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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2010 October 8 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

R. L. Davies, *President*
in the Chair

The President. Welcome to the new session of meetings of the Society. To start with, I have the pleasure of congratulating a few people: Max Pettini, who has been elected a Fellow of the Royal Society; Jocelyn Bell-Burnell, who has won the Faraday Prize of the Royal Society; Carlos Frenk, who has won the Institute of Physics Hoyle Medal; James Binney, who has been awarded the Institute of Physics Dirac Medal; Katherine Blundell, who has won the Royal Society Rosalind Franklin Prize; and Simon White, from the Max Planck Institute in Garching, who won the Institute of Physics Max Born Prize [applause]. That's terrific, but I hadn't quite finished [laughter]. You may also congratulate Ray Wilson and Roger Angel who, together with Jerry Nelson, shared a Kavli Prize, for telescope technology. And yesterday, those of you registered at *The Times* will have seen in the science supplement, *Eureka*, that they have had a poll to find the 100 most influential scientists in the UK; people whom you might recognize and appear on that list are: Stephen Hawking, Martin Rees, Jocelyn Bell-Burnell, and George Efsthathiou. Four in the top 100 is not too bad, and in the under-40 top group, we have Lucie Green and Sarah Bridle, both from UCL, so congratulations! [Applause.]

I am also pleased to announce the award of the Michael Penston Astronomy Prize to Dr. Bao-jiu Li, from the Department of Applied Maths and Theoretical Physics, Cambridge, for the best thesis, entitled 'Physical and cosmological implications of modified gravity theories'. The runner-up was Dr. Emily Curtis of the Astrophysics Group at Cambridge. The Keith Runcorn Prize, which is the symmetric version for Geophysics, is awarded to Dr. David Halliday, from Edinburgh, for his thesis entitled 'Surface wave interferometry'. The runner-up was Dr. Lasse Clausen from the University of Leicester, presently at Virginia Tech, USA. We're hoping both of those prize-winners will give a presentation on January 14 next year.

I can now move on to the formal programme. It's a delight to welcome Professor Ned Wright, from UCLA, who is going to tell us about '*WISE* observations of asteroids in the thermal infrared'.

Professor E. L. Wright. The *Wide-field Infrared Survey Explorer* (*WISE*) is a medium-class Explorer (MidEx) satellite launched on 2009 December 14. It surveyed the entire sky in four infrared bands centred at 3·4, 4·6, 12, and 22 μm during the period from 2010 January 14 to August 7. At that time the solid-hydrogen tank cooling the telescope was exhausted, and the telescope warmed to 45 K, which saturated the 22-μm band. A second solid-hydrogen tank continued to cool the detectors, and the 12-μm band functioned until the second tank was exhausted on 2010 September 29. After that time *WISE* continues to scan the sky at 3·4 and 4·6 μm until a planned end date of 2011 January 31.

The infrared sky at 3·4 and 4·6 μm is dominated by Galactic stars and starlight from external galaxies. At 12 and 22 μm interstellar nebulosity is bright near the Galactic Plane, and star-forming galaxies and AGN occur sparsely over the entire sky. But asteroids, heated by the Sun to a sub-solar temperature of 400 K at 1 AU and 250 K at 2·56 AU in the main asteroid belt, also radiate strongly at 12 and 22 μm. Since IR sources that do not appear on optical surveys are objects of intense interest for follow-up, it was essential that *WISE* observe in a pattern that allowed asteroids to be separated from celestially fixed objects. Multiple observations of each position on the sky would be needed to detect and remove the effects of charged-particle hits on the detectors, so *WISE* chose to collect these multiple observations over a period of about a day by laying down a continuous series of minimally overlapping frames on the circle perpendicular to the Earth–Sun line. The position of the Sun on the sky moves 1 degree per day, while *WISE* orbits 15 times per day and the frame size is 0·8 degrees, so *WISE* gets 12 or more images on each part of the sky. In the 95 minutes it takes *WISE* to orbit around the Earth, asteroids typically move by an easily detectable 1′. The *WISE* Moving Object Processing System (WMOPS) analyzes the sources that do not appear at fixed celestial coordinates and attempts to arrange them into 'tracklets' that each have an object moving at a constant rate across the sky. A visual display of 'postage-stamp' images around each moving-object tracklet is then examined by eye for all objects without a known identification. Accepted tracklets are then sent to the IAU Minor Planet Center. By the end of 2010 September *WISE* had sent nearly 4 million asteroid positions to the MPC, nearly four times as many as the next-most-active observatory.

For an asteroid with a known distance from the Sun, the surface temperature can be modelled assuming thermal equilibrium. This allows the observed flux to be converted to a solid angle, and since the distance to the observer is also known the solid angle gives a projected area and hence a diameter. *WISE* has collected data that will yield radiometric diameter estimates for about 160 000 asteroids, of which about 20% were newly discovered. Smaller asteroids with well-known orbits, too faint to be detected by WMOPS on single frames, can be recovered by stacking frames in a moving reference frame, but are not included in the totals above.

While a typical main-belt asteroid is observed about 15 times by *WISE*, near-Earth objects (NEOs) move at higher rates and can be seen in a much more variable number of frames. In particular, the first NEO discovered by *WISE*, 2010 AB_{78}, first crossed the scan circle 90° behind the Sun in ecliptic longitude on January 13, then sped up as it approached perihelion and caught up with the scan circle again on February 25, and then fell back to cross the scan circle again on July 1. As a result it was measured 59 times by *WISE* over a span of 5·5 months, and seen along widely separated lines-of-sight. The July data are fainter than expected in a simple thermal model, but a thermophysical model that allows for thermal inertia can account for the July data by assuming that the 'morning' side of the asteroid was being observed. This fixes the rotation axis of the asteroid, giving an angle between the orbit pole and the rotation pole of 90° ± 17°, so 2010 AB_{78} is an oblique rotator like Uranus. The derived diameter is 1·33 km. The thermal inertia and albedo of the asteroid surface are also determined: $\mathcal{J} = \sqrt{\{\kappa\rho C\}} = 220 \pm 110\ \ \mathrm{J\,m^{-2}K^{-1}s^{-0\cdot5}}$ and the Bond albedo is $A = 0{\cdot}02$. This means that the thermal-infrared peak in the spectral-energy distribution contains 49 times more energy than the optical scattered-light peak, and since IR photons are 20 times less energetic than optical photons, that there are 1000 times more IR photons than optical photons.

Note that the asteroids discovered by *WISE* are often fairly dark, but this is in part due to a selection effect: the more reflective objects have often already been discovered by optical surveys. A debiassing operation to calculate the true albedo distribution is under way.

WISE has found 120 NEOs to date, including two very recent discoveries with semi-major axes very close to 1 AU (2010 SO_{16} and 2010 TK_7). These objects are very close to the 1:1 mean-motion resonance with Earth and will follow complicated, highly perturbed orbits. Both these objects were discovered at high ecliptic latitudes as they passed over or under the Earth, but they do not have particularly high inclinations.

The *WISE* project plans a preliminary data release in 2011 April that will include the first 55% of the sky observed. The release will include the individual frames, a database of individual frame detections, an image atlas made from co-added frames, and a catalogue of sources detected on the atlas images. The 5-σ point-source sensitivity of the stacked frames should be better than 0·08, 0·11, 1, and 6 mJy, and angular resolution will be 6, 6, 6, and 12″, while the astrometric precision will be better than 0″·15 (1σ) in each axis for high-*S*/*N* sources.

The President. Thank you very much.

Mr. H. Regnart. It's just a suggestion, really. If you have funding difficulties, apply to re-insurance industries. I don't think I need to explain why! [Laughter.]

Dr. S. J. Warren. I wanted to understand what fraction of the asteroids you find are new discoveries, and what are known.

Professor Wright. We've observed about 160 000 objects, and about 35 000 of them are totally new. For about half of them we know what they are: there are well-published orbits. For another 30% of them, the IAU Minor Planet Center finds that the *WISE* tracklet is consistent with a previously submitted tracklet that was not observed long enough to generate an orbit. But about 20% of the *WISE* objects are newly discovered.

Dr. P. Daniels. With the better determination of albedo variation, and asphericity of asteroids for the near-Earth objects, are you then going to apply those to the Yarkovsky-effect calculations and re-determine the orbits?

Professor Wright. Well, that's a very good thought. The Yarkovsky effect is the momentum that is created by the asymmetric radiation due to bodies being hotter in the afternoon. So you actually produce a torque that modifies the orbit, and, yes, we can calculate that. It's a little more complicated in the particular object I considered because it's an oblique rotator. When we observe objects in both the morning and evening we get a pretty good handle on the asymmetry so we can measure the Yarkovsky effect. We hope to do that for a large sample of Baptistina-family objects

Professor M. Rowan-Robinson. Do you have any interesting sensitivity to Planet X, or what I suppose should now be called Planet IX?

Professor Wright. Planet X is way out in the Kuiper Belt or beyond, and we're particularly insensitive to Kuiper Belt objects. We did manage with difficulty to get Pluto with our observations but only in reflected light, and if you have an object out there it has to be quite heavy like a gas giant like Jupiter and self-luminous for us to have a particularly good handle on measuring it; so if you moved Jupiter out to 1 light-year, you should be able to see it — that is if you don't let it cool off due to the fact it's further away. [Laughter.] At least half of the radiation of Jupiter is coming from the inside. In the case of Planet X proposed by Matese and Whitmire with 2–3 Jupiter masses and a distance of about 25000 AU, it gives a signal-to-noise ratio of about 1000:1, so it could well be one of the brown-dwarf candidates I was talking about this morning. If we go back and look at it, it will have moved 20 arc seconds, and people will say, "Hmm". Gas giants have a methane-dominated spectrum, but if you had a rocky planet like the Earth which has a luminosity of $10^{-13}\ L_{\odot}$ due to inherent radioactivity, it would have a temperature of about 30–35 K, which is too cold for us.

The President. We should wrap it up now; thank you very much! [Applause.] The next speaker is Ian Morison, whose topic is 'Are we alone?'

Professor I. Morison. SETI, the Search for Extra-Terrestrial Intelligence, has now been actively pursued for 50 years without success. However, this does not necessarily imply that we are alone in the Milky Way galaxy for, although most astronomers now agree that intelligent civilizations are far less common than once thought, we cannot say that there are none. But it does mean that they are likely to be at greater distances from us and, as yet, we have only seriously searched a tiny region of our Galaxy.

The subject may well have been inspired by the building of the 76-metre *Mk 1* radio telescope at Jodrell Bank in 1957. In 1959 two American astronomers, Guccione and Morrison, submitted a paper to the journal *Nature* in which they pointed out that, given two radio telescopes of size comparable to the *Mk 1*, it would be possible to communicate across interstellar distances by radio. They suggested a number of possible, nearby, Sun-like stars that could be observed to see if any signals might be detected. They also indicated that one should search around the radio spectral lines of H and OH, whose frequencies would be known to all intelligent civilizations.

