measurements of the former to have supported an orbit determination of sorts, but they were very badly distributed in phase. The binary nature of HD 206843 was suggested by Suchkov, and although the suggestion has proved to be correct the actual basis for it — excess luminosity — has not, so the fact that the star has proved to be binary may be just a coincidence.

HD 48913

Appropriately enough for a binary system, HD 48913 is to be found in the middle of the constellation Gemini, indeed within the oblong shape that is delineated by most of the principal stars. It is about 1½° south-following ε Gem. It seems never to have been observed photometrically in the usual way and in the usual bands, and to obtain its broad-band magnitudes we are obliged to fall back on those transformed from *Hipparcos* in the *Tycho 2* listing[2], $V = 7^{m}\cdot 79$, $(B - V) = 0^{m}\cdot 60$.

The literature on the system is remarkably sparse, but each of the first three papers reported by *Simbad* describes an independent discovery of the duplicity of the object. First, Eitter & Beavers[3] resolved it at a lunar occultation in 1972, which they observed in both blue and red wavebands. Their Table 1 notes it as "possibly double", but the ensuing material makes it clear that they regarded its resolution as pretty certain. The angular separation, inevitably in projection onto the coördinate parallel to the Moon's advance, was only $0''\cdot 034 \pm 0''\cdot 007$. There is a note (introduced by the star's *BD* number, +23° 1494), saying, "Both $\mathcal{B}$ and $\mathcal{R}$ records imply that this is a probable double with very small projected separation. The primary appears to be redder than the secondary." A table shows the magnitude differences, in both colours, between the individual components and the un-occulted system. The $\delta\mathcal{R}$ values are $0^{m}\cdot 74 \pm 0^{m}\cdot 34$ for the primary and $0^{m}\cdot 81 \pm 0^{m}\cdot 34$ for the secondary; in $\mathcal{B}$ the corresponding values are $0^{m}\cdot 50 \pm 0^{m}\cdot 54$ and $1^{m}\cdot 25 \pm 0^{m}\cdot 77$. The paper gives no hard information as to the actual wavelengths of the two photometric bands, but a chase back through the literature retrieves a diagram (ref. 4, Fig. 1) which suggests effective wavelengths near 7200 Å for $\mathcal{R}$ and 4500 Å for $\mathcal{B}$. The uncertainties of the measured quantities are obviously such as to dwarf any that arise from doubts as to the exact wavelengths. Although the error bars are very large, the δm values tend to give the impression that the two stars are of comparable brightness in the $\mathcal{R}$ band but may differ by half a magnitude or more in $\mathcal{B}$.

Next, Olsen, who undertook a large programme of Strömgren-photometry measurements, noticed several varieties of idiosyncratic indices, inasmuch as certain relatively small groups of stars did not conform to the usual run of relationships between the different indices that were formed from the photometry. In many cases the photometric idiosyncrasies might draw attention to interesting characteristics of the stars concerned. Olsen noted a dozen different types of anomalous photometric indices, and listed[5] the stars in their respective groups, with possible interpretations in terms of spectral types. The indices of many of the members of one of the largest of his groups (group 9, 95 stars; the caption of the table actually says 96, but a table *of* the tables gives the number correctly as 95), suggested to Olsen that the spectra were composite, although other interpretations were often possible. Certainly the table includes a number of well-known composite-spectrum objects. The most probable interpretation of the indices of HD 48913 is that it is composite, but a less probable alternative of "g/cF8" is also listed, where 'g/c' means 'giant/supergiant'.

Then, in 1983 Bidelman[6], industriously scouring for interesting objects the objective-prism plates newly taken with a 10° prism on the *Burrell Schmidt*[7] after its move to Kitt Peak, identified HD 48913 definitely as having a composite spectrum, with the types gK + A0.

In the original *Hipparcos* catalogue[8] the parallax of HD 48913 was recorded as negative, though not by as much as its standard deviation. In the revision[9] of the parallaxes, the value is still very small but is on the right side of zero, at $0\cdot 47 \pm 0\cdot 88$ milliseconds of arc. The central value corresponds to a distance modulus of $11^{m}\cdot 6$, which would make the absolute magnitude nearly −4; the $\pm 1\sigma$ range runs from a distance modulus of $9^{m}\cdot 3$ to 'beyond infinity'. The former value might be regarded as plausible, corresponding to $M_V \sim -1^{m}\cdot 5$, and in the light of Bidelman's classifications suggesting — in very rough approximation, since better precision is clearly unwarranted — absolute magnitudes near –1 for the gK component and zero for the A0.

The composite nature of the spectrum means that efforts to deduce stellar or interstellar parameters[10–13] just by computer interpretation of photometric numbers are not likely to yield realistic answers. In one case[10], however, where such interpretation is not attempted, it is noted that there are 20 radial velocities available for HD 48913, spanning an interval of no less than 6165 days and showing that the star is a spectroscopic binary. Those velocities must be, in large part, my own. After placing the star on the Cambridge observing programme in a slightly belated reaction to Bidelman's 1983 paper[6], I obtained 18 measurements on a guest-observer basis with the OHP *Coravel* and three with the similar instrument at ESO. Those 21 observations are in the data base that is kept in Geneva of *Coravel* radial velocities and that was the source of the radial-velocity data in ref. 10. I expressly permitted the authors of ref. 10 to include my observations in the enumerations that they published of the radial velocities available for the various stars. That does not explain, however, why the interval covered is noted as 6165 days whereas my contribution spans 'only' 3827 days (suggesting that there exist also measures taken by others), or why the total number of measurements is given as 20 when my own contribution alone amounts to 21. But those discrepancies cannot be resolved here, and are in any case only obliquely germane to the present interest — the solution of the orbit.

In addition to the velocities just mentioned, obtained at OHP and ESO, there are four measured with the original spectrometer at Cambridge, two from the DAO, and 34 obtained with the Cambridge *Coravel* in 1996–2013. All 61 of

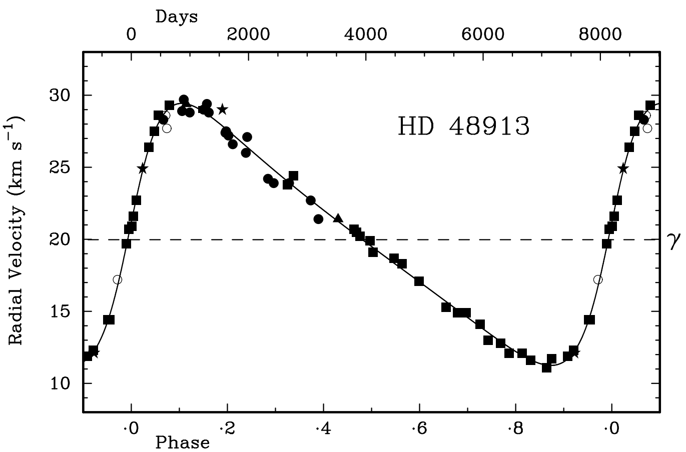

FIG. 1

The observed radial velocities of HD 48913 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The filled squares, circles, and stars represent measurements made with the *Coravel* spectrometers at Cambridge, OHP, and ESO, respectively. The OHP and ESO velocities were given half-weight in the solution of the orbit. Open circles plot observations made with the original Cambridge spectrometer; they were weighted ¼. The triangles represent DAO observations, weighted ½.

TABLE I

Radial-velocity observations of HD 48913

Except as noted, the sources of the observations are as follows:
1991–1998 — OHP Coravel (weight ½); 1999–2013 — Cambridge Coravel (weight 1)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O–C) km s⁻¹*
1987 Nov. 9·38*	47108·38	+12·1	0·923	−0·2
1988 Dec. 13·05†	47508·05	17·2	0·971	+0·5
1990 Feb. 14·19*	47936·19	24·9	1·024	+0·2
1991 Feb. 3·06	48290·06	28·3	1·067	−0·3
Mar. 13·86†	328·86	28·6	·071	−0·2
Apr. 3·88†	349·88	27·7	·074	−1·2
Dec. 19·06	609·06	28·9	·106	−0·5
1992 Jan. 16·98	48637·98	29·7	1·109	+0·3
Feb. 28·27‡	680·27	29·4	·114	0·0
Apr. 27·83	739·83	28·8	·122	−0·5
Nov. 18·13§	944·13	29·0::	·147	+0·1
Dec. 19·07	975·07	29·0	·150	+0·1
1993 Feb. 15·03	49033·03	29·4	1·157	+0·7
Mar. 18·91	064·91	28·8	·161	+0·2
Nov. 5·35*	296·35	29·0	·190	+1·1
Dec. 27·10	348·10	27·4	·196	−0·3
1994 Jan. 8·08	49360·08	27·5	1·197	−0·2
Feb. 17·94	400·94	27·2	·202	−0·3
May 1·84	473·84	26·6	·211	−0·7
Dec. 11·14	697·14	26·0	·238	−0·5
1995 Jan· 2·05	49719·05	27·1	1·241	+0·7
Dec· 24·09	50075·09	24·2	·285	−1·0
1996 Mar. 29·92	50171·92	23·9	1·296	−0·9
Nov. 21·14§	408·14	23·8	·325	−0·2
Dec. 16·10	433·10	23·9	·328	0·0
1997 Mar. 2·95§	50509·95	24·4	1·338	+0·7
Dec. 21·11	803·11	22·7	·373	0·0
1998 May 1·82	50934·82	21·4	1·389	−0·9
1999 Apr. 2·24‡	51270·24	21·4	1·430	+0·2
Dec. 29·10	541·10	20·7	·463	+0·3
2000 Feb. 12·02	51586·02	20·5	1·469	+0·2
Apr. 6·91	640·91	20·2	·476	+0·1
Sept. 25·20	812·20	19·9	·497	+0·3
Nov. 17·16	865·16	19·1	·503	−0·3
2001 Nov. 14·21	52227·21	18·7	1·547	+0·4
2002 Mar. 29·90	52362·90	18·3	1·564	+0·4
2003 Jan. 11·11	52650·11	17·1	1·599	0·0
2004 Apr. 19·86	53114·86	15·3	1·655	−0·4
Oct. 26·20	304·20	+14·9	·679	−0·2

TABLE I (*concluded*)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O – C) km s⁻¹*
2005 Mar. 25·93	53454·93	+14·9	1·697	+0·3
Nov. 14·14	688·14	14·1	·725	+0·2
2006 Apr. 4·88	53829·88	13·0	1·743	−0·5
Nov. 3·18	54042·18	12·8	·769	−0·1
2007 Mar. 27·92	54186·92	12·1	1·786	−0·4
Nov. 3·24	407·24	12·1	·813	+0·2
2008 Mar. 31·88	54556·88	11·6	1·831	0·0
Dec. 27·10	827·10	11·1	·864	−0·2
2009 Mar. 23·88	54913·88	11·7	1·875	+0·5
Dec. 21·11	55186·11	11·9	·908	+0·2
2010 Apr. 5·89	55291·89	12·3	1·921	+0·1
Dec. 12·16	542·16	14·4	·952	0·0
2011 Jan. 10·02	55571·02	14·4	1·955	−0·4
Oct. 16·21	850·21	19·7	·989	+0·2
Nov. 28·13	893·13	20·7	·995	+0·4
2012 Jan. 17·05	55943·05	20·9	2·001	−0·4
Feb. 18·90	975·90	21·6	·005	−0·3
Apr. 10·86	56027·86	22·7	·011	−0·2
Nov. 6·21	237·21	26·4	·036	+0·1
2013 Feb. 6·99	56329·99	27·5	2·048	+0·1
Apr. 16·83	398·83	28·6	·056	+0·6
Oct. 24·21	589·21	+29·3	·079	+0·2

*Observed with ESO *Coravel*; weight ½.
† Observed with original spectrometer; wt. ¼.
‡ Observed with DAO spectrometer; weight ½.
§ Observed with Cambridge *Coravel*; weight 1 (double colon: weight ¼.)

them are set out in Table I, and readily yield the orbit whose elements are given, together with those of the other stars treated here, in Table V towards the end of this paper. The orbit is illustrated in Fig. 1. The period of about 22½ years is determined with an uncertainty of only 23 days.

We might consider the components' angular separation on the sky. The $a \sin i$ value for the primary is somewhat over 900 Gm or 6 AU; if the secondary is supposed to be a bit less massive (since it is still an A star and has not evolved, as the primary *has*), we might take the mean separation of the stars as (say) 13/sin i AU. The parallax of 0″·00135 that we saw fit to adopt above, being 10σ larger than the central value, corresponds to a distance of something over 700 pc, at which 13 AU would subtend a little under 0″·02. At the phase (about ·22 according to the orbit derived here) when the pair was resolved at a lunar occultation[3], the separation would have been appreciably greater than the mean, about 0″·025/sin i. That is just about compatible with the value actually observed at the occultation, 0″·034 ± 0″·007, since we can afford to postulate a small increase above 0″·025 in the expected separation by assuming a value for sin i that is modestly less than 1. After that, the deduced and observed values would agree if each were supposed to be 'off' by about 1σ (as we have already had reason to suggest in respect of the parallax), *and* the orientation of the binary at the time of the occultation is supposed to be practically parallel to the direction of the Moon's advance — which is all a bit uncomfortable but does not seem so implausible as to bring down this whole discussion.