The following year Frank Drake, the father of SETI, used a 25-metre telescope at Green Bank, West Virginia, to observe τ Ceti and ε Eridani in what was called Project Ozma after L. Frank Baum's imaginary land of Oz. Since then there have been nearly 100 serious SETI searches. In 1977 a telescope called 'Big Ear' operated by Ohio State University, which had been carrying out an all-sky SETI survey since 1974, picked up a signal that appeared to have all the right characteristics. It is called the "Wow" signal as the astronomer analysing the data wrote the word in the margin of the computer printout. Sadly, in follow-up observations, no signal has ever been picked up from the same region of sky.

Two significant searches have used the 305-metre Arecibo telescope in Puerto Rico. The first of these, Project SERENDIP (Search for Extraterrestrial Radio Emission from Nearby Developed Intelligent Populations), still continues, whilst the second, Project Phoenix, terminated in 2003. SERENDIP, under the auspices of the University of California, Berkeley, is using the Arecibo dish in 'piggy-back' mode with a dedicated feed system observing the sky close to wherever other astronomers are pointing the telescope. Though they have no control over what part of the sky is being observed, over a few years most of the sky accessible to the telescope will be observed, much of it several times over. SERENDIP is thus looking for signals that are seen on more than one occasion from the same location in the sky. A small part of these data, relating to a narrow band of radio frequencies close to the 1400 MHz hydrogen line, is being analysed by home computers across the world in what is known as SETI@home.

This does highlight a real problem: an ET signal might be transitory and one really needs to make an immediate confirmation that any signal has an extra-terrestrial origin. That was the premise of Project Phoenix which arose out of the NASA SETI project when the American Congress cut funding. This had been managed for NASA by the SETI Institute, which then raised private funds to continue the targeted-search part of the NASA programme and observe around 800 nearby Sun-like stars. In project Phoenix, two telescopes were used to make simultaneous observations so that any signals originating within our Solar System could be eliminated and there would be an immediate confirmation of any extra-terrestrial signal. Initially pairs of telescopes in Australia and the USA were used, but when the Arecibo telescope came back on-line after a major upgrade, it was used in parallel with the University of Manchester's 76-metre *Lovell Telescope* at Jodrell Bank. Owing to their separation across the Atlantic any local interference at either telescope could be immediately discounted. The system was proven each day by observing the very weak signal from the *Pioneer 10* spacecraft, then more than 10 million km from Earth and far beyond Pluto! It hardly needs saying that no positive signals were detected.

The lack of success prompts one to ask what the likelihood is that other advanced civilizations exist in the Galaxy who would be attempting to contact us. If we do not expect there to be any other civilizations then there would not be a lot of point in searching. This problem was first addressed by an eminent group of scientists at a meeting organized by Frank Drake at Green Bank in 1961, who produced the now-famous Drake Equation. Some of the factors in the equation are reasonably well known, such as the number of stars born each year in the Galaxy, the percentage of those stars (like our Sun) that are hot enough, but also live long enough, to allow intelligent life to arise, and the percentage of those that have solar systems. But others are far harder to estimate. For example, given a planet with a suitable environment, it seems likely that simple life will arise — it happened here virtually as soon as the Earth could sustain life — but it then took several billion years for multi-cellular life to arise and finally evolve into an intelligent species. So it appears that a planet must retain an equable climate for a very long time.

The conditions that allow this to happen on a planet may not be commonplace. Our Earth has a large moon which stabilizes its rotation axis. Its surface is recycled through plate tectonics which releases carbon dioxide, bound up into carbonates, back into the atmosphere. This recycling has helped keep the Earth warm enough for liquid water to remain on the surface and hence allow life to flourish. Jupiter's presence has reduced the number of comets hitting the Earth; such impacts have given the Earth much of its water but too high an impact rate might well impede the evolution of an intelligent species. It could well be, as some have written, that we live on a "Rare Earth". How many might there be amongst the stars?

The President. Thank you very much.

Professor Wright. You didn't say anything about the chlorophyll edge which we heard about in infrared photography, but you know that's another signature for life.

Professor Morison. Earlier this year, we saw a glint of one of the seas on Titan, and in principle, with the *JWST* and an occulter, we might be able to spot the reflection of the surface of an ocean on another planet, perhaps indicating that the conditions are right for life.

Dr. G. Q. G. Stanley. The "Wow" event — I've often seen it referred to but not its duration. How long did that signal last?

Professor Morison. It was only the matter of about minute and half, I think. Basically, the telescope beam was fixed due south and sources were observed as they transited and passed through telescope beam, so the observing period was very short.

Mr. N. Calder. I can remember Martin Ryle being furious when Arecibo sent out a signal without permission. Why should we think that these people will be friendly?

Professor Morison. Well, that's absolutely true. That's a very good reason for us not sending a signal because you are obviously letting people know that here is a rather nice piece of interstellar real estate, and they might think "Let's come and take it for us"! It's going to take a long time, so I'm not too worried about that, and I don't believe in UFOs, just because I don't think life is that common. There's another thing about UFOs: you know they come and abduct people, don't they? [Laughter.] The thing that annoys me is, if I went on a spacecraft to another planet, I would love to talk to people like this audience. But they have never spoken to any of my friends! It's not fair [laughter]. I understand the signal was actually sent to the globular cluster M 13 so it won't get there for another 26 000 years, so nothing could possibly happen for a very long time. However, we probably shouldn't transmit, although over the last 50 years we have been transmitting fairly powerful signals — all our television signals, for example. But what is happening now? They are being turned off, aren't they? We're having much-lower-power digital signals; we have satellite television, which doesn't beam at all; or cable, even better. So I think the leakage time, where any significant radiation comes from a planet — which of course is wasteful if you can pick it up a long way away — is a very short time in the life of a planet or an advanced civilization and hence the chance of actually picking it up at the right time somewhere is essentially zero.

A Fellow. Is there an agreed protocol in case we find a signal?

Professor Morison. Yes. There is a committee I'm actually on, which is the SETI post-detection committee, that has to decide what to do, but in fact the UN has also appointed someone who is their 'Take-me-to-your-leader' leader [laughter]. A lady, in fact. So yes, there is an agreed protocol. It doesn't worry me too much that it's going to happen, though.

Dr. R. Massey. Would you consider extending the search to beyond Sun-like stars, because when we look at the number of planets around Gliese 581 for instance ... .

Professor Morison. Yes, that's a red dwarf.

Dr. Massey. Could you visualize extending it to those stars?

Professor Morison. Yes, SERENDIP is not targeting, it's just looking at the whole of the sky. With Phoenix we were targeting, but that was only because, given a certain amount of time, you look to those places where you think it's most likely. But I think the one thing we should not do is to say everything else out there would be like what we know. We must keep our eyes open.

Professor I. Roxburgh. What you have said is predicated on the assumption that in the last few hundred years we've learned all there is to know about physics. A few hundred years is a very short time in the life of the planet.

Professor Morison. Yes, but how much new have we learnt in physics in the last 20 years, or 50 years?

Professor Roxburgh. How much new will we learn in the next million years? It is still a small fraction.

Professor Morison. I don't know — no one managed to shoot Einstein down yet. I think we can say we are getting to know quite a bit about the laws of physics.

Professor D.W. Hughes. And that's what people said in 1895 [laughter].

Dr. C. Trayner. I will give you one example of where history tends to outrun things. Thirty or forty years ago, we might have said that the sort of transmissions people will send would be like morse code. You would find regular pulses. Nowadays, with our proper understanding of what Claude Shannon tells us about information theory, you realize it looks more like noise.

Professor Morison. I agree there are a lot of things one has to learn, but I don't think the fundamentals of physics are going to change. What I was trying to say is that we should try and be more open.

The President. I think we should leave it there. Thanks very much!

The President. Our next speaker is Dr. Barbara Ercolano, who is going to tell us about 'Why do we need 3-D radiative transfer? Science highlights from MOCASSIN'.

Dr. Barbara Ercolano. A large number of astrophysical problems depend on our understanding of how radiation diffuses through and interacts with matter. Theoretical studies rely heavily on our ability to perform numerical simulations of complicated systems where energetic photons travel through highly asymmetric and inhomogeneous density fields. Examples where radiative transfer plays a crucial rôle range from the small scales of planets that form within the circumstellar discs of dust and gas surrounding young solar-like stars, to the formation of the stars themselves from molecular clouds, to the very largest scales of cosmic re-ionization.

Today I will describe some recent results obtained with the 3-D radiative-transfer code, MOCASSIN, that I developed over the last ten years, and for which I was awarded the Royal Astronomical Society Fowler Prize for Astronomy this year.

MOCASSIN (MOnte CArlo SimulationS of Ionised Nebulae) applies a Monte Carlo method to the transfer of radiation, which allows the solution of the radiative-transfer problem for arbitrary geometries and density distributions. The code contains all the microphysical processes that dominate the heating and cooling of ionized regions and self-consistently treats the transfer of photons through gas and dust. Since the public release of the first version in 2003, MOCASSIN's user base has steadily grown and has been applied in a number of astrophysical contexts, including star-forming regions, protoplanetary discs, dusty supernova remnants, and galaxies. A current version of the code, a manual, and a forum can be found at www.3d-mocassin.net.

Today I would like to concentrate on two recent projects that used MOCASSIN: (*i*) the photoevaporation of protoplanetary discs by X-rays from the central star, and (*ii*) the effect of multi-dimensional radiative transfer in photoionization feedback simulation of high-mass star-forming regions.

Dust and gas discs around young stars are natural by-products of the star-formation process, and they are thought to be the birth-places of planetary systems. Indeed, planets are thought to form from the reservoir of dust and gas held in these discs, whose physical properties therefore set the initial conditions for planet formation. In particular, a protoplanetary disc's lifetime is a crucial parameter as it sets the time-scale over which planets must form. Discs are observed to disappear after only ten million years, much less than the time it would take the material they contain to accrete onto the star (the viscous time-scale). The reason for this early death had remained a mystery as many of the proposed models failed to meet all the observational constraints. We knew that the life and final dispersal of these discs is heavily influenced by radiation (particularly X-ray) from the central young stellar object, but understanding to what extent this is true requires the solution of the multi-dimensional, frequency-resolved radiative transfer within the context of a hydrodynamically and viscously evolving disc.

I am pleased to present our results of the first-ever radiation–hydrodynamical simulations of a protoplanetary disc irradiated by X-ray and extreme ultra-violet radiation from its active young central object. The disc is photoevaporated away by the energetic photons, cutting it down in its prime and hence posing a strict limit to the time available for planet formation. These simulations can fit all current observational constraints and prove that the evolution of the disc and hence the formation and evolution of its planetary progeny are intimately linked to the irradiation history from the central star.

Moving on to larger scales, I would now like to focus on ionization feedback from newly formed high-mass stars and how it may affect (positively or negatively) the formation of future generations of stars. In particular, the development of new methods to provide a more holistic approach to the radiative transfer in large-scale hydrodynamical simulations should allow us in the future to make piecewise comparisons of the theoretical results with observations, hence better exploiting the wealth of new data from current and future ground-based and space missions. The problem with radiative transfer is that it is a very demanding computational task, which has to be performed at each of the many time-steps of a hydrodynamical simulation. While the physics is fairly well understood, the current challenge resides in the development of clever algorithms to capture the physical behaviour of the system without introducing enormous computational overheads that would limit the resolution and hence the predictive power of the simulations. I have joined efforts with experts in hydrodynamics to come up with the next generation of radiation–hydrodynamical algorithms. Our preliminary results in the field of high-mass star-formation feedback show that it is possible to obtain structures that closely resemble those seen in nature, like the Pillars of Creation in M 16, or the Horsehead Nebula, clearly demonstrating the effect of radiation in shaping the interstellar medium.

I conclude with the prediction that radiative-transfer calculations will play an even bigger rôle in computational astrophysics in the future, thanks to the advancements in hardware, but also thanks to the efforts of many researchers to provide faster and more realistic tools that will certainly make an impact in our understanding of many complex astrophysical phenomena.

Professor S. Miller. If I can take you back to the protoplanetary-disc question, you've got a time-scale of roughly five million years in that simulation. So how do your time-scales now fit in with the time-scales from people modelling the formation of planets and then watching them migrate in to form hot Jupiters?

Dr. Ercolano. As far as the time-scales that you saw on our plot are concerned, those absolute time-scales are really completely reliant on the viscosity law that you assume, so there are many things that we actually do not know about how fast the material is moving through the disc. Therefore we can only make assumptions about certain parameters, and so the absolute time-scales that you see there could be a week or a gigayear just by changing that one parameter that we really do not know. What is important in that simulation is the relative time-scale, the time-scale in which the disc appears to be completely optically thick all the way to the dust-destruction radius, compared to the time that is spent being a transition disc which is being evacuated. That *relative* time-scale we fit very well. The *absolute* time-scale can be reconciled with anything, unfortunately. So, this type of simulation won't be able to give you an exact time-scale unless we know a little bit more about the viscosity law, which is very much dependent on the radiative transfer of the X-rays; how fast the material moves through the disc is very dependent on how well-coupled the gas is to the magnetic fields through the disc. Therefore you need to know what the ionization structure in the gas is to understand that coupling. So the other work that we are doing is to try and understand how the so-called magneto-rotation instability (MRI) works, and what type of accretion disc there would be. But I agree with you, the time-scales would have to be worked in tandem with the planet-formation people. And the final thing: it also very much depends on the number of ionizing photons that you use for the central source, and young stellar objects have a factor of 100 difference in X-ray luminosity. So, for a typical X-ray luminosity function, what we claim is that the more powerful X-ray sources will probably not be able to make planets, because they will photoevaporate the disc far too fast. The ones with average X-ray luminosities have a chance, but in fact the low-power X-ray sources are the ones that have the better chances because they live the longest.

The President. Any more questions?

Dr. Stanley. The problem is that you get rid of the disc quite quickly, but you must also address the problem of getting rid of the angular momentum, in that short time-scale, which is quite hard.

Dr. Ercolano. We don't quite understand how the dissipation of angular momentum works; we think there is a turbulence dissipation process which is related to MRI, and it is a slightly different problem in the sense that the proper way to do this would be actually to calculate what the rate of the MRI is, self-consistently within these simulations, but that is very difficult. Nobody has actually done an MRI calculation over a whole disc. People are still just able to simulate only very thin regions — what they call 'shear boxes' — so magneto-hydrodynamic models can only really do very small parts of the disc because they are so computationally intensive. Of course, in the future, you would want to couple these kind of simulations with magnetohydrodynamic simulations and then you would have the right answer, I guess.

The President. Thank you very much! [Applause.]

The President. Our final speaker this afternoon is Professor Paul Feldman, from Johns Hopkins University, and he's going to give us 'A brief spectroscopic tour of the Solar System with the *Far Ultraviolet Spectroscopic Explorer*'.

Professor P. Feldman. The *Far Ultraviolet Spectroscopic Explorer* (*FUSE*) satellite was initially conceived as a successor to the *International Ultraviolet Explorer* (*IUE*), a very successful collaboration between NASA, ESA, and the UK. *FUSE* was launched on 1999 June 26, and operated for over seven years, though not without some loss of capability in its final years. Its spectral band of 905 to 1185 Å, resolving power of ~ 20 000 for point sources, and high sensitivity, provided a unique capability to study the principal atoms, ions, and molecules in a variety of planetary environments. Observations were made of Mars, the Jovian system, Saturn, and four comets. In this talk I present a brief overview of the principal results of these observations and as a specific example, concentrate on the question of fluorescent pumping of molecular hydrogen by solar Lyman-β and Lyman-α, which has enabled the detection for the first time of H_2 in the atmosphere of Mars and in cometary comae.

At the spectral resolution of *FUSE*, individual lines of the H_2 Lyman and Werner band systems are clearly resolved. There is an accidental coincidence in wavelength between the P(1) line of the Lyman (6,0) band at 1025·93 Å and the solar Lyman-β line at 1025·72 Å, whose full width at half maximum is ~ 0·5 Å. At temperatures between 50 and 300 K, most of the H_2 is in the $J = 1$ rotational level, so the pumping is very efficient. Fluorescence is detected in the P(1) lines of the (6,1) band at 1071·62 Å and the (6,3) band at 1166·76 Å (as well as at longer wavelengths outside the *FUSE* spectral band). The observations of Mars were made at a time of increased interest in the history of water on the surface of the Red Planet and the detection of these lines gave conclusive evidence for H_2 in the present-day atmosphere of Mars. The presence of H_2O in the atmosphere was first postulated in 1972 to explain the long-term stability of the CO_2 atmosphere, and current models predict an H_2 column abundance compatible with that derived from the *FUSE* observations. Similar fluorescence was also detected in the spectra of Jupiter, Saturn, and comets. In the case of comets, the H_2 is identified as a dissociation product of H_2O and not as a volatile constituent of the nucleus.

Amongst the objectives of the *FUSE* comet programmes was the search for the presence of atomic argon and O VI in the coma. Argon is an important diagnostic for comet-formation scenarios while the O VI doublet at 1032 and 1038 Å was expected to be the strongest signature in the *FUSE* spectral range of charge transfer of solar-wind ions, the primary source of the soft-X-ray emission seen in recent comets. Four long-period comets were observed by *FUSE*. The comets exhibited an unexpectedly rich emission spectrum that included the (0,0) bands of three CO Hopfield–Birge systems $C^1\Sigma^+ - X^1\Sigma^+$, $B^1\Sigma^+ - X^1\Sigma^+$, and $E^1\Pi - X^1\Sigma^+$, the H_2 lines fluorescently pumped by solar Lyman-β radiation, several multiplets of O I and C I, and ten members of the H I Lyman series. Argon was not detected but there was a large number of initially unidentified lines.

The rotational envelopes of the CO bands were resolved and found to consist of both a cold and a warm component, the cold component (with a rotational temperature of ~ 70 K) accounting for most of the flux. The warm (~ 400 K) component arises from the dissociation of CO_2. Also present was a weak peak centred at the position of the P(40) line of the $C - X$ (0,0) band whose origin is described below.

The O VI doublet was searched for but only the 1032-Å line was apparently detected in two of the four comets observed. Comet C/2001 Q4 (NEAT), observed in 2004 April, provided the highest-signal-to-noise *FUSE* spectrum of a comet, which enabled an accurate determination of the wavelength of the 'O VI' feature and identified it as the H_2 (1,1) Q(3) Werner line, fluorescently pumped by solar O VI. The Q(3) lines of the (1,3) band at 1119 Å and the (1,4) band at 1164 Å were also detected, confirming this identification. However, for the fluorescence to occur it was necessary to have a significant population of H_2 in the $v = 1$, $J = 3$ state, which is not expected at temperatures in the coma. Moreover, all of the initially unidentified lines could be accounted for by excitation from $v = 2$ by solar Lyman-α. As it turns out, laboratory studies had shown that both vibrationally excited H_2 and a non-thermal rotational population of CO, peaked at $J = 40$, are produced by the photodissociation of formaldehyde, H_2CO. The amount of formaldehyde needed to fit the observed spectrum was found to be in good agreement with its abundance derived from infrared and sub-mm ground-based observations of those same comets.

In addition to those molecular studies, the Io plasma torus surrounding Jupiter provided a rich spectrum of sulphur ions in three ionization stages ranging from singly to triply ionized. These included the resonance transitions as well as transitions from the ground ^2D and ^2P states, providing useful diagnostics for the determination of the electron temperature and density. The high spectral resolution and sensitivity of *FUSE* enabled the detection of singly and doubly ionized chlorine in the torus, at an abundance about 1% that of sulphur. Searches for ion emissions from other species such as C, N, Si, and P, all of which have resonance transitions in the *FUSE* spectral band, have all been negative.

Professor Miller. You get the solar-pumped fluorescence lines in the ultraviolet, and you also get solar-pumped fluorescence lines of water in the infrared; I just wondered if you see a correlation between what happens in the UV and in the IR measurements.

Professor Feldman. The IR lines are pumped by solar IR, and there are quite a few measurements. There are recent results from *Herschel* and also from *ODIN* that show very nicely a number of transitions in water, and they are all pumped by sunlight. I can give you some references if you are interested.

Professor Miller. I know the references; I just wondered if there are any correlations between the IR and UV lines.

Professor Feldman. No, we are slightly disadvantaged in that we are usually seeing only dissociation products of water. We see CO and we believe that it is coming predominantly from the nucleus, but everything else — all the atoms and H_2 — we don't believe is. We found that argon is at least 20 to 50 times lower than solar abundance, so again it's volatile and probably disappeared early on in the history of the comet. CO, which is pretty volatile, is a major component in the collapse of discs and the formation of stars and you do expect it to be incorporated in the debris of which the comets are formed.

Professor Carole Jordan. Have you been able to use spectra to get column densities?

Professor Feldman. You mean in the solid part of the comet or in the coma?

Professor Jordan. Well, in the coma.

Professor Feldman. Yes, it works! I didn't go into the details. We calculate the fluorescence efficiency and we find that in the case of CO in fairly active comets we actually have to apply radiative transfer because individual rotational lines saturate and it's very dependent on the rotational temperature, because you have more lines to absorb more of the sunlight. The analysis was published a few years ago by my student, Roxana Lupu.