The mass function is unusually large, but that is to be expected in a case where the secondary is known to be an early-type star. If we took the mass of the primary to be 2 $M_\odot$, the secondary would need to be almost as massive (about 1·92 $M_\odot$) in order to honour the mass function even if the factor $\sin^3 i$ in that function were taken as unity. If $\sin i$ were to be taken as, say, 0·95, and the primary still supposed to be somewhat more massive than the secondary as is suggested by its more evolved state, then the masses would need to be about 2·6 and 2·4 $M_\odot$. For $\sin i$ to be as 'low' as 0·9, the two stars would need to have masses as large as about 3·0 and 2·7 $M_\odot$. The numbers given in this paragraph seem likely to bracket the true values, so the primary star can be expected to be within the range 2–3 $M_\odot$, the secondary somewhat (perhaps 10%) less, and $\sin i$ not less than 0·9 ($i \gtrsim 65°$).

The late-type star that is measured with the *Coravel*s exhibits appreciable line-broadening, presumably of rotational origin. The mean $v \sin i$ values are 5·4 ± 0·6 km s^{-1} from the OHP instrument and 4·7 ± 0·4 from the Cambridge one, so a round value of 5 km s^{-1} may be adopted.

HR 6853 (HD 168322)

HR 6853 (unfortunately listed by *Simbad* under the arcane main heading "NLTT 46245"*) is a 6^m star on the western border of Lyra, about 4° preceding and 2° north of Vega. It has been the subject of a rich literature (84 papers retrieved by *Simbad*), evidently because it was recognized early on to be a member of the 'high-velocity' stellar population, often known as Population II. Its radial velocity alone, determined[16] in 1926 to be below –70 km s^{-1}, is enough to qualify it for that category; it has in addition a proper motion of nearly 0″·2 per annum, which was known more than 100 years ago when the star was listed (under the identity Groombridge[17] 2538) as no. 4624 in Boss's *Preliminary General Catalogue*[18] (the fore-runner of the *Bright Star Catalogue*). That such a motion represented a substantial transverse velocity became apparent when the star was recognized as a giant, as it was in 1935, in a major Mount Wilson paper[19] giving spectroscopic parallaxes. It was there attributed an absolute magnitude of $+1^m\cdot1$ — not far from the modern *Hipparcos* value[9], which is close to $+0^m\cdot7$ with an uncertainty less than $0^m\cdot1$. In fact, the old Mount Wilson estimate has proved to be nearer the truth than the $+1^m\cdot5$ found by the usually reliable method that depends on the width of the *K*-line emission reversals, the brainchild of O. C. Wilson[20].

Thus the scene was set, when realization dawned of the existence of stellar populations of different ages and of the progressive enrichment with heavy elements of the star-forming medium in the Galaxy, for HR 6853 to take its place immediately in what is now sometimes called the 'old disc population'.

*It is not at all obvious where *Simbad* obtained the number 46245, which in any case must be seen as a reprehensible infringement of Luyten's explicit intention to *avoid* creating yet another new identification number for the star. "As far as designations are concerned, there has been such a plethora of completely unnecessary and utterly meaningless, as well as highly confusing, new systems used (mainly for the greater ego of the recent observers) that I decided NOT to assign a running serial number. I have used BD and CoD whenever possible . . ." (ref. 14, vol. **1**, p. 3). Luyten does in fact identify the star by its BD[15] number, +40° 3332.

Keenan & Keller, in a paper on *Spectral classification of the high-velocity stars*[21], were the first to classify it on the MK system[22], as G8 III; they added the comment, "CN weak! Atomic lines slightly weak? CH slightly strong", and they gave its absolute magnitude as $+0^{m}\cdot5$. Soon afterwards, Miss Roman, in her *Catalogue of high-velocity stars*[23], gave its type as K0 III and its photometry as $V = 6^{m}\cdot10$, $(B-V) = 1^{m}\cdot00$, $(U-B) = 0^{m}\cdot70$ (subsequent authors have been in tolerable agreement), and put its space motion, relative to the 'local standard of rest', at 131 km s^{-1}. Greenstein & Keenan[24], who based their investigation of the abundances of metals and of CN and CH in giant stars on Mount Wilson 4·5-Å mm^{-1} coudé spectra, gave a classification of G9 III^{-} with the comment, "CN weak, CH slightly strong?", and gave quantities that the present writer understands to be intended to be close to logarithmic abundance figures as $-0{\cdot}75 \pm 0{\cdot}10$ for CN, $-0{\cdot}08 \pm 0{\cdot}04$ for atomic lines, and $+0{\cdot}15 \pm 0{\cdot}06$ for CH. Photoelectric measurements, published[25] more than 50 years ago, of the strength of the λ4200-Å CN band in the spectra of late-type stars showed HR 6853 to have the third-weakest CN among 79 stars considered to be of type K0 III, even weaker than that in HD 191046, a binary system for which the writer[26] has recently published an orbit with a γ-velocity of about −93 km s^{-1}. The two stars that have CN bands that are (marginally) weaker than in HR 6853 are themselves both high-velocity stars, HD 2925 and 38 Aur. It is of interest that the weakening of the metallic lines, caused by the low metallic abundances in HR 6853, misled the pre-MK Mount Wilson authors[19] into classifying the spectrum considerably too early, at G4. That type could be seen as very close to the first one ever given for HR 6853: the star appears in the *Draper Catalogue*[27] of 1890 as D.C. 8062, type H — a minor fraction of the way from G to K.

Most authors have considered the metal deficiency of HR 6853 to be substantially larger than the amount indicated by Greenstein & Keenan[24], mentioned above. As far as the atomic ('metallic') abundances are concerned, Cottrell & Sneden[28] found a mean logarithmic deficiency of 0·40 with respect to the Sun; in the same investigation they derived the star's mass to be only 0·26 $M_{\odot}$. (Much more recently, Luck & Heiter[29] have listed the mass as 2·01 $M_{\odot}$, a dramatic discrepancy upon which the present author has no means of adjudicating.) Photometrically (as opposed to spectroscopically) estimated values of [Fe/H] by different authors[30–35] have reached somewhat accordant conclusions, all attributing to the star metal deficiencies in the range 0·3–0·6 dex.

Keenan, in his last period in which he repeatedly made small adjustments to spectral classifications (though his perspicacity is freely acknowledged here), made a number of successive changes to the type of HR 6853. The star does not feature in his first listing[36] of revised types in 1980, but is listed as 'G8·5 IIIb CN-1 Fe-0·5 CH0·5' in a 1983 paper[37]. There is circumstantial evidence that he must have sent some such type to the compiler of the *Bright Star Catalogue* just in time for it to be incorporated, at least in principle, in the fourth edition[38] of that *Catalogue* in 1982. The problem evidently arose that it overflowed the space available there for printing the type. That occurred in a number of cases of Keenan's new classifications, and a scheme was adopted for coping with the problem. The *Introduction* to the *Catalogue* says, "In a few instances the revised classes take up more spaces than are available in the spectral class column. These have been abbreviated and marked with an asterisk in the spectral class column. The complete classes are given in the SUPPLEMENTARY REMARKS." Under the heading REMARKS in the *Introduction*, we are told, "An asterisk in the last column is an alert to the REMARKS following the Catalogue proper." Not only is the type of HR 6853 shown in the body of the *Catalogue* as

G9III*, but there is an asterisk in the last column too. The *Catalogue* has indeed a section headed REMARKS, which has many pages of additional data that would not fit into the main body of the listing, but HR 6853 has no entry there. It takes a diligent search to locate, right at the end of the *Catalogue*, after three *Appendices*, a single page (p. 472) of supplementary remarks and corrections, which *does* feature HR 6853, and gives its type as 'G8·5IIIb CN-1 Fe-0·5 CH 1'.

Further Keenan re-classifications followed. In 1985[39] it seemed that the (previously substantial) CN deficiency was no longer individually noteworthy but the general under-abundance was regarded as more conspicuous, because the type changed to G8·5 IIIb Fe-1 CH 0·5. It is no more than fair to Keenan, who is no longer in a position personally to defend his position on that issue, to relate what happened in a very comparable instance some twenty years ago.

The present writer (lapsing into 'I' to avoid repeated circumlocutions in what follows), wrote a paper presenting the orbit of the third-magnitude star ζ Cyg, which Keenan recognized as a 'mild barium star'. In the blizzard of re-classification papers in the 1980s, Keenan first added 'CN 1' to the basic spectral type, but later he omitted it, in just the same way as happened in the case of immediate present interest. By the time I had finished the paper, which I felt ought to describe (however impassively) all the successive changes to the type, I feared that it might read as a rather comprehensive indictment of Keenan. I knew him to be a nice chap, who certainly had a flair for divining interesting things from spectra of very small scales, and I did not wish to offend him without at least giving him a chance to have a say on the matter. So instead of sending the paper to the Editors of this *Magazine*, I posted it, just as it stood, to Keenan instead, inviting him to explain everything and to join me as co-author. To his credit, he was not at all offended, and offered a perfectly plausible and obviously authoritative explanation of what I had regarded as apparent changes of mind, and he took up the offer of co-authorship. What he said — I quote here *verbatim* from the published paper[40] — was, "The CN 1 index was later dropped for ζ Cyg because the apparent strengthening of the CN band that would be anomalous in a star of solar composition had not been shown to be abnormal in a marginal barium star." Following an analogous line of thought, one can see that Keenan, upon re-consideration of the spectrum of HR 6853, recognized that the substantial weakness of CN, which 'would be anomalous in a star of solar composition', was only to be expected in a star with the metal-deficiency of HR 6853. It is only because the ordinary reader is not sufficiently *au fait* with Keenan's thought-processes, as they gradually developed, that successive refinements are so easily seen as extraordinary changes of mind, as they evidently were by, *e.g.*, Eggen & Iben[41]. The particular discrepancies that those authors understandably found so remarkable are fully explained on p. 173 in ref. 40; if such explanations had been included in the introductory text to the *Perkins Catalogue*[42] a good deal of misunderstanding and even cynicism might have been avoided.

In that *Perkins Catalogue* of 1989 the same type as had appeared[38] for HR 6853 in 1985 had merely rearranged itself to G8·5 IIIb CH0·5 Fe-1. Rather pathetically, Keenan's final catalogue[43] of spectral types stops short at a right ascension a bit before that of HR 6853, when its senior author, who was of a great age, expired.

Eggen[44] included HR 6853 (whose HD number he misprinted in one place as 1688322) in a paper on the 'Arcturus group', but in it he assigned it instead to his 'η Cephei group'; it does not, however, reappear in any of his subsequent papers on moving groups.

It has been noted at the beginning of this section that the radial velocity of HR 6853 was first determined at Mount Wilson[16] the best part of a century ago. The three velocities that were then published as a mean were long afterwards provided individually by Abt[45] and are reproduced at the head of Table II. It seems that no further velocity measurements were published until de Medeiros & Mayor[1] included the star in a large summary of *Coravel* velocities in 1999, in which they identified the star as a spectroscopic binary and recorded that they had made 22 measurements of it over a total interval of 5542 days. (Although that is somewhat longer than the orbital period, we can see now why they did not publish an orbit: there was a phase gap of more than half the orbital period after the first three measurements.) The 22 velocities were later made accessible through the CDS, and have been transcribed here into Table II, where they follow the three old Mount Wilson measures. Although the data base from which they were taken includes measurements from the ESO *Coravel* at La Silla as well as ones from Haute-Provence, in view of the declination of HR 6853 (+41°) it seems most unlikely that the 22 velocities include any from ESO.

Exactly the same data as appear in the tabulation by de Medeiros & Mayor were repeated in another paper[46] (in a different journal) by de Medeiros, da Silva & Mayor, except for the curious omission of the actual mean velocity. Whereas the first paper[45] is entitled *A catalog* [*sic*] *of rotational and radial velocities for evolved stars*, the second is *The rotation of binary systems with evolved components*, so maybe it could be claimed not to be interested in *radial* velocities even though *they* are what it is all about — how else would its entries be known to be binary systems?! Both of them give the rotational velocity of HR 6853 as 1·8 km s^{-1}, but only the earlier paper gives also its uncertainty, of 1 km s^{-1}. The only other rotational velocity noticed in the literature is given by Prugniel, Vauglin & Koleva, in a paper[47] of which *A&A* deigned to print only the summary; they give the "instrumental and physical broadening" of the spectrum as 57 km s^{-1} — which seems excessive, and must be largely of 'instrumental' origin. The spectra discussed by those authors started as *Elodie*[48] ones, with a resolution equivalent to about 7 km s^{-1}, and we know (see below) that the actual rotation of the star contributes almost nothing to the line-broadening; it is obvious that almost the whole of the 57 km s^{-1} of "instrumental and physical broadening" must arise from blurring deliberately performed by the authors of the paper, who seem thereby to have reduced worthwhile information that existed in their input data to uselessness.