The President. Thank you very much again. [Applause.] The next time the Society meets is on November 12.

NECROPANSPERMIA

By Paul S. Wesson
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Life on Earth may have been seeded by the arrival from space of radiation-driven grains carrying *dead* micro-organisms like viruses whose *information* content kick-started evolution.

Life might in principle be seeded throughout the Milky Way by astronomical means; but a recent comprehensive review of traditional panspermia confirms that the theory is hardly viable in practice, because micro-organisms are deactivated or killed by radiation in space[1]. However, it will be argued below that this is not necessarily the end of the theory but rather a beginning. The arrival of *dead* micro-organisms, such as viruses, could have provided the essential *information* needed to start the evolution towards complex biological systems on a planet like Earth.

History shows that panspermia is a perennial idea. In 1871, Lord Kelvin suggested, in an address to the British Association for the Advancement of Science, that life could be distributed through space on rocks such as meteorites. In 1908, Arrhenius argued in his book *Worlds in the Making* that the dominant mechanism was the radiation pressure from stars like the Sun, acting on organism-laden dust grains[2]. Many subsequent versions of panspermia were variations on this theme, using various natural phenomena. By contrast, in 1973 Crick & Orgel proposed that life was deliberately seeded in the early Milky Way by an advanced race using spacecraft laden with DNA-rich micro-organisms[3]. More recently, natural processes have again been in focus, and data sets from spacecraft and genome studies have been used to provide the astronomical and biological components of more sophisticated versions of panspermia[4–12].

Despite this continual interest, a problem persists: complex organic molecules such as DNA and RNA are relatively fragile, and easily damaged by electromagnetic radiation (especially ultraviolet) and particle radiation (especially cosmic rays), so it is reasonable to ask why panspermia continues to be discussed. The answer is that without it there is a countervailing problem of even larger proportions: the $4{\cdot}5 \times 10^9$ years age of the Earth is not long enough to allow for the creation of complex biological molecules if they evolve by random chemical processes in normal planetary environments. For example, on the primitive Earth with an appropriate complement of amino acids, random molecular interactions would only produce about 200 bits of information over the time preceding the first fossil evidence of life, which is tiny compared to the 10^5 bits in a typical virus or the 10^7 bits in a typical bacterium. The low gain in information from a number of trials is given by $I = \log_2 N$ assuming binary encoding, so there is simply not enough time to accumulate the information encoded in the genomes of present-day organisms. It is possible, in theory, that the development of life on the Earth may have involved some form of 'directed' evolution. If the change in the information content of a large molecule is proportional to the amount of information already present, there will be an exponential growth in biological complexity, analogous to a runaway reaction in nuclear physics. However, there is no experimental basis for directed or super-fast biological evolution. In short, the life-forms of the present Earth appear to be too complex to have evolved here, hence the recurrent appeal of panspermia.

It is apparent that the main objection to panspermia, namely that micro-organisms cannot survive damage in space, can be circumvented if the view is taken that it is the *information* content of the molecules which is critical, not the question of whether their host organisms are alive or dead. From the informational viewpoint, a broken piece of DNA in a dead organism is as valid as a whole molecule in a living organism. Given this view, two questions arise. First, what is the physical mechanism which transports genetic information around the Milky Way, and was presumably responsible for seeding the Earth? Second, what is the biological nature of the precursor material, and in particular the primitive predecessor of life on Earth?

The first of these questions can be answered with some reliability[1,4,6,9,10]. Radiation pressure from the Sun causes dust grains of 10^{-5}-cm size and smaller to 'leak out' of the Solar System at a speed of order 10 km sec^{-1}. Dust grains of this type would have been particularly plentiful in the early Solar System, by virtue of impacts on the Earth and other planets by asteroids and comets. At the noted speed, grains reach nearby stars in 10^5–10^6 yr, and can populate the optical disc of our Galaxy in a time of order 10^9 yr. Micro-organisms embedded in such grains would be partially shielded from radiation in the originating and destination system, and in interstellar space. Of course, by the time the grains are decelerated into a new system in a reciprocal manner to how they are accelerated out of the original system, the organisms will be dead. But this is, by hypothesis, no longer a fatal objection to the theory. Indeed, the appropriate view of the Milky Way is now one of a cloud of genetically-laced dust, slowly churning under the pressure of its component stars.

The second question posed above, concerning the biological nature of the material which seeds new life, is harder to answer. However, viruses are suitable candidates[1,7,11,12]. They have sizes of 20–300 nm (*i.e.*, of order 10^{-5} cm), are fairly robust, and have nucleic-acid molecular weights of order 10^6–10^8. The identification of viruses as the precursors of more complex life-forms may be surprising to some biologists, who have traditionally assumed the reverse relationship because modern viruses require the medium inside a cell for existence. A closer examination of the physical nature of viruses shows, though, that they could have thrived in the environment of the primitive Earth; and there are viable models of the early viral world[11,12]. It is of course commonly stated that viruses are not really 'alive', because the modern ones are simple in structure and cannot reproduce without a host cell. But it is their primitive nature which makes them attractive as agents in panspermia. The philosophical quandary about what is 'alive' or not is here sidestepped, by reason of the hypothesis that it is *information* which counts, not vivacity.

Panspermia as a theory is certainly more alive than dead. Indeed, with modern data from astrophysics and biophysics, it is viable in a form close to how it was originally conceived by Kelvin and Arrhenius more than a century ago. However, the modern theory has a new twist, forced by the fact that the radiation which powers the dissemination of dust grains also implies the death of any micro-organisms they may carry. Discounting the incidental aspect of whether something is alive or dead, and concentrating on the more pertinent consideration of information, it is quite feasible that the Milky Way has been seeded by pieces of complex molecules like DNA or the bodies of dead viruses. How these might evolve into more lively specimens can be tested by carrying out laboratory experiments under conditions reproducing those of early Earth. It is also possible to test the theory by collecting material directly from space. Such retrieval missions should be undertaken in the *outer* Solar System, because the inner parts are already contaminated by human activity, including the dumping of faeces from manned space missions. A proof of necropanspermia would be the isolation, either in the laboratory or in space, of a complex molecule which can withstand vacuum and radiation but lead in a suitable environment to the emergence of something that is alive.

Acknowledgements

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HIGH-SPEED PHOTOMETRY OF THE ECLIPSING CATACLYSMIC VARIABLE 1RXS J180834·7+101041

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We present high-speed photometry covering one eclipse of the cataclysmic variable system 1RXS J180834·7+101041. The data are modelled as arising from a close-binary system containing a low-mass star filling its Roche lobe plus a white dwarf with an accretion disc around it. We find an orbital inclination of 77°·07 ± 0°·47 and a mass ratio of 0·168 ± 0·016. The relatively low inclination means that only the accretion disc and not the white dwarf is eclipsed by the low-mass star in this system.

Introduction

The *ROSAT* satellite detected a source of X-ray radiation which appears in the *Bright Source Catalogue*[1] under the name 1RXS J180834·7+101041 (hereafter J1808+1010). As part of a systematic search for the optical counterparts of *ROSAT* sources, Denisenko *et al.*[2] obtained time-resolved photometry of the region of sky containing this source, finding strong photometric variability for the star USNO-B1 1001-0317189. Their photometric monitoring lasted for 6·2 hours and exhibits four sharp minima on a period of 0·070037 ± 0·000001 days. The star also showed longer-time brightness variations within the magnitude range 16·2–17·5. Based on these characteristics, Denisenko *et al.* classified J1808+1010 as a cataclysmic variable of possibly AM Her (magnetic[3]) type.

Follow-up observations of J1808+1010 were obtained by Bikmaev & Sakhibullin[4]. Their 11 phase-resolved low-resolution spectra showed strong hydrogen Balmer-line emission and weaker helium-line emission, which is a common characteristic of cataclysmic variables (hereafter CVs). The Balmer emission lines are clearly double-peaked, which is an indicator of a high orbital inclination. Bikmaev & Sakhibullin also presented an *r*-band light-curve covering 2·1 hours at a time resolution of 25 s, which showed a single sharp eclipse event of depth $0^{m}\cdot8$ and total duration 8·6 minutes. From their observations those authors confirmed that J1808+1010 is a CV, but favoured the SU UMa (non-magnetic) subtype of these objects.

CVs are a class of interacting-binary-star systems which display a huge diversity of physical phenomena. The majority of them are composed of a white dwarf and a low-mass and unevolved secondary star, plus an accretion disc through which material passes from the secondary star to the white dwarf. This accretion disc often totally dominates the light of the system, so the only readily observable physical property of most CVs is their orbital periods[5,6]. The importance of eclipsing CVs lies in the information which can be extracted from them: detailed modelling of their eclipses allows one to obtain the basic physical properties of the system, including the masses and radii of the stellar components[7,8]. Such information is valuable in understanding the evolution of CVs, and of other classes of interacting binary system, so we decided to obtain high-speed photometry of J1808+1010 to assess its usefulness for detailed study.

Observations

We observed J1808+1010 at the end of the night of 2009 February 16, starting at UT 05:56 and ending at 07:12. We used the Auxiliary Port focal station of the 4·2-m *William Herschel Telescope*, operated by the Isaac Newton Group on the island of La Palma. A total of 555 observations were obtained through a *V* filter. The detector was a 2148 × 4200-pixel EEV CCD, windowed down to $(2000\ \mathrm{px})^2$ and binned 4 × 4 to minimize the readout time. With an exposure time of 4 s, we achieved a cadence of 9 s. The data were reduced following standard procedures and using a pipeline written in IDL[9,10].

Our data are plotted in Fig. 1, and cover one eclipse of depth $0^{m}\cdot9$ and duration 6·5 minutes. The variation in eclipse depth and duration compared to previous studies can be attributed to changes in the accretion-disc size and structure[11]. The outside-eclipse magnitude was $V \approx 18$, and the system was gradually increasing in brightness throughout our observing sequence. This puts it towards the faint end of the magnitude interval within which J1808+1010 has been seen, but this must be interpreted with caution due to the range of pass-bands used in previous observations. The substantial brightness variations outside eclipse are due to the phenomenon of flickering, which is caused by the stochastic nature of the mass-transfer rate onto the accretion disc or white dwarf[12].

Analysis

A model was fitted to the light curve using the LCURVE code written by T. R. Marsh. This code models a CV as four components: the white dwarf, the secondary star, an accretion disc surrounding the white dwarf, and bright spot where the mass-transfer stream intersects with the edge of the accretion disc. A detailed description of LCURVE can be found in ref. 13. For optimization of the parameters of the model we used the Nelder-Mead downhill simplex[14],

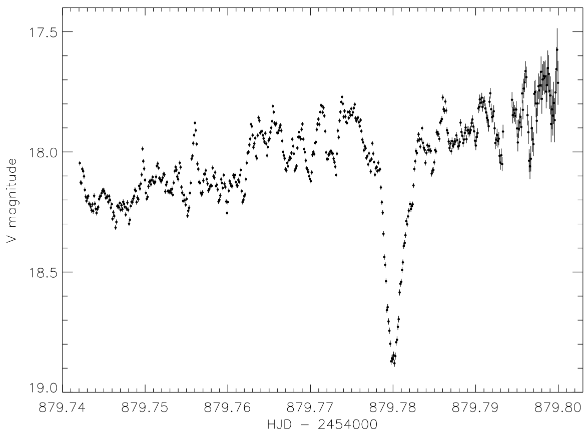

FIG. 1

Plot of the full light-curve obtained for J1808+1010. In most cases the error bars are smaller than the point size.

Levenberg-Marquardt[14], and Markov Chain Monte Carlo methods. LCURVE includes a large number of parameters which are needed to represent the eclipses of CVs observed with very high precision and time resolution[13]. Our data are not of such quality so those parameters were fixed at values which we have found to be physically appropriate in detailed modelling of other eclipsing systems[13,15–18].

In our analysis we found that only part of the accretion disc of J1808+1010 is eclipsed; the white dwarf is not eclipsed at any point in the orbit. This means that the eclipse shape is rather uninformative, and allows us only to constrain the orbital inclination ($i = 77^\circ{\cdot}07 \pm 0^\circ{\cdot}47$), the ratio of the mass of the secondary star to that of the white dwarf ($q = 0{\cdot}168 \pm 0{\cdot}016$), and the midpoint of the eclipse (HJD[UTC] 2454879·27851 ± 0·00006). The best fit (Fig. 2) accurately reproduces the data for most of the eclipse but does not fit the egress of the bright spot well. An improvement in this situation would require similar observations of a larger number of eclipses in order to allow the flickering to be averaged out.

If the white dwarf were eclipsed, the times and durations of the partial phases of the eclipse would allow the radius of the white dwarf to be determined, and therefore the mass and radius of both the white dwarf and secondary star by the imposition of a theoretical mass–radius relation for white dwarfs[7,8]. In the case of J1808+1010 we are unable to obtain these constraints, which means it is not well suited to detailed eclipse-modelling analyses.

Summary and conclusions

1RXS J180834·7+101041 is a cataclysmic variable which is known to be eclipsing with a period of 100·85 minutes. We obtained high-cadence photometry of one eclipse event, finding that the accretion disc is eclipsed but that the white dwarf is not. We have fitted a parametric model to the light-curve, and measured the orbital inclination ($i = 77^\circ{\cdot}07 \pm 0^\circ{\cdot}47$) and mass ratio ($q = 0{\cdot}168 \pm 0{\cdot}016$) of the system. Because the white dwarf is not eclipsed we are unable to obtain additional constraints from our data; we find that J1808+1010 is not a promising system for the study of eclipses in CVs.

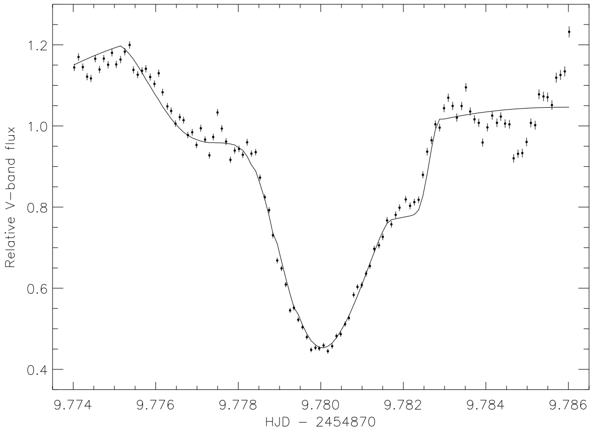

FIG. 2

Comparison between the observed data for J1808+1010 (points with error bars) and the best fit found using the LCURVE code (solid line).

Whilst the current paper was in preparation we became aware of an independent study of J1808+1010 by Yakin *et al.*[19] which presents photometry and spectroscopy of this system. Our light-curve is of substantially better time resolution, and the orbital inclination and mass ratio we find are in agreement with (but more precise than) the values they obtain: $i = 78° \pm 1°{\cdot}5$ and $q = 0{\cdot}18 \pm 0{\cdot}05$. Yakin *et al.* also find that J1808+1010 has an asymmetric brightness distribution in its accretion disc (see ref. 20 for an extreme example of this phenomenon). From their observations and from some simple relations Yakin *et al.* infer the masses of the two stars to be $0{\cdot}8 \pm 0{\cdot}22$ and $0{\cdot}14 \pm 0{\cdot}02$ $M_\odot$ for the white dwarf and secondary component, respectively.

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SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 217: HD 159220, HD 211922, HD 212859, AND HD 219726

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Interest in the four stars arose from the extension, described in the immediately preceding paper in this series, of the 'Clube Selected Areas' programme. HD 211922 and HD 212859, only, are actually Clube stars; the other two are merely stars in the same fields as genuine Clube ones (about 3′ separation). A feature that they all have in common is a short period — their periods are about 2·0, 12·1, 6·5, and 4·5 days; all the orbits are circular or very nearly so, except (probably) that of HD 212859. The high rotational velocity of the late-F star HD 159220 suggests that the star is seen equator-on, so the orbit too is likely to be of high inclination; a watch should be made for eclipses, for which an ephemeris is given. HD 211922, although classified K0 in the *Henry Draper Catalogue*, is actually an Am star. HD 212859 is a triple system, since it is a close visual binary as well as having a short-period spectroscopic orbit; the object is seen and measured as if it were a single star, so the orbit (particularly its amplitude) will require revision in the future when the visual components can be measured separately. The rotational velocity of HD 219726 indicates a minimum radius of 1·68 $R_\odot$, larger than a star of its putative type of F5 is supposed to have. Possibly its rotation is not synchronized with the orbit; otherwise, either it must be starting to evolve or else it is really a metallic-lined star of somewhat earlier type.

Introduction

Paper 216[1] described the supplementation of the 'Clube Selected Areas' programme in the northern hemisphere by increasing the sizes of the Areas so as to embrace a lot of additional stars, while still maintaining the same selection criteria of type K0 and $m_{pv} = 9^{m}{\cdot}0 \pm 0^{m}{\cdot}5$ in the *Henry Draper Catalogue*. Naturally enough, some of the newly added stars have turned out to be spectroscopic binaries. Among them are HD 211922 and HD 212859, in Area 10, nominally centred at $l = 90°$, $b = -35°$, corresponding roughly to RA = 23^{h}, $\delta = +21°$, in Pegasus. The other two stars treated in this paper were first observed just because they were near to Clube stars — in one case by mistake, but that object would probably have been measured in any case because the observer's curiosity often impels him to observe other stars that he sees in the fields of programme stars. In fact some of the most interesting objects reported in this series of papers, including the triple-lined objects HR 2879 B[2] and HR 8082 B[3], and ϕ Psc B[4], which caused much trouble because its orbital period is so very close to two sidereal days, were discovered in that way. One of the two 'pseudo-Clube' stars discussed here, HD 219726, is in the field of HD 219713, another Area-10 star. The other, HD 159220, is a companion to HD 159182, in Area 2, which is mainly in Draco, centred at $l = 90°$, $b = +35°$ (RA ~ 17^h 30^m, δ ~ +60°). Except for HD 219726, which is close to one of the 'original' Clube stars[5], the actual stars with which this paper is concerned are a considerable distance away from the nominal centres of their respective Areas — they are necessarily outlying stars since they were brought within the programme only by the recent extension of those Areas. Of the four, the only one that was observed by *Hipparcos* is HD 211922, but we are indebted to *Tycho* for the V, $(B - V)$ photometry of the others too.

This series of papers has been more apt to document orbits of long periods than of short ones; the fact (illustrated in the *Synopses* presented after every fiftieth paper, most recently[6] in 2008) is that the distribution of periods of (even spectroscopic) binaries is a rising function of period. Despite that, however, until radial velocities were routinely measured by the photoelectric method[7] the great majority of known orbits[8] were of short periods, purely as a result of observational selection. Among the 356 orbits so far presented in this series of papers, only 40 have had periods as short as the longest (12 days) of those given here, and many of those 40 were not random discoveries like the present four but were specifically brought to attention for other reasons (*e.g.*, chromospheric activity, Am classification, eclipses) that could lead to an actual *expectation* of a short period.

HD 159220

The first observation of this star, which is to be found in northern Hercules about 2° preceding ι Her, was made by mistake in 2002. The Clube stars HD 159138 and 159182 are separated by about 4′ in a south-preceding/north-following configuration; HD 159182 and 159220 are 3′ apart in a comparable position angle, and were mistaken for the former pair. The radial-velocity trace of what was thought to be HD 159182 and was really 159220 showed a very wide and 'feeble' dip, such as might be given by a rapidly-rotating F-type star. Not a few of the supposedly-K0 Clube stars are really F types, so the nature of the trace did not flag up the mistaken identity, which came to light only in the following year when the two genuine Clube stars were re-observed; that said, however, the *HD* type of HD 159220 is in fact F8. The sort of trace given by HD 159220 did not encourage further interest in that star, but another observation was made, again by mistake, in 2006. It differed by more than 70 km s^{-1} from the first one! Of course that *did* arouse interest, so the star was transferred to the spectroscopic-binary programme and its 2-day period was quickly established. There are now 30 radial-velocity measurements, well distributed in phase; they are listed in Table I and give the orbital elements that are set out in Table V (below, with those of the other three stars after they too have been described). There is little likelihood of, and no statistical support in the observations for, a non-circular orbit, and the zero-eccentricity solution has been unhesitatingly adopted; it is plotted in Fig. 1.