HR 6853 was placed on the Cambridge radial-velocity observing programme in late 2004 and its observations have been scheduled at approximately two-month intervals during the annual season when the star is most readily accessible to the 36-inch telescope. There are 42 of them, listed in Table II; they do not quite cover a complete orbital cycle, but that shortcoming is made good by the OHP measures, whose phases overlap on both sides of the gap in the Cambridge observations. As is usual in this series of papers, the OHP velocities have been increased by 0·8 km s^{-1} from the published values; the Cambridge ones have been adjusted by –0·3 km s^{-1} from the 'as initially reduced' quantities, an amount not out of line with colour-dependent corrections that have empirically been found desirable in previous instances. The velocities from the three *Coravel*s have all been weighted equally in the solution of the orbit, which is illustrated in Fig. 2; the orbital elements are included in Table V, with those of the other binary systems treated here, towards the end of the paper. The Mount Wilson observations[16,45] were not used in the solution.

TABLE II

Radial-velocity observations of HR 6853

Except as noted, the sources of the observations are as follows:
1978–1993 — OHP Coravel[1]; 2004–2013 — Cambridge Coravel (both weight 1)

Date (UT)	MJD	Velocity km s^{-1}	Phase	(O–C) km s^{-1}
1926 July 3·37*	24699·37	−71·8	0·676	−1·9
Sept. 26·14*	784·14	−75·2	·694	−5·0
Nov. 15·11*	834·11	−71·7	·705	−1·3
1978 June 27·04	43686·04	−71·8	4·783	0·0
July 10·02	699·02	−71·8	·786	+0·1
1979 June 16·02	44040·02	−73·6	4·860	+0·2
1986 Aug. 6·91	46648·91	−68·1	5·424	0·0
1987 June 13·96	46959·96	−69·0	5·491	−0·7
1988 Oct. 14·84	47448·84	−69·1	5·597	−0·1
23·80	457·80	−69·1	·599	−0·1
1989 June 2·07	47679·07	−69·6	5·647	0·0
July 19·04	726·04	−69·8	·657	−0·1
Nov. 25·76	855·76	−69·8	·685	+0·3
1990 Aug. 13·87	48116·87	−70·6	5·742	+0·4
Dec. 3·70	228·70	−71·6	·766	−0·1
1991 Mar. 19·19	48334·19	−71·9	5·789	+0·1
July 19·99	456·99	−72·6	·815	0·0
Nov. 10·81	570·81	−73·5	·840	−0·2
1992 Apr. 13·17	48725·17	−74·4	5·873	−0·2
May 9·11	751·11	−74·5	·879	−0·1
Aug. 19·95	853·95	−75·2	·901	−0·2
1993 Apr. 9·12	49086·12	−76·2	5·951	−0·1
July 16·92	184·92	−76·5	·973	−0·2
29·93	197·93	−76·3	·975	0·0
Aug. 28·93	227·93	−76·0	·982	+0·2
2004 Dec. 21·72	53360·72	−74·0	6·876	+0·3
2005 June 1·06	53522·06	−75·3	6·911	0·0
Aug. 21·98	603·98	−75·6	·929	+0·1
Oct. 27·78	670·78	−76·2	·943	−0·2
Dec. 11·70	715·70	−76·4	·953	−0·2
2006 Apr. 9·11	53834·11	−76·3	6·978	−0·1
June 1·08	887·08	−76·1	·990	0·0
Aug. 7·97	954·97	−75·4	7·004	+0·5
Sept. 7·98	985·98	−75·6	·011	+0·1
Oct. 24·82	54032·82	−75·4	·021	0·0
Nov. 25·74	064·74	−74·8	·028	+0·4
2007 Jan. 14·30	54114·30	−74·6	7·039	+0·2
Feb. 15·27	146·27	−74·8	·046	−0·3
Apr. 2·19	192·19	−74·3	·056	−0·2
May 1·12	221·12	−74·1	·062	−0·3
July 7·06	288·06	−73·2	·077	0·0

TABLE II (*concluded*)

Date (UT)	MJD	Velocity km s^{-1}	Phase	(O–C) km s^{-1}
2007 Aug. 31·02	54343·02	−72·6	7·088	+0·1
Oct. 23·79	396·79	−72·1	·100	+0·1
Dec. 15·72	449·72	−71·8	·112	0·0
2008 Feb. 16·25	54512·25	−71·5	7·125	−0·2
Apr. 24·17	580·17	−71·0	·140	−0·2
July 21·99	668·99	−70·0	·159	+0·3
Sept. 18·88	727·88	−70·1	·172	−0·1
Nov. 18·70	788·70	−69·7	·185	0·0
Dec. 11·70	811·70	−69·5	·190	+0·1
2009 May 20·12	54971·12	−68·7	7·224	+0·3
July 12·98	55024·98	−68·7	·236	+0·1
Sept. 9·92	083·92	−68·7	·249	0·0
2010 Apr. 17·18	55303·18	−68·3	7·296	0·0
June 5·08	352·08	−68·0	·307	+0·2
Aug. 5·99	413·99	−67·9	·320	+0·3
Oct. 27·80	496·80	−68·6	·338	−0·5
2011 May 15·12	55696·12	−68·0	7·381	0·0
Aug. 9·97	782·97	−68·0	·400	0·0
Oct. 15·87	849·87	−67·9	·414	+0·1
2012 May 16·11	56063·11	−67·9	7·461	+0·3
Aug. 4·94	143·94	−68·0	·478	+0·2
Nov. 5·83	236·83	−68·7	·498	−0·4
2013 Apr. 2·20	56384·20	−69·0	7·530	−0·5
June 3·11	446·11	−68·1	·543	+0·5
Aug. 25·90	529·90	−68·7	·561	0·0
Oct. 16·82	581·82	−68·7	·573	+0·1

*Published Mt. Wilson observation[16,45]; wt. 0.

Ordinarily one could interpret the mass function as a means of setting an approximate lower limit to the mass of the unseen companion star. To do that one has to make an estimate of the mass of the primary — the writer usually adopts 2 $M_\odot$ as the putative mass when the primary is a giant. Here, we have conflicting assessments of the primary's mass as 0·26[28] and 2·01[29] $M_\odot$. They would demand minimum values of about 0·17 and 0·57 $M_\odot$, respectively, for the mass of the secondary, corresponding to the masses of main-sequence stars of late M and very late K types, respectively. Neither of those could be expected to be visible in the spectrum in competition with the giant primary. If for once we appeal to thought rather than to observation, we might think that HR 6853 with its three- or four-fold metal deficiency must be quite old even by the standards of stellar longevity and so must have lasted for a long time on the main sequence before becoming a giant — a time comparable with the main-sequence lifetime of a solar-type star. So we might guess a primary mass of 1 $M_\odot$. Then the secondary would need a minimum mass of about 0·38 $M_\odot$ to fulfil the mass function, so it could be as late as M2, with an absolute magnitude of about 10, nine magnitudes fainter than the primary and *not* a good prospect for direct resolution on the sky. If we were keen to find support for such a conclusion, we might notice that Prugniel *et al.*[47] derived a log *g* of

2·39 for HR 6853, 0·36 (a factor a little more than 2) less than the 2·75 from which Luck & Heitter[26] obtained the mass as 2·01 $M_{\odot}$. It could be admitted, however, that to pick and choose between a number of wildly different options and settle on one that suits one's purpose does smack of prejudice!

The Cambridge observations indicate a small rotational velocity for HR 6853; the mean is 2·9 km s^{-1}, with an r.m.s spread of about 1·4 km s^{-1} per observation, yielding formally a standard error of little more than 0·2 km s^{-1} for the mean. The rather simplistic treatment of the line width as being due just to a fixed intrinsic width (the minimum given by other stars), modified only by a rotational velocity, makes it unsafe to claim an external uncertainty less than 1 km s^{-1}.

HD 206843

HD 206843 is an 8^{m} star in the more easterly of the two north-following corners of Cygnus, within minutes of the boundary with Cepheus. It is about 3½° almost due south of μ Cep, Herschel[49]'s famous 'Garnet Star'*. Once more we are obliged to fall back on *Tycho 2*[2] for the photometry, $V = 8^{m}{\cdot}38$, $(B - V) = 0^{m}{\cdot}50$. The parallax[9] yields a distance modulus of $3^{m}{\cdot}60 \pm 0^{m}{\cdot}09$, so the absolute magnitude is close to $4^{m}{\cdot}8$, with an uncertainty only of about $0^{m}{\cdot}1$.

The star was one of a number drawn to the writer's attention privately by Suchkov after the conclusion of a considerable investigation[51] of the natures of stars that he identified as potential binary systems by the 'ΔM_{c_0}' criterion[52] of 'over-luminosity', whereby the actual luminosity of a star, found from its parallax, significantly exceeds that which would be predicted from its Strömgren indices. (Clearly, a system consisting of two mutually similar stars will be twice as bright as (three-quarters of a magnitude brighter than) one of them alone, although the photometric indices will be exactly the same.) The indices of the 'new' batch of Suchkov stars suggested that they were metal-poor as well as candidate binaries.

HD 206843 was already known to have a large proper motion; indeed, the main heading in the *Simbad* bibliography is "G 232-31 — High proper-motion star", where the first part refers to the 'Giclas' identity of the star in the *Lowell Observatory Bulletins*[53] and the descriptive part does not mean what must be intended unless the reader supplies from his imagination the missing hyphen. Although the Lowell papers do identify a lot of objects with large proper motions, in the relevant case they were not reporting the *discovery* of

*There has been some — in my view unwarranted — doubt as to which star was intended to receive the designation μ. Notes ('Remarks') in the back of the *Bright Star Catalogue*[38] refer to the designation having been applied variously to 13 and 14 Cep as well as to the star that, being much the brightest of the three, is the obvious candidate for the honour. 13 and 14 Cep are $5^{m}{\cdot}6$ and $5^{m}{\cdot}8$, respectively, while the star that we know as μ, although it has no Flamsteed number, is much brighter; it varies in a 'semi-regular' manner (from about $3^{m}{\cdot}6$ to $4^{m}{\cdot}2$ during the *Hipparcos* era). In an effort to throw light on this matter even though it is only obliquely related to spectroscopic binaries, through the courtesy of the Cambridge librarian, Mr. M. Hurn, he and I have examined the relevant page of the Bayer *Atlas*[50] where the Greek-letter designations were originally assigned. Although Bayer's cartography leaves a lot to be desired, we were left in no doubt that the star marked μ by Bayer is indeed the one that we know by that designation. The one remaining puzzle is why Herschel[49] did not refer to it in that way, instead of laboriously describing its position with respect to 10 Cep. From ref. 49, p. 257: "A very confiderable ftar, not marked by FLAMSTEAD, will be found near the head of Cepheus. Its right afcenfion in time, is about 2′ 19″ preceding FLAMSTEAD's 10th Cephei, and it is about 2° 20′ 3″ more fouth than the fame ftar. It is of a very fine deep garnet colour, fuch as the periodical ftar o Ceti was formerly, and a moft beautiful object, efpecially if we look for fome time at a white ftar before we turn the telefcope to it, fuch as α Cephei, which is near at hand."

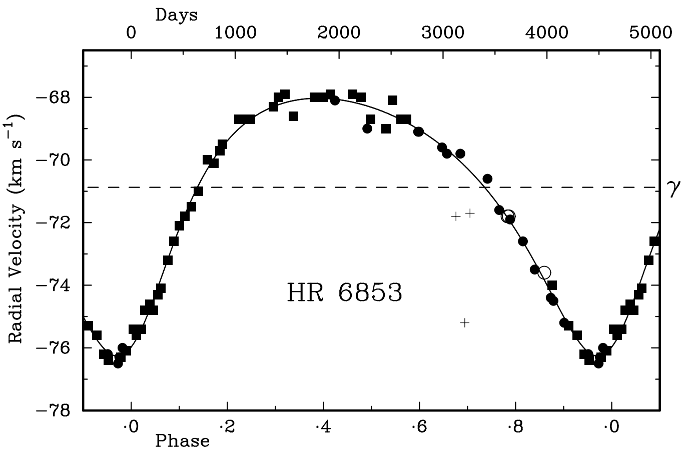

FIG. 2

As Fig. 1, but for HR 6853. Here the OHP observations were not made by the writer but were retrieved from the *Centre de Données Stellaires*, where they were deposited by de Medeiros & Mayor[1]. They were given the same weight as the Cambridge ones in the solution of the orbit. The three open circles (two of them are almost coincident) are from the same source but date from the preceding cycle and so help to refine the determination of the orbital period. The three plusses represent the photographic observations made[16,45] at Mount Wilson in the 1920s; they were not included in the orbit solution.

that property, since HD 206843 had already been listed by Luyten in his '*LTT*' ('*Luyten Two-Tenths*')* catalogue[54], as LTT 16350. (That was before he was so concerned to avoid proliferation of designations as he became by the time of his 'new' (*NLTT*) catalogue[14] (see footnote on p. 62). The proper motion and distance found by *Hipparcos*[9] combine to indicate a transverse velocity of about 76 km s^{-1}, quite high but not at all remarkable for a metal-weak object.

The absolute magnitude of HD 206843 is practically identical to that of the Sun, but the colour index is significantly ($0^{m}{\cdot}16$) bluer. It is hard to attribute the blueness to a hot companion, since if the primary star is near solar type the secondary would be expected to be lower down the main sequence and so ought to be redder, unless indeed it were a white dwarf, in which case it should be so relatively faint as not to make much difference to the colour index. The most reasonable way to understand the rather blue colour index is merely in terms of reduced blanketing in the blue part of the spectrum (in comparison with the Sun) caused by the metal-weakness of the star. Photometric indices in the Strömgren[55] and Vilnius[56] systems have led to rather odd suggestions for the spectral type being 'sdF9'[5] and 'sdF8'[57], respectively — trying at least to indicate that the star appears to be somewhat hotter than the Sun but not as luminous as its somewhat earlier type might suggest; a photometric estimate[10] of its actual metal abundance indicated its [Fe/H] to be seriously deficient, at –0·99. The weakness of the absorption lines is reflected in that of the 'dips' measured in radial-velocity traces.

The only radial velocities that are known to have been obtained previously for HD 206843 are four that were reported in 2004 just as a mean, of –5·3 ± 3·7

* [of a second of arc annual proper motion]

TABLE III

Radial-velocity observations of HD 206843

All the observations were made with the Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)* *km s⁻¹*
2003 July 16·09	52836·09	−7·5	0·680	−0·7
Aug. 16·06	867·06	+10·2	3·988	−0·3
20·07	871·07	−18·8	4·416	−0·3
31·04	882·04	−13·9	5·588	−0·3
Sept. 10·99	892·99	+0·1	6·758	+0·1
14·01	896·01	+5·1	7·080	+0·3
14·98	896·98	−5·3	·184	−0·1
15·96	897·96	−13·9	·288	0·0
16·93	898·93	−18·0	·392	+0·2
18·16	900·16	−17·0	·523	−0·2
20·04	902·04	−2·9	·724	+0·1
22·99	904·99	+8·6	8·039	+0·6
Oct. 3·95	915·95	−8·0	9·210	−0·3
7·95	919·95	−10·1	·637	+0·1
17·01	929·01	−12·2	10·605	+0·3
17·88	929·88	−4·4	·698	+0·9
18·94	930·94	+4·6	·811	0·0
19·98	931·98	+10·4	·922	−0·3
24·94	936·94	−18·3	11·452	+0·2
27·99	939·99	+1·2	·778	−0·6
Nov. 3·77	946·77	−17·9	12·502	−0·4
Dec. 7·81	980·81	−1·0	16·138	−0·3
2004 July 7·06	53193·06	+4·5	38·809	+0·1
Aug. 8·12	225·12	−9·5	42·233	+0·3
Sept. 1·08	249·08	+3·2	44·792	+0·1
1·92	249·92	+9·0	·882	−0·3
3·96	251·96	+2·8	45·100	−0·2
4·12	252·12	+1·3	·117	−0·1
6·04	254·04	−15·8	·322	+0·1
14·93	262·93	−13·1	46·272	−0·3
15·92	263·92	−17·4	·377	+0·5
Oct. 25·90	303·90	−9·0	50·648	+0·4
26·85	304·85	−0·8	·749	−0·1
Nov. 13·92	322·92	−7·3	52·679	−0·4
26·79	335·79	+6·9	54·054	0·0
Dec. 26·77	365·77	−11·8	57·256	−0·1
2005 June 28·06	53549·06	+6·6	76·834	+0·2
July 17·05	568·05	+8·6	78·862	+0·4
Sept. 16·99	629·99	−18·4	85·478	−0·3
Oct. 2·89	645·89	−4·3	87·176	+0·2
2006 Aug. 30·08	53977·08	−15·7	122·551	−0·1
2007 Sept. 26·07	54369·07	−18·2	164·421	+0·4
2010 June 23·10	55370·10	−17·0	271·343	−0·2
Aug. 24·06	432·06	+11·1	277·961	+0·1
2013 Sept. 4·00	56539·00	−6·2	396·195	+0·2
Oct. 29·82	594·82	−2·7	402·158	−0·1

km s^{-1}, by Nordström *et al.*[10]; the large uncertainty shows that the star is a spectroscopic binary, but the measurements are not available individually and so cannot assist in the determination of the orbit. The object was put on the observing programme of the Cambridge *Coravel* in 2003 and, being found to have changed velocity a great deal between the first two measurements made a month apart, was observed assiduously, so within the ensuing month its orbit was determined. Observations at progressively reducing frequency have since refined the orbit and helped to reach a total of 46 measures, which are set out in Table III. The orbit is shown in Fig. 3, and its elements are included in Table V below. There is no sign of the secondary, either in the radial-velocity traces or in any distortion of the velocity curve.

At the period of only about nine days, many orbits are circularized, but that of HD 206843 is evidently not; although its eccentricity is small, it is nevertheless fifteen times its own standard error and so is very definitely non-zero. In interpreting the mass function, in this case we are on relatively solid ground in supposing the mass of the primary star to be close to 1 $M_{\odot}$. The mass function is small and requires the secondary to be no more than about 0·16 $M_{\odot}$, corresponding to that of a star far down the M-dwarf sequence, perhaps eight magnitudes fainter than the primary.

The $v \sin i$ determined from the radial-velocity traces is 5·2 km s^{-1}, with a formal standard error (calculated from the mutual agreement of the values given by each observation individually, but neglecting other sources of error and other processes that may affect the line-widths) of 0·35 km s^{-1}. A solar-type star, such as HD 206843 is supposed here to be, if it rotated in the orbital period of 9·4 days, would have an equatorial rotational velocity of 5·4 km s^{-1}; if, rather than being synchronized to the orbital period, it were *pseudo*-synchronized[58] at the modest eccentricity of the orbit, it would be slightly less. The similarity of the observed projected rotational velocity to the figures for synchronized or pseudo-synchronized rotation are certainly suggestive of a 'captured' rotation; if the agreement is indeed significant it also implies an axial (and so also an orbital) inclination close to 90°, or at least high enough that $\sin i$ is not far short of 1.

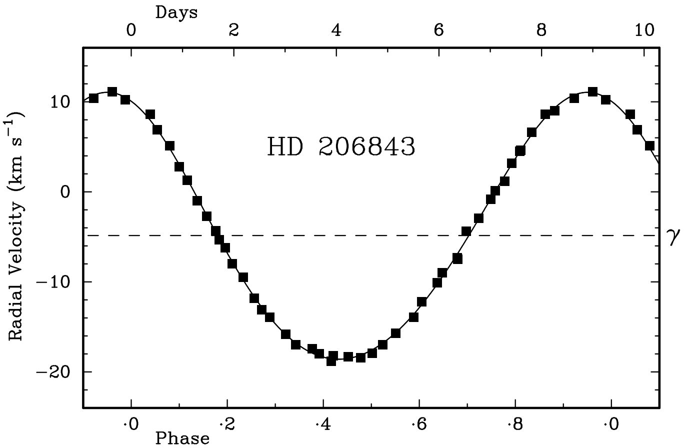

FIG. 3

As Fig. 1, but for HD 206843. In this case, all of the observations come from the Cambridge *Coravel*.

HR 8589 (HD 213720)

At a declination of 54° in Lacerta, the 6^{m} object HR 8589 passes close to the Cambridge zenith. It is about 2° north-following the $4^{m}\cdot4$ star β Lac, and less than 1° following HD 212790, an interesting 7^{m} double-lined binary system whose orbit was presented[59] in Paper 196. Its photometry has been given by Oja[60] (under the designation BD +53°2910) as $V = 6^{m}\cdot38$, $(B-V) = 1^{m}\cdot06$, $(U-B) = 0^{m}\cdot90$. The colour indices are exactly those averaged from K0 III stars[61]; the *HD* type is indeed K0, but the only actual MK classification[62] (from an objective-prism spectrogram, with designation LF4 +53 881) is G8 III.

Four spectrograms of HR 8589 were taken at the David Dunlap Observatory in the early 1940s with the 74-inch reflector and a one-prism spectrograph giving a dispersion of 33 Å mm^{-1} at Hγ. Radial velocities measured from the four plates were sufficiently accordant that they were published[63] only as mean, of $-13\cdot4 \pm 0\cdot7$ ('probable error') km s^{-1}. More than half a century later, de Medeiros & Mayor[1] gave a mean of three velocities from the OHP *Coravel*, as $-9\cdot54 \pm 1\cdot70$ km s^{-1}, and noted that their mutual discordance was enough to identify the star as a spectroscopic binary. After another three years, in 2002, they made their individual measurements available through the CDS (their mean *there* appears to be –8·96), and it was from that listing that the writer selected the star for observations, which started in that same year, from Cambridge. There are now 54 Cambridge measurements, made systematically with 3–6 observations per season over 12 seasons that just cover one circuit of the orbit. By themselves, they yield a circular orbit with a period of 4215 ± 36 days and an r.m.s. residual that, for orbits in this series of papers, is agreeably small at 0·17 km s^{-1}. It is unusual to find an orbit that seems to be exactly circular at such a period, but when the eccentricity is allowed as a free parameter it takes a value that is considerably less than its own uncertainty (0·009 ± 0·014), and the sum of the squares of the residuals falls only from 1·68 to 1·67 km s^{-1} — it does not need a formal statistical test to demonstrate that no significant improvement has been made. If we hope to try to reduce the uncertainty of the orbital period by the inclusion of the OHP velocities, whose mean date is about 1½ cycles earlier than that of the Cambridge ones, it behoves us to put both sets on the same zero-point. The OHP ones have been adjusted by the usual offset of +0·8 km s^{-1}, and since the colour index of HR 8589 is very close to that of HR 6853 (treated above, having a good number of velocities from both sources and found empirically to need an offset of –0·3 km s^{-1} for the Cambridge observations), that same offset has been applied to the Cambridge ones here. The duly adjusted measurements from both observatories are shown in Table IV. Then, with the two sources given equal weights, and the eccentricity fixed at zero, the orbital period is refined to 4201 ± 16 days, which is the best value that the writer can propose from the available data. The full set of elements appears here in Table V, and the orbit is illustrated in Fig. 4. Unusually, the three OHP velocities are shown there as open circles — otherwise two of them are hard to distinguish, and it is quite useful for them to be visible since they halve the standard error of the period.

If, in this case, we can reasonably attribute a mass of 2 $M_{\odot}$ to the primary star, the mass function shows that the secondary must be at least about 0·5 $M_{\odot}$, approximately that of a star of type M0 V, so there is no obligation on the system to appear double-lined. The radial-velocity traces show 'dips' that are almost always very slightly wider than the minimum width; the mean $v \sin i$ is 2·1 km s^{-1}, with a formal standard error much smaller than the 1 km s^{-1} that is the smallest uncertainty claimed for rotational velocities determined from *Coravel* traces, as indicated at the end of the section on HR 6853 above.