Simbad knows of no papers specifically referring to HD 159220, but the *Tycho* photometry gave its V magnitude as $9^m \cdot 05$, and $(B - V) = 0^m \cdot 54$. The colour index confirms the inference from the character of the radial-velocity traces that HD 159220 is of F type, and the very short orbital period guarantees that it is a dwarf. Its type may be estimated at F8 V and its distance modulus at just under five magnitudes, so as a hostage to future fortune the estimate of its

TABLE I

Cambridge radial-velocity observations of HD 159220

Date (UT)	*MJD*	*Velocity* $km\ s^{-1}$	*Phase*	*(O–C)* $km\ s^{-1}$
2002 Sept. 10·962	52527·962	+18·4	0·132	+0·3
2006 July 21·034	53937·034	−53·9	697·531	−0·1
2007 Aug. 5·979	54317·979	+26·6	886·074	−1·0
6·964	318·964	−49·5	·562	+1·9
7·072	319·072	−44·5	·615	−0·7
9·907	321·907	+30·9	888·018	−1·1
10·073	322·073	+21·6	·100	−2·3
10·900	322·900	−58·1	·510	−3·5
13·029	325·029	−52·4	889·563	−1·2
Sept. 7·938	350·938	−44·1	902·387	0·0
12·955	355·955	+16·7	904·870	−1·8
15·836	358·836	−21·3	906·296	+2·2
22·878	365·878	−2·3	909·781	+0·5
24·828	367·828	−10·8	910·746	+1·5
29·892	372·892	−12·4	913·253	−0·5
Oct. 4·828	377·828	−24·2	915·696	+1·6
17·886	390·886	+13·9	922·158	+1·4
20·909	393·909	−36·9	923·655	−1·2
2008 July 1·047	54648·047	−49·5	1049·436	+1·7
13·000	660·000	−38·1	1055·352	−0·9
21·024	668·024	−30·8	1059·324	−0·2
22·046	669·046	+10·4	·829	+0·8
24·014	671·014	+3·4	1060·803	+0·3
Aug. 12·972	690·972	−29·5	1070·681	−0·1
2010 May 17·108	55333·108	−54·5	1388·497	+0·1
18·056	334·056	+31·3	·966	0·0
19·103	335·103	−54·8	1389·484	−0·4
June 3·085	350·085	+25·0	1396·899	+1·1
28·023	375·023	−9·2	1409·242	−0·2
July 6·042	383·042	+0·9	1413·211	+1·6

parallax is given as just over 0″·01. The mean value of $v \sin i$ from the radial-velocity traces is 30·0 ± 0·4 km s^{-1}, which, with the orbital period of 2·02 days and the near-certainty that the period of axial rotation is the same, leads to a projected radius, $R_1 \sin i$, of 1·20 $R_\odot$. That is at least as great as the radius to be expected of a late-F dwarf, so the implication is that $\sin i \sim 1$. (We could be misled, however, if the star is starting to evolve and expand.) The mass function demands a minimum mass of about 0·35 $M_\odot$ for the unseen secondary if the primary's mass is taken as 1·2 $M_\odot$, so if indeed $\sin i \sim 1$ it fixes the mass of that star at close to the lower limit of 0·35 $M_\odot$, making it likely that the companion object is an M dwarf with a type of M2 or M3.

A system having an orbit of such a short period and probably of high inclination could easily prove to be eclipsing. The radius of the putative early-M secondary may be estimated at $R_2 = 0{\cdot}5\ R_\odot$. The separation of the stars, whose masses differ (according to the discussion in the last paragraph) by a factor q of about 3·4, is given by $a_1 + a_2 = (1 + q)(a_1 \sin i)/\sin i$, which we see from the orbital elements in Table V is 4·4 × 1·2/sin i Gm — about 5·3/sin i Gm, or 7·6 $R_\odot$ if we accept that $\sin i \sim 1$. Eclipses will occur if the distance, projected onto the 'plane of the sky', between the centres of the stellar discs is less than $R_1 + R_2$, or

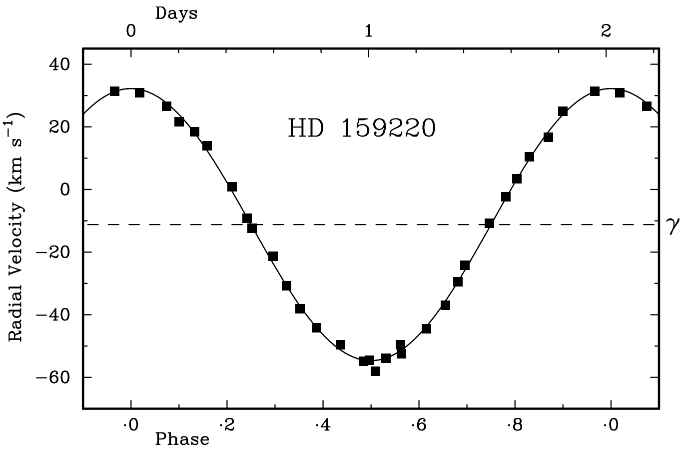

FIG. 1

The observed radial velocities of HD 159220 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. All the observations were made with the Cambridge *Coravel*.

about 1·7 $R_\odot$. That will occur if sec $i \gtrsim$ 7·6/1·7 or about 4·5, leading to $i \gtrsim 77°$. At that limit, eclipses would be grazing. They would become total (with the primary in front, at orbital phase ·75), or 'annular' (*i.e.*, transits in which the secondary is projected fully within the limb of the primary, at phase ·25) if the projected separation of the stars is less than $R_1 - R_2$ or about 0·7 $R_\odot$, requiring $i \gtrsim 85°$. According to the parameters adopted here, a central eclipse would be total or annular while the stars moved relatively to one another by $2(R_1 - R_2)$ at a relative transverse velocity $K_1(1 + q)$ — in round numbers, 1·0 Gm at 190 km s^{-1}, taking about an hour and a half. The partial phases would each last about an hour, giving an overall duration of 3½ hours for a central event.

Eclipses at phase ·75 in the spectroscopic orbit are of the secondary star by the primary, and would scarcely be observable except perhaps in the infrared: if the above proposal for the nature of the secondary is near the truth, the stars differ in V by about six magnitudes, so the drop in the combined brightness during even a total eclipse would be only about 0^{m}·004. At the other conjunction, in an 'annular' transit where the secondary would be seen fully projected in front of the primary, its supposed 0·5-$R_\odot$ disc would obscure about 17% of the primary's 1·2-$R_\odot$ one, giving a drop of about 0^{m}·20 which ought to be easy to observe.

The only existing information that we can press into service to search for eclipses comes from *Tycho*, and is very much less accurate than photometry from *Hipparcos* itself, which unfortunately did not observe HD 159220. When the *Tycho* photometry is folded on the orbital period, it appears to exhibit an exciting minimum at one particular phase — the five lowest points out of the 158 are all within a small range of phase around $\phi \sim$ ·31. Extrapolating the timing of the spectroscopic orbit back to the *Hipparcos* era some 17 years or 3000 orbital periods earlier, there is a 1-σ uncertainty of about 0·034 days or 0·017 cycles in terms of phase, so the discrepancy in phasing is three or four standard errors — unacceptable to a statistician, but perhaps marginally plausible when the uncertainty of the standard error itself is taken into account.

A more sober assessment of the photometric situation, however, rather dispels the initial excitement occasioned by the existence of the low points rather near the correct phase. First, one could notice that they are a whole magnitude low, whereas we can hope only for a drop of about $0^{m}\cdot2$. Then, it comes to attention that there are seven other points that are *not* particularly low in the same small range of phase — although they might still be consonant with a $0^{m}\cdot2$ drop, such is the scatter of the *Tycho 2* photometry. The *coup de grâce* comes when one looks at the detailed table of the photometry and unpacks the 'bit flags' at the ends of the lines recording the low points, all of which date from the same epoch: according to my reading of them, they are saying that in the relevant entries the magnitudes were not measured at all but represent 'non-detections'! Thus in the end we see the *Tycho* photometry in this instance as a 'broken reed'[9], and no decision is possible as to whether eclipses occur. They certainly would be worth watching for. Their times are given by MJD $= T_0 + (n + ¼)P$, where n is any integer; it will be decades before the timing uncertainty reaches an hour.

There is a fainter star (TYC 3509-866-1), measured by *Tycho* as $V = 10^{m}\cdot71$, $(B - V) = 0^{m}\cdot70$, $3'\cdot25$ north-following HD 159220 in position angle 58°; its radial velocity was measured on 2010 June 28·02 and Oct. 6·81 with results of +14·6 and +14·8 km s^{-1}, respectively.

HD 211922

HD 211922 ($V = 9^{m}\cdot34$, $(B - V) = 0^{m}\cdot34$), is to be found about 5° preceding, 1° north of η Peg. Although of *HD* type K0, it was found by Bidelman[10], in the only paper known to *Simbad* regarding the star, to be of Am type. Bidelman has a terse note, "HD type in error.".

The first two radial-velocity observations, in 2002 and 2003, were in tolerable agreement, but when a third measurement was made, in 2005, it disagreed by nearly 60 km s^{-1}; only a fortnight later the form of the orbit was known and its 12-day period determined. There are now 34 velocities, listed in Table II; they yield the orbit whose elements are given in Table V below and which is portrayed in Fig. 2. That non-circular orbit was selected, after some consideration, in preference to the circular solution. The former has an eccentricity of 0·0114 ± 0·0033 — so it is about 3·5 times its standard error. The sum of squares of the residuals is 7·24 (km $s^{-1})^2$, against 10·33 for the circular solution. Utilized in Bassett's[11] second statistical test, those figures yield a variance ratio, $F_{2,28}$, of 5·98, which is appreciably above the 1% significance level of 5·45.

The mass function demands a mass of least 0·8 $M_\odot$ for the companion star if the primary is supposed to be about 2 $M_\odot$; the companion, therefore, if it is a main-sequence star as opposed to a white dwarf, must be no later in type than about K0 and must be within about four magnitudes of the brightness of the primary. It may well be observable in the infrared, or even in the visible if a spectrum were obtained with good S/N. It is not obvious in the radial-velocity traces, and the orbit shows no sign of the residuals of observations made near the conjunctions being biassed towards the γ-velocity. Its non-appearance might be seen as evidence that the orbital inclination is high and that the secondary star, therefore, is close to the minimum mass. The primary is rotating with a mean $v \sin i$ of 11·2 ± 0·3 km s^{-1}; if it were synchronized with the 12·1-day orbital period the implied projected stellar radius would be 2·7 $R_\odot$. That is rather larger than an A-type star is normally supposed to be; it seems a bit improbable that the rotation would be just a little (about a third) faster than the rate corresponding to synchronism, and another possibility is that the star has begun to evolve and really *is* somewhat larger than a normal main-sequence A star.