TABLE IV

Radial-velocity observations of HR 8589

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

Date (UT)	MJD	Velocity km s^{-1}	Phase	(O–C) km s^{-1}
1986 Aug. 18·04*	46660·04	−5·9	0·056	+0·2
1987 Aug. 11·97*	47018·97	−7·2	0·142	0·0
1993 Aug. 1·09*	49200·09	−11·4	0·661	+0·1
2002 July 21·10	52476·10	−13·0	1·441	0·0
Sept. 2·00	519·00	−13·0	·451	0·0
11·02	528·02	−12·9	·453	+0·2
Nov. 12·92	590·92	−13·3	·468	−0·2
2003 Jan. 17·85	52656·85	−13·5	1·484	−0·3
May 26·11	785·11	−13·3	·514	−0·1
July 16·12	836·12	−13·2	·526	0·0
Sept. 14·99	896·99	−12·7	·541	+0·4
Nov. 12·93	955·93	−12·8	·555	+0·2
2004 Jan. 9·77	53013·77	−12·7	1·569	+0·2
Apr. 3·19	098·19	−12·8	·589	−0·1
July 6·11	192·11	−12·4	·611	0·0
Oct. 7·02	285·02	−12·0	·633	0·0
Dec. 5·78	344·78	−11·7	·647	0·0
2005 May 12·13	53502·13	−11·3	1·685	−0·3
July 20·09	571·09	−10·6	·701	0·0
Sept. 15·02	628·02	−10·5	·715	−0·2
Nov. 9·89	683·89	−10·0	·728	0·0
2006 Jan. 4·83	53739·83	−9·5	1·742	+0·2
July 3·07	919·07	−8·9	·784	−0·2
Sept. 8·05	986·05	−8·3	·800	+0·1
Nov. 25·98	54064·98	−7·8	·819	+0·2
2007 Jan. 20·79	54120·79	−7·5	1·832	+0·2
22·82	122·82	−7·6	·833	+0·1
May 31·10	251·10	−7·3	·863	−0·2
July 25·10	306·10	−6·7	·876	+0·2
Sept. 26·08	369·08	−6·9	·891	−0·2
Nov. 23·90	427·90	−6·6	·905	−0·1
2008 Jan. 17·76	54482·76	−6·5	1·918	−0·2
May 19·12	605·12	−6·2	·947	−0·2
July 24·09	671·09	−5·8	·963	+0·1
Sept. 18·98	727·98	−5·9	·977	0·0
Nov. 22·85	792·85	−5·8	·992	0·0
2009 Jan. 24·74	54855·74	−6·2	2·007	−0·4
July 26·10	55038·10	−5·9	·051	+0·1
Sept. 21·03	095·03	−6·0	·064	+0·1
Dec. 6·79	171·79	−6·4	·082	−0·1
2010 Feb. 1·74	55228·74	−6·4	2·096	+0·1
May 23·11	339·11	−7·0	·122	−0·1
July 22·11	399·11	−7·0	·136	+0·1
Sept. 13·00	452·00	−7·4	·149	−0·1
Dec. 8·94	538·94	−7·8	·170	−0·1

TABLE IV (*concluded*)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O – C) km s⁻¹*
2011 Aug. 8·09	55781·09	−8·8	2·227	+0·2
Sept. 11·00	815·00	−9·1	·235	+0·1
Nov. 17·89	882·89	−9·4	·252	+0·2
2012 Jan. 12·75	55938·75	−10·0	2·265	−0·1
Apr. 30·14	56047·14	−10·8	·291	−0·3
June 29·10	107·10	−11·0	·305	−0·2
Aug. 31·00	170·00	−10·7	·320	+0·4
Nov. 3·02	234·02	−11·5	·335	−0·1
2013 Feb. 1·77	56324·77	−11·7	2·357	+0·1
May 14·10	426·10	−12·4	·381	−0·2
Aug. 28·01	532·01	−12·4	·406	+0·2
Oct. 29·89	594·89	−13·0	·421	−0·2

*Observed with Haute-Provence *Coravel*[1].

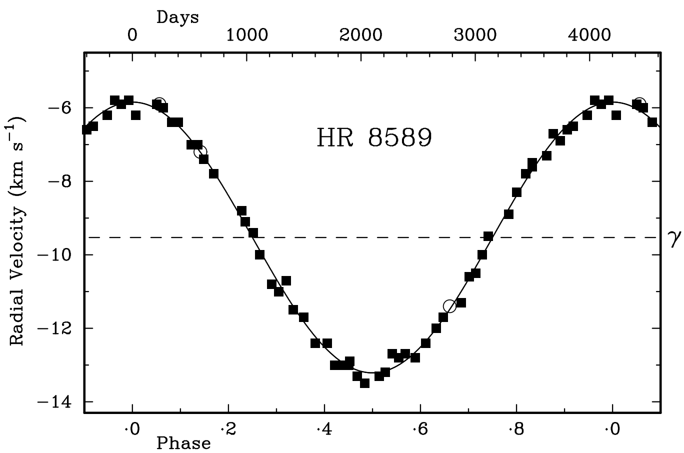

FIG. 4

As Fig. 1, but for HR 8589. All but three of the observations are Cambridge ones. The other three are OHP ones retrieved from the CDS, like those reported for Fig.2; they were given the same weight as the Cambridge velocities, and are plotted here with open circles to make them more easily distinguished than they would be if they were plotted with the filled circles of normal size usually used in these diagrams for OHP measurements.

TABLE V

Orbital elements for the four stars

Element		*HD 48913*	*HR 6853*	*HD 206843*	*HR 8589*
P	(days)	8195 ± 23	4623 ± 18	9·36224 ± 0·00007	4201 ± 16
T or T_0	(MJD)	55938 ± 18	53934 ± 20	53400·82 ± 0·09	54826 ± 6
γ	(km s⁻¹)	+19·97 ± 0·06	−70·87 ± 0·05	−4·85 ± 0·05	−9·53 ± 0·03
K_1	(km s⁻¹)	9·09 ± 0·08	4·12 ± 0·04	14·82 ± 0·08	3·68 ± 0·03
e		0·437 ± 0·007	0·333 ± 0·013	0·078 ± 0·005	0
ω	(degrees)	275·3 ± 1·3	201·6 ± 2·2	21·0 ± 3·6	—
$a_1 \sin i$	(Gm)	922 ± 9	246·9 ± 3·0	1·902 ± 0·010	212·8 ± 2·1
$f(m)$	($M_\odot$)	0·466 ± 0·014	0·0281 ± 0·0010	0·00313 ± 0·00005	0·0218 ± 0·0006
R.m.s. residual (wt. 1)	(km s⁻¹)	0·34	0·23	0·32	0·17

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CORRESPONDENCE

To the Editors of 'The Observatory'

Reply to Mr. Osmaston Concerning the Nature of the Redshift

I will leave it to the reader to decide whether my reply[1] to Mr. Osmaston's comments[2] on Professor Liddle's Gerald Whitrow Lecture[3] indicate that I concur. My comment would have been the same had Mr. Osmaston's "transmission effect" not been incorrectly reported[4] as a "transition effect". I have in fact read the paper[5] referred to by Mr. Osmaston. Even if the effect were true (to my knowledge, it has not been confirmed), it is interesting to note that the authors point out that "it can only be responsible for one part in 10^4 of the total effect [of the cosmological redshift]"[6]. Thus, it seems irrelevant to Professor Liddle's talk.

There are many reports of anomalous redshifts[7]. Most of them have not been confirmed. I will leave it to the reader to check whether the Sadeh *et al.* result and/or Mr. Osmaston's own theory, as outlined in the latter part of his reply[8], will be confirmed.

Yours faithfully,
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[The Editors have now closed this correspondence.]

REVIEWS

Science, Religion and the Search for Extraterrestrial Intelligence, by David Wilkinson (Oxford University Press), 2013. Pp. 240, 23·5 × 15·5 cm. Price £25 (hardbound; ISBN 978 0 19 968020 7).

A review of a book about the interaction between Christianity and the search for extraterrestrial intelligence? What are the editors of *The Observatory* thinking? Surely SETI is 'non-science' and religion has no place in a scientific journal? If that is how you think, then reading this book may lead you to a broader point of view. If you are open both to SETI and religion, then this book is a good introduction to SETI and to how SETI and one form of Christianity interact. It is an interesting and worthwhile read.

The book is titled *Science, Religion and the Search for Extraterrestrial Intelligence* but is actually about 'SETI and Christianity from a Methodist point of view', which means it is quite focussed, but doesn't really cover the broader topics of the interaction between other faiths and SETI, or the general relationships of science to religion. If you wish to consider these matters you are in trouble. For on the former there is a distinct lack of resources, while on the latter, although there are many books, institutions, and university groups, they are of widely varying standards and many approach from a position of trying to justify how science supports their own particular faith or non-faith.

When scientists write about religion and theologians write about science, the Dunning-Kruger effect (he's so incompetent he can't even recognize his own incompetence) often kicks in. However, David Wilkinson is not like that. He did a PhD in star formation before training for the Methodist ministry and getting a PhD in systematic theology, and is now a Professor of Religion, writing extensively on the relationship between science and religion. Indeed the boot is on the other foot — as a SETI astronomer, I'm happy in covering the SETI bit but on the religion bit I'm only an amateur (full disclosure — I'm not only a scientist but also a half-hearted atheist), so you should bear that in mind. I wish I could point you to a review of this book from a theologian's perspective, but so far there does not seem to have been one.

The summary of SETI is good, but Wilkinson goes astray in some points. He thinks that the Fermi Paradox (if they are there, why aren't they here?) "seems to indicate" that we are pretty much alone in the Galaxy, but "does not rule out ETI in other galaxies" as the distances to them are so large that "they" would not have time to get here. While it is true that the simplest explanation of the Paradox is that they are not there, we have no way of inferring that that is more likely than any of the other numerous explanations put forward. And Anders Sandberg in Oxford has shown that intergalactic travel is practicable.

Wilkinson then goes on to describe the rationale behind his choice to believe in a type of Christianity — having had a personal experience of God, and of considering the Bible to contain a reliable account of Jesus being God. It is disappointing to see that that includes the idea that the Biblical Jesus must have been mad, bad, or God, and since he was clearly not the first two, he must be God. But that ignores many other possibilities such as that he was a charismatic preacher who felt he was on a mission from God, and whom later followers came to believe was much more.

In relation to SETI, that section has two main themes. Firstly, how has religion affected the search for ETIs? Like it or not, the reasons why we do science, astronomy, and SETI emerge from the general culture which, up until now at least, has involved religion. Does the concept of an all-present god lead to the idea that the Universe has uniform laws which shape it and thus make enquiry possible? Does the concept of 'the heavens declare the glory of God' lead to exploration being thought a good thing? Wilkinson believes that on the whole religion has supported and guided the sciences, including SETI. I'm not so sure about that. Would Galileo have thought that the Pope was on his side, or Darwin considered Bishop Wilberforce helpful? Apart from egging us on to do more searching, religion does not seem to help us now on how to fashion these searches.

And then secondly, can religion tell us anything about ETIs, and conversely how would a proof of the existence or non-existence of ETIs affect religion? In a recent poll 67% of Americans who went to church once a week declared that ETIs were unlikely as their existence would clash with their religion. On how ETIs would affect religion, Wilkinson gives little thinking on how a billion-year-old civilization would have evolved its thinking on Jesus. But Methodism seems to have a very non-dogmatic approach and its tenets can thus change as science advances, so evolution and now ETIs can easily be accommodated. Learning that ETIs exist and what their beliefs were would merely lead to developments in the faith. However, to me, the trouble with developments which reinterpret Christian religion is that they seem to devalue it. It has its own scriptures and if they are God-given then how can you 'reinterpret' them? As Newman inadvertently demonstrated in his infamous Tract 90 (*Remarks on Certain Passages in the Thirty-Nine Articles*), 'interpretations' of statements of religious belief can be made to mean almost anything, and so have little value. On the other hand if scriptures are man-made then of what special value are they?

An example of a possible problem with ETIs is the question of their relationship to Jesus. Since Jesus is central to Christian faith, does it mean that ETIs cannot exist since Jesus was on this planet? Or perhaps Jesus has to die on every inhabited planet? The 'reinterpretation' given by Wilkinson is that the discovery of an ETI would lead us to learn more about the nature of Jesus, and that Methodism would be deepened and broadened, not disproved. Points like that are not new: an example is the discussion over the last eighteen-hundred years of the fate of people who died before learning about Jesus, such as Socrates and people in the Antipodes.

But we are now venturing deep into theology whereof perhaps this scientist should be silent. — Alan Penny.

Weird Worlds: Bizarre Bodies of the Solar System and Beyond, by David A. J. Seargent (Springer, Heidelberg), 2013. Pp. 309, 23·5 × 15·5 cm. Price £31·99/$34·95/€37·44 (paperback; ISBN 978 1 4614 7063 2).

Planetary investigation was given a huge boost by the Space Age. In fact the Space Age converted a group of astronomical objects that were largely ignored in the first half of the 20th Century into bodies about which we now know a huge amount. And then there was another shock. With the recent discovery of a myriad of planetary systems around nearby stars, replete with their hot Jupiters, and eccentric Jupiters, and planetary migration, we have realized that our Solar System is not the archetypical model planetary system, but is rather a strange exception.

David Seargent has the enviable ability to pick his way through an enormous amount of detail and to spice up our understanding of planets, moons, asteroids, and comets with the odd and interesting aspects of their physics and chemistry. He expertly balances the depth of our knowledge against the huge number of unknowns. When it comes to planets there is still a great deal of work to be done. Questions abound. Why has Mercury lost most of its crust? What made Venus spin round so slowly? Why is there such a large angle between the magnetic axis and the spin axis of Uranus and Neptune? Why is there so much mass in the rings of Saturn? How did the Edgeworth–Kuiper Belt form? Where is the evidence for Martian life? Why does Titan's methane seem to be a relatively recent addition? Does electrostatically elevated dust help to explain transient lunar phenomena?

Wherever you look in the Solar System there are mysteries and variety. Seargent is right, these worlds are weird, and this engagingly written, introductory-level, and extremely accessible book will do much to encourage more people to investigate planets. — DAVID W. HUGHES.

Annual Review of Earth and Planetary Sciences, Volume 41, 2013, edited by R. Jeanloz & K. H. Freeman (Annual Reviews Inc., Palo Alto), 2013. Pp. 819, 24 × 19·5 cm. Price $269 (institutions, about £174), $97 (individual, about £63) (hardbound; ISBN 978 0 8243 2041 6).