TABLE II

Cambridge radial-velocity observations of HD 211922

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O – C)* *km s*$^{-1}$
2002 Sept. 29·899	52546·899	+25·0	0·560	+0·2
2003 Nov. 3·864	52946·864	+26·0	33·571	−0·6
2005 Aug. 26·026	53608·026	−32·5	88·139	+0·5
27·054	609·054	−35·3	·224	−0·6
Sept. 2·973	615·973	+31·4	·795	−0·7
7·017	620·017	−31·7	89·129	+0·3
7·976	620·976	−35·0	·208	+0·2
9·030	622·030	−28·8	·295	−0·4
12·906	625·906	+33·0	·615	+0·2
14·994	627·994	+33·8	·787	+0·7
17·017	630·017	+1·0	·954	+0·5
17·968	630·968	−18·1	90·033	−1·0
20·968	633·968	−29·6	·280	+0·6
23·966	636·966	+18·5	·528	−0·2
28·060	641·060	+20·7	·866	+0·3
Oct. 4·978	647·978	−0·9	91·437	+0·3
2006 Sept. 20·056	53998·056	−22·5	120·330	+0·6
20·994	998·994	−7·8	·407	−0·1
Oct. 26·944	54034·944	−15·5	123·374	−0·8
Nov. 25·820	064·820	+24·5	125·840	−0·7
Dec. 9·811	078·811	−8·8	126·995	+0·2
2007 July 27·100	54308·100	+8·8	145·919	+0·1
Aug. 10·030	322·030	−23·6	147·069	+0·3
Sept. 8·035	351·035	+5·3	149·463	+0·6
Oct. 28·953	401·953	+37·4	153·665	+0·2
2008 Sept. 19·010	54728·010	+27·7	180·576	+0·3
Oct. 1·924	740·924	+35·2	181·642	−0·4
31·926	770·926	−31·3	184·118	−0·4
2010 Aug. 24·078	55432·078	+37·6	238·685	−0·3
Sept. 3·027	442·027	+14·1	239·506	−0·2
Oct. 11·958	480·958	+38·6	242·720	+0·6
20·061	489·061	−11·7	243·388	+0·1
Nov. 15·873	515·873	+31·1	245·601	0·0
23·851	523·851	−32·7	246·260	−0·3

Not being sure whether the inclination is really high, we cannot say how strong a possibility there is of eclipses, but they certainly cannot be ruled out. The ephemeris for the relevant conjunctions is the same as that given in the preceding section, except of course for the substitution of the parameters T_0 and P relevant to HD 211922. As it is not clear what radius should be assigned to the primary, we cannot be as definite as in the previous case concerning the character expected for an eclipse, but we can say that a central transit would create an eclipse at least $0^{m}\cdot1$ deep, and might be 'total' for about 4 hours with partial phases lasting about 2½ hours on either side of it.

HD 212859

This star is about 5° south-preceding η Peg, and has the magnitudes $V = 8^{m}\cdot95$, $(B - V) = 1^{m}\cdot11$. Like the others stars that feature in this paper, it must be a

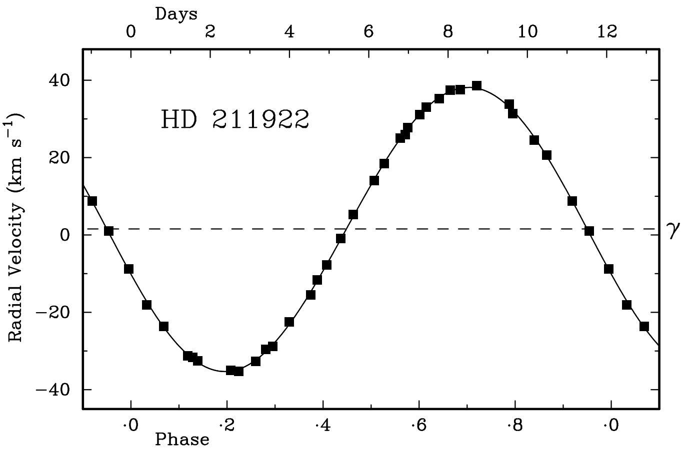

FIG. 2
As Fig. 1, but for HD 211922.

main-sequence object, otherwise it could scarcely have such a short period as is actually found for it. A complication in this case is that the star is a close visual binary; it eluded the observers in the golden age of double-star discovery, but succumbed in 1970 to the scrutiny of Couteau[12] with the 50-cm Nice refractor. Couteau, who gave it his 'discovery number' 539, saw it as a pair with a Δm of $1^{m}\cdot 0$ and an angular separation of $0''\cdot 55$. There has been no certain orbital motion since discovery; Couteau's original Δm has been largely confirmed from re-reductions of *Tycho* observations[13], although some of the visual observers[14–16] have given larger values.

There was a discordance of 1·9 km s^{-1} between the first two Cambridge radial velocities, obtained in 2002 and 2003 — more than there ought to have been if there were no real change. Ensuing measurements were equally ragged, but no more so. Eventually, in 2005 August an intensive campaign was launched in an effort to see what was really going on, and on September 20 of that year the 6½-day period was divined. The period was surprisingly short in relation to the small amplitude of the velocity variation, and clearly implies a companion object of very low mass and/or a very low orbital inclination. The total number of radial velocities now available is 58; they are listed in Table III and produce the elements given in Table V below. The orbit is illustrated in Fig. 3.

With this orbit, as with the last one, there is a question as to whether the eccentricity is non-zero. In the present case the eccentricity given by the 'free' solution is 3½ times its standard error, and the sum of squares of the residuals is reduced to 2·71 (km s^{-1})^2 from the 3·35 given by a solution with zero eccentricity forced upon it. The difference of 0·64 represents the effect of granting the solution an extra two degrees of freedom, which have evidently cost 0·32 (km s^{-1})^2 each, while the other 2·71 (km s^{-1})^2 represent the 52 degrees of freedom (58 observations solved for six unknowns) left in the 'free' solution — so they cost 2·71/52 or 0·052 (km s^{-1})^2 apiece. The ratio 0·32/0·052 is 6·14, which is the variance ratio that constitutes Bassett's[11] second statistical test for

TABLE III

Cambridge radial-velocity observations of HD 212859

Date (UT)	MJD	Velocity km s^{-1}	Phase	(O–C) km s^{-1}
2002 Sept. 29·92	52546·92	−31·9	0·903	−0·2
2003 Nov. 3·88	52946·88	−30·0	62·601	+0·1
5·84	948·84	−31·4	·904	+0·3
7·83	950·83	−30·5	63·211	0·0
Dec. 17·81	990·81	−29·3	69·378	+0·6
2004 Jan. 29·77	53033·77	−32·3	76·005	−0·5
Aug. 19·04	236·04	−30·4	107·207	+0·1
Sept. 14·01	262·01	−30·5	111·213	0·0
Oct. 18·94	296·94	−30·2	116·602	−0·1
21·98	299·98	−31·4	117·071	+0·1
Nov. 4·89	313·89	−30·6	119·216	−0·1
Dec. 16·76	355·76	−30·0	125·675	+0·4
2005 Jan. 13·74	53383·74	−32·1	129·991	−0·3
June 28·09	549·09	−29·9	155·498	0·0
July 18·07	569·07	−30·1	158·581	−0·1
Aug. 7·10	589·10	−30·5	161·670	−0·1
15·09	597·09	−31·4	162·903	+0·3
26·05	608·05	−30·2	164·594	−0·1
27·06	609·06	−30·8	·749	0·0
Sept. 2·98	615·98	−31·5	165·817	−0·2
7·02	620·02	−29·9	166·440	−0·1
7·99	620·99	−30·1	·590	0·0
12·91	625·91	−30·1	167·349	−0·2
15·00	628·00	−30·2	·671	+0·2
17·02	630·02	−31·7	·983	+0·2
17·96	630·96	−31·2	168·128	−0·1
20·97	633·97	−30·1	·592	0·0
23·97	636·97	−31·4	169·055	+0·2
28·07	641·07	−30·7	·687	−0·2
29·05	642·05	−31·6	·839	−0·2
Oct. 1·93	644·93	−30·4	170·283	−0·3
4·98	647·98	−30·9	·753	0·0
Nov. 18·85	692·85	−30·3	177·675	+0·1
2006 Sept. 9·04	53987·04	−31·4	223·057	+0·2
11·00	989·00	−30·0	·359	−0·1
19·97	997·97	−30·8	224·743	0·0
20·98	998·98	−31·7	·899	0·0
23·01	54001·01	−30·6	225·212	−0·1
Oct. 5·01	013·01	−32·0	227·063	−0·4
26·95	034·95	−29·9	230·447	−0·1
Nov. 1·94	040·94	−29·8	231·372	+0·1
2·89	041·89	−30·0	·518	−0·1
3·86	042·86	−30·3	·668	+0·1
25·83	064·83	−31·0	235·057	+0·6
Dec. 2·81	071·81	−31·1	236·134	0·0
3·80	072·80	−30·0	·286	+0·1
9·82	078·82	−30·7	237·215	−0·2
16·82	085·82	−30·1	238·295	0·0
2007 Aug. 7·05	54319·05	−29·8	274·273	+0·4
Sept. 16·02	359·02	−29·7	280·439	+0·1
Oct. 4·98	377·98	−30·1	283·363	−0·2

TABLE III (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2008 Oct. 1·93	54740·93	−29·8	339·352	+0·1
8·98	747·98	−29·9	340·440	−0·1
31·93	770·93	−32·3	343·980	−0·4
2009 Sept. 4·98	55078·98	−29·6	391·500	+0·3
21·04	095·04	−31·8	393·978	+0·1
Oct. 8·94	112·94	−30·8	396·739	0·0
2010 Sept. 15·97	55454·97	−29·8	449·501	+0·1

circularity, and is to be compared with entries for $F_{2,52}$ in tables of significance, *e.g.*, ref. 17, which show that the 1% level is passed at 5·05. The eccentric solution accordingly receives substantial support and is adopted here, although the variance-ratio criterion falls considerably short of the level ($F_{2,52} \sim 7{\cdot}95$) required for significance at the 0·1% level.

According to the *Tycho* photometry, HD 212859 is considerably redder than a star of type K0 V; to model its colour index with a pair of main-sequence stars differing by $1^{m}{\cdot}0$ in absolute magnitude requires components of about K4 and K7 V, with absolute magnitudes of approximately 7 and 8, respectively. Their combined absolute magnitude would be nearly $6^{m}{\cdot}6$, leading to a distance modulus of only $2^{m}{\cdot}3$, corresponding to a distance of 29 pc. At that distance the angular separation of $0''{\cdot}55$ represents a projected separation of about 16 AU — about half-way between the radii of the orbits of Saturn and Uranus in the Solar System. If the projected separation of the binary were near to the actual separation, then in the 40 years since discovery the system could be expected to have accomplished something like half a revolution; instead of that, it has not shown any certain movement at all, a fact that the writer finds puzzling. The only type of orbit that he can visualize to hold the components relatively almost

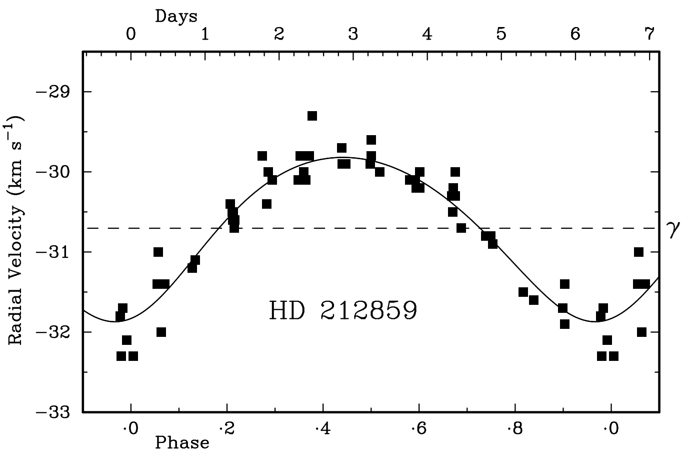

FIG. 3
As Fig. 1, but for HD 212859.

stationary for such a long time is one of high eccentricity with its major axis aligned nearly in the line of sight. Of course the relative fixity would be no more than natural if the system were to consist of a pair of giants, but it would be hard to explain how one of them could be involved in a 6½-day orbit, and their annual proper motion of about 0″·06 would then correspond to an improbably high transverse velocity of over 100 km s^{-1}.