Wow! What a giant. *Review* grows bigger and better every year. This year it kicks off with the appropriately named chapter 'On escalation'. That is one of several that take unusual looks at different aspects of evolution and life on Earth. The chapter discusses what drives evolution rather than evolution itself. Close on its heels is a chapter on stromatolites that contains some beautiful field photographs and should be read by anyone who still thinks they are all the same. The biological theme is reinforced throughout the book with other chapters covering subjects such as how to simulate organic compounds in the laboratory under conditions that might resemble those of the early Earth, plant–insect interactions, how grasslands have affected tectonics, and psychrophiles (cold-water organisms). The effect of global warming on the biosphere is represented by a chapter discussing whether Earth's tropical rain forests will survive global warming. Apparently they will if we don't chop them all down first. A chapter on geoengineering discusses how best to combat climate change, weighing up reducing the sunlight that falls on Earth's surface against the removal of CO_2 from the atmosphere. Another very useful chapter provides an update on the heat generated internally within Earth — where it comes from and where it goes. I think I'll recommend it to my undergraduates.

A couple of current hot topics are represented. A chapter on the Anthropocene deals with the question of when it started up. We have now evidently moved beyond the question of whether it is an appropriate concept in the first place. When plate tectonics started up is boldly addressed by Jun Korenaga, who provides a neat and clear summary of current data and opinion (outrageously divergent). If that's not enough wild new stuff, two more chapters introduce subjects that are so new they are hardly subjects at all yet. One describes the emerging field of isotopic anatomies of molecules and minerals — variations in isotopic composition between different mineralogical atomic sites. It seems like only yesterday that geochemists were announcing chemical variations within a single crystal. Another chapter describes 'Earth's hum' — constant on-going free oscillations caused by oceanic and (maybe) atmospheric waves.

A good number of chapters deal with things extraterrestrial. We are given the latest on exoplanets, in particular 'super-Earths' — planets between Earth and Uranus in size — that might be habitable. Two contributions look at meteorites.

One deals with impacts and what the high-pressure minerals they form can tell us about Earth's interior, and a second addresses problems surrounding the origin of meteorites of different composition, and whether more than one type could come from a single differentiated planetesimal (maybe).

Well, there's no maybe about whether this year's *Review* is fantastic value for money. That it certainly is, keeping us all well up to date on current controversies, all with the usual beautiful colour, superb diagrams, helpful boxes and notes, and luxurious-quality paper. Buy yourself a copy immediately. — GILLIAN FOULGER.

Annual Review of Astronomy and Astrophysics, Volume 51, 2013, edited by S. M. Faber, E. van Dishoeck & J. Kormendy (Annual Reviews Inc., Palo Alto), 2013. Pp. 685, 24 × 19·5 cm. Price $235 (print only for institutions; about £146), $92 (print and on-line for individuals; about £57) (hardbound; ISBN 978 0 8243 0951 0).

Astronomy has always willingly had an international perspective as the globally scattered practitioners exchanged ideas to further our science. Over the last 50 years or so, that outlook has increasingly been forced upon the astronomical community as costs have spiralled and giant collaborations have become the norm at the cutting edge of research. But are these shotgun marriages, or is the typical astronomer an isolationist at heart? Yasuo Tanaka, the author of the lead chapter in the 2013 *ARA&A*, is clearly an internationalist, having been one of the leaders of Japan's high-energy-astronomy community to pick up the pieces after the chaos of WW II and work with a number of overseas partners to deliver a series of successful X-ray experiments. His scientific biography is an inspiration.

But to begin near the beginning (of the Universe), Galli & Palla describe 'The dawn of chemistry', in which the first elements were created in the time before structure formation, and were the stuff of which Population III stars were formed. Traces may still be found in 'Cool gas in high-redshift galaxies', described by Carilli & Walter in a chapter which may be read in conjunction with another by Bolatto *et al.* that explains how the abundant — but observationally difficult — H_2 molecule may be measured by proxy by using CO.

After some time, and some initial processing through those earliest stars, the dust and gas is ready to form the kind of stars with which we are familiar, although on the way there, the formation process needs to take into account 'Three-dimensional dust radiative transfer', as related by Steinacker *et al.* Star formation will then proceed to give us clusters containing a wealth of 'Stellar multiplicity', investigated in some detail by Duchêne & Krause (and helped in part, I am sure, by the multiplicity of papers by Professor Griffin in these pages!). Our stars will then run through their lives in the usual way and allow us to investigate the 'Asteroseismology of solar-type and red-giant stars' discussed by Chaplin & Miglio. The Sun, of course, is of particular interest to us, and an update on 'Solar neutrinos' by Haxton *et al.* shows whether we are getting our models of the Sun right. And we really ought to try because 'Solar irradiance variability and climate' (Solanki *et al.*) is of immediate concern.

At last our stars will die and feed the interstellar medium with new chemicals, as outlined by Nomoto *et al.* in 'Nucleosynthesis in stars and the chemical enrichment of galaxies', a modern take on the earlier work of ex-Editor of this *Magazine*, Bernard Pagel (see **118**, 314). This evolution certainly impacts the appearance of galaxies and must be accounted for in 'Modeling the panchromatic spectral energy distributions of galaxies' described by Conroy.

This volume ends with a real *tour de force* in which Kormendy & Ho consider the 'Co-evolution (or not) of supermassive black holes and host galaxies'; this chapter alone takes up 143 pages — enough to fill a book on its own. I was quite amazed to see how far we had got in establishing the statistics of such bizarre objects at the centres of remote galaxies. But then, the whole series of *ARA&A* volumes shows the amazing progress of our science, and Volume 51 is no exception. — DAVID STICKLAND.

Astrophysics Through Computation: With Mathematica® Support, by B. Koberlein & D. Meisel (Cambridge University Press), 2013. Pp. 373, 26 × 21 cm. Price £40/$70 (hardbound; ISBN 978 1 107 01074 1).

This is an undergraduate textbook covering a range of topics in astrophysics. It is complemented by 119 *Mathematica* notebooks that can be downloaded from www.cambridge.org/koberlein. Each chapter is concluded by exercises, some of which can be tackled by pencil and paper and others better done by computer. The topics in the book are chosen to fit available software.

Stellar atmospheres are clearly explained and calculated for realistic continuum opacities, followed by the formation of spectral lines with their different broadening mechanisms. *Mathematica* cannot handle the singularity at the line centre but an easy work-around is provided. Moving on to hydrostatic stellar interiors and polytropes, the explicit solutions for $n = 1$ or 2 are developed but $n = 5$ gives particular difficulty to *Mathematica*. Other values must be tackled numerically following Emden's *Gaskugeln* (1907). A set of polytropic solutions provides mass–radius and mass–luminosity relationships; the effect of radiation pressure is easily incorporated.

Stellar pulsation is based on Ledoux's use of the virial theorem from the 1940s. This is mathematically elegant but there is no mention of the role of the physically important helium-ionization zones. Energy generation is followed through the P–P chain and the CNO cycle. Real stars are non-polytropic and there is a good discussion of fully convective stars, composite stars, and departures from LTE. The gas in white dwarfs is degenerate, leading to the Chandrasekhar mass limit. When the neutrons become degenerate a neutron star is formed. The ultimate compact stellar object is a black hole.

The advance of the perihelion of Mercury could not be explained until Einstein published the general theory of relativity. Other tests are the deflection of starlight at an eclipse and the gravitational red-shift of light from white dwarfs. The most critical test of General Relativity is the binary pulsar PSR1913+16 which is mentioned but not taken further. Astrophysical plasmas are first discussed from a theoretical standpoint covering the effect of magnetic fields, polarization, and absorption. Those phenomena are important observationally for pulsar signals where dispersion can smear out the pulses and a magnetic field causes Faraday rotation. Another current topic is the termination of the solar wind.

There is a great deal about celestial dynamics, including binary stars and clusters. Variational mechanics form the backbone and are used to discuss the *N*-body problem and the stability of the Solar System, which may well be chaotic. The Trapezium cluster in the Orion nebula may also be a chaotic system. The dynamics of the Milky Way are searched for evidence of dark matter and the Hénon–Heiles equations are used to model the gravitational potential of the Milky Way in the neighbourhood of the Sun.

Another topic is the calculation of the orbit of the visual binary ξ UMa. The book first follows Kowalsky's method as presented in Smart's *Spherical*

Astronomy (1960). The student is guided through the orbit calculation in an on-line notebook. There is so much explanation to the code, in this and other notebooks, that they read like recipe books. To make sense of the code it is necessary to have Smart's book to hand because some letters are used twice; *e.g.*, *A* is both a coefficient in the algebraic curve of the apparent orbit and a point on that curve. The problems are compounded because the necessary data files are not provided. But their source is given so that the student can key in the file. That is true in several other places, but some files cannot be found at all. Later, the orbit of the star S2, which passes close to the Galactic centre, is determined by the Thiele–Innes method following Green's *Spherical Astronomy* (1985). It makes the puzzling reference to ξ UMa, discussed earlier: "Thiele–Innes could not be used for ξ UMa because the observations needed to cover more than one orbit period". There are ample observations to cover three periods, and the Thiele–Innes method can be followed from any three well-spaced observations, which may well cover less than a period. Leaving that comment aside, the orbit of S2 is clearly derived and discussed for the laws of gravity of both Newton and Einstein.

This is avowedly not a textbook on how to program *Mathematica*. Students with no previous knowledge will require several hours of instruction to reach the necessary level. Version 9 is recommended as it provides features not available in earlier versions. *Mathematica*'s ability to solve recondite equations is used widely, but there is little use of *Mathematica*'s capabilities for dynamic interactivity. *Mathematica* is proprietary software and many universities have site-licences, but students at other universities will struggle as licences are expensive. There is free software to read the notebooks but not to edit or run them. On the other hand, the provision of such powerful and versatile software removes the need for students to think their way through a problem. If students have more limited resources they are forced to think for themselves. — DEREK JONES.

A Student's Guide to the Mathematics of Astronomy, by D. Fleisch & J. Kregenow (Cambridge University Press), 2013. Pp. 197, 23 × 15 cm. Price £40/$75 (hardbound; ISBN 978 1 107 03494 5), £16·99/$28·99 (paperback; ISBN 978 1 107 61021 7).

The first forty pages of this book explain in exquisite and repetitive detail how to carry out such mathematical tasks as converting between feet and inches (and between parsecs and light years), how to calculate speed by dividing distance by time, and how to multiply numbers such as 3×10^8 by $6{\cdot}67 \times 10^{-11}$. Thus the reader evidently has some way to go before he or she will be able to master the full range of mathematical techniques that might be encountered in astronomy. In other words, the book is evidently intended for those with no mathematical experience at all. But after having mastered the first chapter, the student is presented with a nice selection of problems. Here's one for you: the *Hubble* Ultra-Deep Field captured images of about 10 000 galaxies over an exposure time of one million seconds. How many years would it take to photograph all of the estimated 100 billion galaxies in our observable Universe?

Following the introductory chapter on basic mathematics are chapters on selected topics in astronomy, namely gravitation (including Kepler's laws), radiation (Wien's and Stefan's laws, Doppler effect, radial-velocity curves), parallax, stars (magnitudes, H–R diagram), black holes, and cosmology. The treatment of each is very basic, but the science is sound, and there is a selection of excellent little problems at the end of each chapter. One of them shows an ellipse drawn on some graph paper, and the student is asked to determine the eccentricity of the ellipse, and, if the ellipse represents a planet's orbit, locate the position of the star. And if each division on the grid on the graph paper is one AU and the mass of the star is one solar mass, what is the period of the planet's orbit? So, although the level of the book is very elementary, there is no doubt that, if the student manages to do the problems, he or she will have learned a great deal.

One item in the chapter on gravity may puzzle a British reader greatly, where it is asserted that a kilogram is equal to 2·2 lb on Earth, but would not equal 2·2 lb on the Moon. This is because in the United States many engineers use what is called the British Engineering System of Units, a system used exclusively in the United States and which is and always has been completely unknown in Britain. In that system, I understand, a pound is a unit of *force*, not of *mass*. (The unit of mass is something called a *slug*.) It was a lack of understanding of how to convert between that funny system and the Système International that led to the loss of the *Mars Climate Orbiter* in 1999. The NASA engineers would have greatly profited if they had had the benefit of being able to read the opening forty pages of this book.

The chapter that I enjoyed the most was the chapter on stars, in which the H–R diagram is nicely presented, including a set of lines of constant stellar radius based on the idea that the luminosity of a star is proportional to R^2T^4. Some good physics there.

The chapter on black holes starts by explaining at length (three pages) how to calculate the density of a uniform cube if you know its volume and its mass. As a further exercise in calculating density it shows how to compare the mean density of the Sun with that of the Earth. (Quick, without calculation, which is greater, and by about how much?) After that rather elementary start it soon moves to a convincing calculation of the Schwarzschild radius.

I almost forgot to mention — the book apparently has a website associated with it, with solutions to the problems, for things that are not adequately covered in the book itself, or for which the student may want further explanation. I haven't tried it, but you can have a go (at www.cambridge.org/9781107610217).

This is an elementary book, but I cannot fault the science in it. I spotted two small spelling mistakes and an unfortunate wrong definition of stellar parallax, but nothing major wrong. A strong feature of the book is the excellent selection of instructive problems at the end of each chapter. Could be useful if you are desperately trying to think of some questions for your mid-term exams. — JEREMY TATUM.

A Student's Guide to Entropy, by D. S. Lemons (Cambridge University Press), 2013. Pp. 181, 23 × 15 cm. Price £45/$75 (hardbound; ISBN 978 1 107 01156 4), £17·99/$28·99 (paperback; ISBN 978 1 107 65397 9).

It is rather strange that this book and the previous one reviewed — *A Student's Guide to the Mathematics of Astronomy* — are in the same series and of very similar appearance. Whereas the latter is intended for those with no experience at all with mathematics and who need to be taught scientific notation for numbers and how to calculate density if you know the mass and the volume, the *Guide to Entropy* is a high-powered scientific text that would tax a final-year-honours physics student, not to mention a professor who is supposed to know it all already.

This is one of the most riveting books I have read on the subject of entropy, and is one I expect to turn to again and again. Entropy is one of the harder thermodynamical concepts to grasp — somewhat harder, I should judge, than, say, fugacity or partial molar Gibbs function, but maybe not quite as hard as temperature. The concept of entropy turns up in classical thermodynamics ($dS = dQ/T$), statistical mechanics ($k \ln W$), information theory, and General Relativity. It is not always obvious that the entropies in these contexts are one and the same concept.

Of the eight chapters in the book, the first is on classical thermodynamics, the last is on information theory, and the intervening six — the bulk of the book — are on statistical mechanics. One may gather from this that the reader is expected to have had some grounding already in classical mechanics, so the first chapter is a bit of a reminder. Do not skip over it, though, for it defines a theme that is returned to over and over again in subsequent chapters, namely that, if one can discover an equation for the entropy of a system in terms of its energy and other extensive state variables, one can deduce the equation of state and any other thermodynamic properties of interest.

There is no doubt that the meat of the book is in the six chapters on statistical mechanics. Example after example is given from many branches of classical and quantum physics, yes, and astronomy, too, showing the power of the entropy approach. The clarity of the writing, the breadth of topics covered, and the excellent and instructive examples (with answers) in each chapter combined to give me a deeper understanding and appreciation of entropy than I would have imagined possible. Here's an example from astrophysics: show that the mass and radius of a white dwarf are related by

$$R = \left(\frac{1}{M^{1/3}}\right)\left(\frac{h^2}{Gm_e m_p{}^{1/5}}\right)\left(\frac{5}{2\pi e}\right)\left(\frac{3}{4\pi}\right)^{5/3}.$$

I cite this to give an idea of the academic level of the text. Another astrophysical example asks us to calculate the ratio of the radiation pressure to the gas pressure in the centre of the Sun — though I think there is, unfortunately, some ambiguity in the answer given.

The author gives us many fascinating glimpses into the history of the subject. The famous equation $S = k \ln W$ is engraved upon Boltzmann's tomb — but could Boltzmann have known it? The author argues that this form of the equation requires the third law of thermodynamics, which in turn, he argues, is a consequence of only a fully quantized system. And yet in Boltzmann's day, even the concept of atoms was uncertain.

The final chapter, on information theory, is tantalizing. It is a subject with which I am not sufficiently familiar to comment on with authority, but one can immediately see how the concept of entropy is central to the theory. Initially the subject seems a far cry from classical thermodynamics, but without prompting I found myself struck by the similarity between the Shannon entropy of information theory and the classical Gibbs entropy of mixing. It was like "*déja vu* all over again". Here is an interesting problem: what is the Shannon entropy of a source that produces single characters of the alphabet with frequencies observed in English text? The answer is 4·47 bits, but I was fascinated to see a table of the relative frequencies with which the letters of the alphabet appear in English text. The most frequent letters are *etaon* in that order — the same order that was given by Sherlock Holmes in the celebrated story of *The Dancing Men*.

The one topic that I was hoping for but which is not covered in this book is the role of entropy in General Relativity. I was hoping to find an explanation of why the entropy of a black hole is equal to a quarter of its surface area, or how so many joules per kilogram can equal so many square metres. But that aspect of entropy is the one major point that is not covered. Otherwise this is a truly first-rate book on the subject, and I would happily recommend it as the main (and inexpensive) text for a course on statistical mechanics. — JEREMY TATUM.

New Quests in Stellar Astrophysics III: A Panchromatic View of Solar-Like Stars, with and without Planets (ASP Conference Series, Vol. 472), edited by M. Chavez, E. Bertone, O. Vega & V. De la Luz (Astronomical Society of the Pacific, San Francisco), 2013. Pp. 312, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 828 2).

As the preface explains, this series of conferences is devoted to understanding the properties of stars and stellar systems on different scales: as stellar aggregates, different stellar populations, or individual stars. Meeting III focussed on the star–planet connection through multi-wavelength studies of 'solar-like' stars (defined as "main-sequence objects of intermediate and late spectra [*sic*] types"), and the proceedings provide a rich assortment of recent or on-going research. Though the conference was international, the majority of the participants were (not surprisingly) from Mexico, the host country, and one gets a very positive impression of the vibrancy and enthusiasm of that developing community. The research which they presented is indeed panchromatic, weighted somewhat towards the IR and beyond but including much of the visible and UV, and even X-ray. Papers cover all types of research, from instrumentation, observing, and immediate interpretations of data, *via* surveys and statistics, to modelling and theory. If this book represents a fair cross-section of astrophysics in Mexico, one can feel assured that stellar physics there is very much alive and kicking.

The format of the book follows that of other ASPC volumes, but places the usual casual photos of conference speakers adjacent to their talks; it is certainly helpful to discern the career stage of a writer. Many of the papers describe work in progress, increasing one's positive impression of a youthful, eager community. But there the positive aspects of this volume stop.

There has been no evident attempt at proof-reading the contributions. The four editors may not share good fluency in English, which is certainly not their fault, but it leaves the contributors rather poorly served. Virtually every paper, including those authored by the editors, has errors of some kind. Most are simply grammatical: wrong prepositions, spelling mistakes, surplus or omitted words; often there are 10 or 20 such errors per page, and one begins to wonder at the wisdom, even the kindness, of allowing into print work that could readily have been corrected, made easier to read, and given a slightly more professional appearance. Many of the original figures were produced in colour, and a lot of captions still refer to coloured symbols, leaving the reader of this black-and-white version somewhat mystified — another matter which the editors failed to address. It reflects badly on the ASPCS, but — worse — it may encourage young authors to copy their mistakes in future papers and presentations. There are some scientific errors, too; a few arose from language problems, but those cannot excuse doubtful statements, unjustified claims, or inferences that are not well substantiated by their accompanying plots. Allowances may be made for 'work in progress', but not in contributions that are written as for a journal.

A product like this makes one question the purpose of conference proceedings. The idea must surely be to represent what was communicated to the meeting at the time, appropriately re-formatted. Thus, giving only literature references to data or methods which are central to a talk communicates little to the *reader*, and was surely not how it was presented to the *audience*. Even the title of the meeting is misleading, since several of the papers address evolved or A-type objects, even a galaxy — not by any stretch of the imagination 'solar-like' — and the cover illustration of the *LMT* (Mexico's *infra-red* telescope now at first-light stage) does not represent the meeting's focus on *current panchromatic* research. Many of the papers that were said to be 'work in progress' may well have become out of date by the time this book appeared in print. Is, then, the role of the ASPCS that of a recorder or a journal? — ELIZABETH GRIFFIN.

Gravitation and Spacetime, 3rd Edition, by H. C. Ohanian & R. Ruffini (Cambridge University Press), 2013. Pp. 528, 26 × 18·5 cm. Price £45/$75 (hardbound; ISBN 978 1 107 01294 3).

I wish I had owned this book when I was trying to teach myself General Relativity for the first time. The choice of textbooks then was not extensive, and the landscape was dominated by the giants of Misner, Thorne & Wheeler, or Weinberg. The former insisted that you learn a completely new mathematical approach before proceeding (that of differential geometry); I felt then (and continue to feel) that that is an excessive demand to make of a physicist wanting to get to grips with the subject for the first time. Weinberg used more traditional mathematics, and was full of deep physical insights such as the role of the equivalence principle in forcing us to a metric description of space–time. I remain very fond of his book — but it took few prisoners in its willingness to cover many pages with detailed index manipulations. There was a need for a treatment that would follow Weinberg by discussing General Relativity in familiar mathematical language, at a level suitable for an advanced undergraduate: not shying away from the details, but sympathetic in its pacing.

This new text from Ohanian & Ruffini satisfies the above criteria to a large extent (actually, it is not new; but I had somehow overlooked the previous editions). The authors present a view of the subject that starts with the familiar electrodynamic analogy, and moves to an equation of motion and a field equation purely in the weak-field limit, without needing to move beyond the idea that all that is embedded in flat space–time. The logic of this is pleasing, and it is good to see how far one can get in that way — especially as that is how relativistic theories of gravity got started, although it would have been nice to see more on the history of the dead ends that preceded Einstein. Having then done a number of the standard weak-field topics (orbital precession, light deflection), the stage is set for looping back to consider the exact field equations and strong-field issues (black holes, cosmology). The black-hole section is quite detailed, covering, *e.g.*, the interior maximal Kerr solution and Hawking radiation. The cosmology sections are necessarily more broad-brush: there is so much to say given modern observational and theoretical advances, and only 85 pages to cover it all. The basics of Friedmann models are covered thoroughly, but the last half of the cosmology material is largely descriptive.

In many ways that is an attractive approach. A student meeting General Relativity for the first time would be able to attain a respectable understanding of the subject without unreasonable pain. But I find the determination to hold back on any suggestion of curved space–time is overdone. General Relativity can be quite an intuitive subject, starting above all with the equivalence principle, and the basic idea of curved space–time is equally a deep idea that should be introduced as soon as possible. I still feel Weinberg's route is the optimum: use the equivalence principle to prove that space–time has a general metric character, and to obtain the general equations of motion in terms of derivatives of the metric. All that is achieved by Weinberg in two remarkable pages. At that point, it indeed makes sense to explore the weak-field limit, but it can be approached with a curvature-based physical insight. For example, take the infamous factor 2 in light deflection. That can be derived transparently by noting that the weak-field perturbations in the metric affect space and time parts equally. Thus the reduction in the coordinate speed of light is twice what we would obtain by a Newtonian approach in which only time coordinates were perturbed. The factor 2 is inevitable from the moment the problem is formulated; but such insight is lacking in a flat-space approach.

A further problem for Ohanian & Ruffini is that the bar has been very considerably raised in recent decades, and there are now a number of outstanding texts that approach the subject in a way suitable for the advanced novice: d'Inverno, Hartle, Cheng, for example. This book deserves comparison with those, and can be read together with them for additional useful insights; but I would not see it as a first choice at this level for the reasons of intuition-building referred to above. — John Peacock.

Numerical Modeling of Space Plasma Flows: ASTRONUM–2012 (ASP Conference Series, Vol. 474), edited by N. V. Pogorelov, E. Audit & G. P. Zank (Astronomical Society of the Pacific, San Francisco), 2012. Pp. 300, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 832 9).

This volume contains a collection of forty-three 6–8-page summary papers presented at the seventh annual International Conference on Numerical Modeling of Space Plasma Flows (ASTRONUM–2012), held in 2012 June in Hawaii. There are three major sections in the book, dealing with astrophysical flows, space-plasma flows in Solar System contexts, and numerical methods, together with smaller sections dealing, *e.g.*, with data handling and visualization techniques. The largest of them is the set of papers on astrophysical flows, covering a wide variety of topics including star formation in molecular clouds and its effect on the surrounding medium, the magneto-rotational instability in accretion discs, and the consequences of binary-object mergers involving neutron stars and black holes. There is also a paper dealing with the interaction between the atmosphere of an unmagnetized hot Jupiter with the stellar wind of its main-sequence star, showing how that might explain the asymmetrical light-curve exhibited by transiting exoplanet WASP-12b. The space-plasma-flows section is then focussed mainly in the solar-physics area, particularly concerning the acceleration of the solar wind away from the Sun, and efficiently modelling that outflow into the Solar System for practical 'space weather' predictive purposes using solar data as input. Owing to the recent outer-boundary observations by *Voyager 1*, particular focus also falls on the interaction between the heliosphere and the local interstellar medium, which involves not only magnetized-plasma processes, but also the significant consequences of pick-up of penetrating interstellar neutral hydrogen as well. The numerical-methods section contains papers dealing with techniques applicable to the modelling of both fully- as well as the latter partly-ionized plasmas, together with the complex issues involved in computations of radiative transfer in astrophysical systems. Overall, this volume provides a timely snapshot of the wide variety of numerical modelling work in astrophysical and space-plasma research that now provides an essential contribution in these fields. — Stan Cowley.

The Intriguing Life of Massive Galaxies (IAU Symposium No. 295), edited by D. Thomas, A. Pasquali & I. Ferreras (Cambridge University Press), 2013. Pp. 387, 25 × 18 cm. Price £76/$125 (hardbound; ISBN 978 1 107 03384 9).

IAU Symposium 295 was held in Beijing at the end of 2012 August, while the publication date is 2013 July, so the publishers have been quite speedy in their production of this volume. In addition, some contributions have clearly been updated in the meantime and include references to 2013 papers. The Proceedings therefore give an admirably up-to-date feel for work on the evolution of large galaxies. The list of participants is lengthy and most major groups and players, or their collaborators, were present. The overall emphasis (at least in terms of number of papers) is observational, and even theoretical contributions tend towards the phenomenological (to the extent of one of its practitioners feeling the need to defend semi-analytical modelling in the face of 'simplified' approaches). While the interspersed one-page contributions are little more than 'adverts' for their authors, it should repay the effort of PhD students (and probably their elders) working on galaxy evolution at least to look through this volume as a convenient compendium of present work and search out their own 'nuggets' (of whatever colour). — STEVE PHILLIPPS.

Imaging the Southern Sky, by S. Chadwick & I. Cooper (Springer, Heidelberg), 2012. Pp. 415, 23·5 × 15·5 cm. Price £22·95/$39·95/€42·75 (paperback; ISBN 978 1 4614 4749 8).

Unlike some of the loftier titles gracing publishers' astronomy lists, this chunky paperback tells it like it is. Subtitled 'An Amateur Astronomer's Guide', and badged in the 'Practical Astronomy' series of the late Sir Patrick Moore, it is a genuinely useful contribution to the 'how-to' literature of astronomical imaging. If you could bring yourself to describe an astronomy book as 'down to Earth', this would be an excellent candidate.

Written by two prominent New Zealand amateurs, Stephen Chadwick and Ian Cooper, the book is a compendious colour atlas of all that is glamorous south of the celestial equator. Nine chapters group together the clusters, nebulae, and galaxies worth imaging in nine broad swathes of the southern sky. For each chapter, there is a brief introduction to the featured area — often with a reference chart — followed by images and detailed descriptions of the selected sources. Practical suggestions on how to go about imaging each field complement the descriptions, together with technical information on the images themselves. All excellent stuff. But wait — there's more. At the end of the book, three further chapters provide a constructive mini-handbook of digital imaging, including choice of equipment, observing techniques, and image-processing.

Amidst the accolades, though, a mild word of warning. For those traditionalists who were brought up, as I was, on the true-colour astro-imaging pioneered in the early 1980s by David Malin, some of the emission nebulae in this book look a little garish. There is a good explanation for this, which the authors are happy to provide. Owing to shortage of time in the book's preparation, many of the images were made through narrow-band filters to allow observing in moonlight. Fair enough — but one can't help wondering whether, here and there, the saturation knob was tweaked a little too enthusiastically in the post-processing.

Another idiosyncrasy that could irritate some readers is the provision of home-grown names for otherwise nameless (although numbered) objects. Taking their lead from John Herschel with his Keyhole Nebula, and following a long tradition of informal names bestowed by professional and amateur astronomers alike, the authors have added a few of their own. While I certainly don't have a problem with it, some might baulk at the Running Chicken Nebula, the Flying Jaw Nebula, or Thor's Helmet. Maybe just a little too down-to-Earth?

The book is excellent value for the price, and its sturdy binding should be well able to withstand heavy use from keen astro-imagers. As far as I can tell, it is remarkably error-free, although the one typo I did find turned up twice. I almost wondered whether 'the plain of the Galaxy' was an intentional pun, conjuring up a rather poetic image of our Galaxy's disc. In a book that is unashamedly about imagery, I guess that would have been perfectly acceptable.

This volume should be on the shelves of any amateur astronomers who aspire to produce professional-standard images, whether they live north or south of the equator. And for casual stargazers, it doubles as an excellent guide to the southern hemisphere's most sumptuous star clusters. Highly recommended. — FRED WATSON.

Deep-Sky Companions: Southern Gems, by S. J. O'Meara (Cambridge University Press), 2013. Pp. 466, 26 × 18·5 cm. Price £30/$48 (hardbound; ISBN 978 1 107 01501 2).

Here is an exceptional deep-sky armchair and field book for amateur astronomers. Not only do you get an interesting selection of deep-sky objects, but the added bonus of well-researched, in-depth background information for each object too. The author has included both historical information and modern data to give the reader just about everything known about each selected object. The historical information takes you into an amazing world of early telescopic astronomy. You can read the very expressive descriptions the original discoverers recorded and learn of the feats they accomplished with old-style equipment. The modern data pack a 'wow-factor' punch with vivid explanations of the incredible dynamics in our amazing Universe. This book certainly has something for everyone.

There are 120 deep-sky objects which have been selected from a catalogue by the early Australian pioneering astronomer James Dunlop. In this book each object is given up to seven pages of extraordinary information, along with modern images, sketches, sky charts, and visual descriptions as seen through a small- to medium-sized telescope. The author has even included a description on how to find the object in the sky. If you like showing friends, family, or the general public the sky through your telescope, then this book will certainly help you educate and inspire the great excitement our hobby generates.

I was also impressed with the full background history on James Dunlop's work and other early telescopic astronomers. The author focussed on 120 selected objects from Dunlop's catalogue to compile a selection of objects analogous to the northern-sky Messier list, with an appendix list of a further 42 deep-sky objects.

When I first opened this book of 'southern gems' I was expecting a selection of the very brightest deep-sky beauties in the southern skies. The name of this book is a little misleading in that it really is a selection of both bright and somewhat faint objects. Perhaps the title could have been 'Dunlop's Southern Gems'. But then, what's in a name when this book really is full of information, wonder, and adventure.

I did notice one error regarding object No. 115. The object is a galaxy, NGC 7410, which lies in Grus; however, in the table the constellation is given as Centaurus.

I would recommend this book highly to new and experienced amateur astronomers, and those living in both the northern and southern hemispheres, because it's one that is both fascinating and exciting. — JENNI KAY.

PAPERBACK RELEASE

The Cosmic Century, by M. Longair (Cambridge University Press), 2013. Pp. 545, 24·5 × 17·5 cm. Price £30/$48 (ISBN 978 1 107 66936 9). Reviewed in **126**, 428, 2006.

OBITUARIES

George Howard Herbig (1920–2013)

George Herbig was an outstanding member of a notable generation of astronomers who established a solid foundation for today's astronomy. He was born on 1920 January 20 on the East Coast of the USA but lived for most of his life on the West Coast before finally moving to Hawaii in 1987 — essentially to avoid the astronomer's nightmare of having to retire. While he did eventually retire in 2001, he published his final — his 138th — paper (with B. Reipurth and S. Dahm) in 2012. It is therefore unsurprising that he received many awards including the C. W. Bruce Medal and the Medaille from the Université de Liège.

George's interest in astronomy began early in his school days when he built his own telescope. His formal education completed at the University of California at Los Angeles, he joined the Lick Observatory as a WWII assistant in 1943. His thesis title foreshadowed his future astronomical research — it included that long-term interest, nebulosity. He was then appointed to the staff of Lick in 1948 and became a full professor at the University of California in 1966, the same year as the Lick astronomers ceased living on Mt. Hamilton and moved to the University of California, Santa Cruz.

His scientific interests were focussed on young stars: their formation and evolution towards maturity. His research on the pre-main-sequence T-Tauri stars led to a fruitful collaboration with Guillermo Haro and their study of Herbig–Haro objects. His name is also attached to the Herbig Ae/Be stars. His interests in young stars naturally led him to study the interstellar medium, including some of its most intractable problems.

George also designed the superb high-resolution coudé spectrograph for the 120-inch Lick *Shane Telescope*, which he used to great scientific effect, and also ensured that the spectrograph could be used when the main reflector was in use for non-spectroscopic purposes, *e.g.*, photometry in the dark of the Moon, through the addition of a coudé auxiliary-feed telescope.

I first got to know George in 1964 when I spent a year at Yerkes Observatory with Chandrasekhar developing computational methods to study the collapse of interstellar gas clouds. Part of that year's activities was a grand tour of the major US observatories; Lick Observatory was the first stop on the US West Coast. George was my guide on that tour and it was a life-changing experience as he convinced me that while theory could have its moments, the acid test would be what actually happened to the constituents of interstellar space. One such component, well known to stellar spectroscopists since the early 1900s, the carriers of the diffuse line absorption, remains unidentified. In 1969 I had a further opportunity to observe with George using the *Shane Telescope* and its fine high-resolution spectrograph. It was then that I got first-hand experience of the care George took in making the final adjustment of the spectrograph to obtain the best possible resulting spectra. We discussed the known facts regarding the diffuse interstellar bands — widely distributed in our own and other galaxies. George was also instrumental, together with the late Donald Osterbrock — then Director of the Lick Observatory — in allowing me ten nights annually between 1978 and 1984 to re-observe the diffuse interstellar lines using a new Mark II Varo image tube — a device designed to enhance night vision and not for spectroscopy! However, the final spectra still had to be recorded on photographic plates; but useful insights were gained into the interstellar environmental dependence of the carriers of the diffuse lines.

Our final encounter was an effort to establish whether or not the carriers of the diffuse-line absorption might exist in the tails of comets. Independently of each other we seized on the reappearance of Comet Halley in 1985. The null result was published by George in the *Astrophysical Journal* in 1990. This observation is very exasperating to make. Not only is a cometary appearance a short-lived event, there is only a very small chance that the tail will actually occult an early-type star. Unabashed, George and I joined forces when Comet Hale–Bopp appeared. We had the benefit of a comparatively long period from first detection to tail formation to receive an excellent determination of the orbit — and we were lucky to find that the tail would occult an early-type star. But had it not been for George's formidable reputation we would not have had the facilities of the *Keck* for 3 hours on each of two nights — to be fair it was a mad enterprise. But once again, and par for the diffuse-lines course, we confirmed the Halley's comet result: no trace of any diffuse lines was obtained. As many very distinguished spectroscopists before us had painfully discovered, the carriers of the diffuse interstellar absorption lines will not give up their secrets easily.

George Herbig was an astronomer of great integrity and insight. He spanned a range of disciplines — instrument design, meticulous attention to optimizing instrumentation, exacting observational techniques, a high standard of astrophysical knowledge, and maintaining interpretation within the boundaries of the available data. His conversation was thoughtful, to the point, and quietly expressed. It was a privilege to know him. With his death on 2013 October 12, astronomy has lost a great practitioner and, to many of us, a much valued colleague. — DEREK MCNALLY.

Dimitri Mihalas (1939–2013)

World-renowned astrophysicist Dimitri Mihalas passed away in his sleep at his home in Santa Fe, New Mexico, on 2013 November 21. He retired from the University of Illinois at Urbana-Champaign in 1999 and from the Los Alamos National Laboratory in 2011.

Dimitri was born on 1939 March 20 in Los Angeles, California, where he grew up. He received his BA, with Highest Honours, in three majors: Physics, Mathematics, and Astronomy from the University of California at Los Angeles at the age of 20. Four years later he received his PhD in Astronomy and Physics from the California Institute of Technology. He then joined the faculty of the Department of Astrophysical Sciences at Princeton University. In the following three decades he was a professor in the Department of Astronomy at the University of Chicago, the University of Colorado at Boulder, and the University of Illinois at Urbana–Champaign. He was also a pioneer in astrophysics and computational physics and remained a world leader in the fields of radiation transport, radiation hydrodynamics, and astrophysical quantitative spectroscopy for most of his career. His broad knowledge and immense contributions earned him election to the US National Academy of Sciences in 1981 (at age 42, fifteen years earlier than the usual age of entry) and many other distinguished awards. During a visit to Britain in the early 1970s, he spent some time at the Royal Greenwich Observatory, Herstmonceux, a stay enjoyed and appreciated by many of the staff and students there.

Dimitri had an exceptional record of both quantity and quality of work, and developed new and far-reaching methodologies yielding results of great importance. He made outstanding contributions to the field of astronomy and astrophysics. Besides many high-quality papers, he authored or co-authored seven books and co-edited three others. His *Stellar Atmospheres* was of especial value to me as a lucid guide in my early research.

Throughout his long career, Dimitri gave generously of himself to all with whom he interacted. He touched the lives and careers of many students and colleagues and has left a lasting legacy to be cherished by those who knew him. — DAVID STICKLAND (adapted from an obituary written at the Los Alamos National Laboratory).

Here and There

RELATIVELY INACCURATE

Einstein equation an equation expressing the relation of mass and energy: $E = MC^2$. E is the energy in joules; M is the mass in grams; C is the velocity of light in centimetres per second. — *Gage Canadian Dictionary* (Gage Educational, Toronto), 1983.

I MUST BE IMAGINING THE PLEIADES

Clusters disperse on time scales of a few hundred years — *A&A*, **521**, 12, 2010.