HD 212859 looks like a single star as its image sits on the ~1″ entrance slit of the radial-velocity spectrometer. It is not clear which component of the visual system is the spectroscopic binary. The real amplitude of the velocity variations must be greater than the observed one, by approximately the inverse of the share of the total luminosity of the system that is contributed by the spectroscopic binary. The stars differ by about one magnitude — a factor of 2½. Thus if the visual primary is the spectroscopic system, the observed amplitude would need to be multiplied by 3½/2½ and would become 1·44 km s^{-1}, whereas if it is the secondary that varies then the multiplication factor would be 3½, giving the true amplitude as 3·6 km s^{-1}. Since the mass function shows the mass of the spectroscopic secondary to be in any case very small in comparison with that of its primary, it effectively gives a value for $m_2 \sin i$. The 'raw' value is about 0·007 $M_\odot$; if the visual primary is the culprit then its companion's mass is about 0·010/$\sin i\, M_\odot$, whereas if it is the visual secondary then the companion is about 0·025/$\sin i\, M_\odot$. In either case, the likelihood is that the companion is an object of sub-stellar mass.

The mean $v \sin i$ values measured from the radial-velocity traces of HD 212859 is 3·75 ± 0·22 km s^{-1}, although the formal standard error is surely smaller than the true uncertainty. We have to remember, too, that the spectra from which the velocities are measured are the superposition of the spectra of two stars whose brightnesses are considerably unequal and whose velocities are probably not identical, and we do not even know which of the stars has the variable velocity. Also, it is by no means assured that the primary star in the spectroscopic system will rotate in synchronism with the orbit of an associated object of such probably-small mass. It is perhaps 'grasping at straws' to think that any light might be thrown on the problems that remain with HD 212859 by seeing whether the measured $v \sin i$ values show any systematic variation with the radial velocities derived from the corresponding traces — and so it proves: they don't!

HD 219726

HD 219726 lies about 2′·9 distant in position angle 58° from the Clube star HD 219713, whose parallax and colour found by *Hipparcos* indicate it to be indeed a K0 star, with the luminosity of a typical giant. The two stars are to be found about 5° north-following α Peg, and are projected against the field of the Abell[18] 2572 group or cluster of galaxies. In fact the only paper that *Simbad* knows to refer to HD 219726 is one[19] about an X-ray survey of the A 2572 region, in which the star is identified as a G0 object, which is how it is listed in the *HD*. The X-rays from it are said probably to represent coronal emission, a suggestion that is endorsed now by the discovery that the star is the rapidly rotating primary in a binary of very short period. *Tycho* has provided the magnitude and colour index of HD 219726 as $V = 8^{m}{\cdot}95$, $(B - V) = 0^{m}{\cdot}41$, so its spectral type is probably appreciably earlier than is given in the *Henry Draper Catalogue*. The colour is appropriate to a type of F5 V, which is in fact exactly the type that was noted as probable by the writer on sight of the first Cambridge radial-velocity trace of the object.

TABLE IV

Radial-velocity observations of HD 219726

All observations were made at Cambridge, except the first, which was made with the OHP Coravel

Date (UT)	MJD	Velocity km s^{-1}	Phase	(O–C) km s^{-1}
1997 Sept. 15·015	50706·015	+21·4	0·835	−1·4
2002 Oct. 5·006	52552·006	+22·4	409·832	+0·4
2004 Oct. 26·909	53304·909	−29·7	576·645	+0·6
Nov. 13·894	322·894	−33·1	580·630	+0·6
14·913	323·913	+29·1	·855	+1·4
19·771	328·771	+41·7	581·932	0·0
26·808	335·808	−48·7	583·491	−0·2
26·912	335·912	−48·6	·514	−0·2
Dec. 17·821	356·821	+27·3	588·146	0·0
19·830	358·830	−41·9	·592	−1·0
20·896	359·896	+19·6	·828	−1·3
2005 Aug. 15·117	53597·117	−37·2	641·386	−0·2
Sept. 8·053	621·053	−19·5	646·690	−0·7
16·988	629·988	−23·6	648·669	+0·7
21·032	634·032	−44·7	649·565	−0·1
24·002	637·002	+7·0	650·223	+0·4
Oct. 1·945	644·945	+46·4	651·983	+0·7
5·008	648·008	−24·7	652·662	+1·5
Nov. 17·849	691·849	−35·1	662·375	−0·4
2006 Sept. 9·063	53987·063	+8·7	727·782	+0·4
19·955	997·955	+14·8	730·196	+0·3
21·034	999·034	−44·7	·435	−0·1
Oct. 3·074	54011·074	+35·3	733·102	−1·2
2007 Sept. 11·997	54354·997	−16·9	809·302	−0·5
Oct. 21·982	394·982	+23·7	818·161	−0·1
Nov. 16·858	420·858	+36·1	823·894	+0·3
2008 Oct. 21·997	54760·997	−2·4	899·255	+0·3
2009 Aug. 18·099	55061·099	−2·4	965·745	+0·3
Sept. 5·071	079·071	−8·5	969·727	−0·5
Oct. 25·968	129·968	+46·0	981·004	+0·1
Nov. 17·875	152·875	+40·9	986·079	+0·7
Dec. 22·789	187·789	+16·7	993·815	−0·7
2010 Aug. 24·125	55432·125	+43·4	1047·950	−0·2
28·116	436·116	+22·4	1048·834	−0·1
Sept. 1·089	440·089	−12·1	1049·714	−0·3
15·019	454·019	+13·4	1052·800	0·0
21·988	460·988	−27·0	1054·345	+0·7

The first observation of HD 219726 was made at Haute-Provence in 1997, quite deliberately; the second was made at Cambridge in 2002, when the star was mistaken for HD 219713. The fact that those two observations were of the same object as one another did not come to light until later, when both stars were re-observed repeatedly at Cambridge and the rapid variations in the velocity of the former were recognized. The number of observations is now 37; they are listed in Table IV, and yield the orbit that is shown in Fig. 4 and whose elements are given, along with those of the other three stars already described here, in Table V.

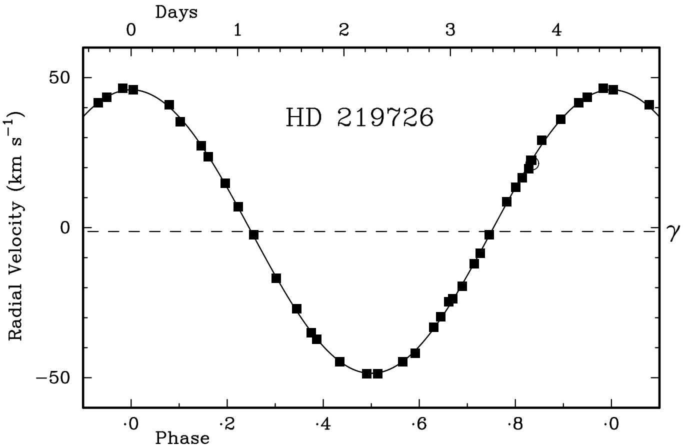

FIG. 4

As Fig. 1, but for HD 219726 and with the exception that there is one observation from the OHP *Coravel*; it is plotted with an open circle.

The radial-velocity traces show wide dips, which enable the $v \sin i$ to be quantified as 18·9 ± 0·3 km s^{-1} in the mean. Multiplying that rate by the orbital period gives the projected circumference of the star, on the very probable assumption that the rotation is synchronized to the orbit; the projected radius is 1·68 $R_{\odot}$, which is larger than a star of the putative F5 type is supposed to have. There is no getting away from the observational fact, which might be explained (otherwise than by non-synchronism) either by supposing that the star has made a significant start on its evolution towards the giant branch of the H–R diagram or else that it is actually a metallic-lined star of an earlier type than that with which we have credited it.

TABLE V

Orbital elements for the four stars

Element	*HD 159220*	*HD 211922*	*HD 212859*	*HD 219726*
P (days)	2·020468 ± 0·000011	12·11623 ± 0·00012	6·4825 ± 0·0006	4·513461 ± 0·000010
T_0 (MJD)	54538·0602 ± 0·0026	54147·925 ± 0·007*	53821·08 ± 0·04†	54033·1794 ± 0·0023
γ (km s^{-1})	−11·20 ± 0·26	+1·55 ± 0·09	−30·70 ± 0·03	−1·29 ± 0·12
K_1 (km s^{-1})	43·45 ± 0·39	36·74 ± 0·12	1·03 ± 0·04	47·25 ± 0·17
e	0	0·0114 ± 0·0033*	0·14 ± 0·04†	0
$a_1 \sin i$ (Gm)	1·207 ± 0·011	6·120 ± 0·021	0·0905 ± 0·0039	2·932 ± 0·011
$f(m)$ ($M_{\odot}$)	0·0172 ± 0·0005	0·0624 ± 0·0006	0·00000070 ± 0·00000009	0·0494 ± 0·0005
R.m.s. residual (km s^{-1})	1·30	0·56	0·22	0·65

*T = MJD 54151·6 ± 0·6; ω = 108 ± 18 degrees †T = MJD 53824·61 ± 0·32; ω = 196 ± 17 degrees

Acknowledgement

I am very grateful to Dr. Brian Mason of the USNO for promptly sending all the visual-binary data for HD 212859 from the *Washington Double-Star Catalogue*.

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NEPTUNE COMPLETES ITS FIRST REVOLUTION SINCE DISCOVERY

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In 2011 Neptune will complete its first heliocentric revolution since discovery. Topocentrically, it passes its discovery position five times in 2010/11. At closest in 2011 it will pass within 33″ of its discovery position.

Introduction

Since 1846 it has been a source of chagrin to English astronomers that Neptune was not discovered here, but by Galle and d'Arrest in Berlin. Two opportunities to observe Neptune close to its discovery position will occur in the autumn of 2011.

Galle had received a letter from Le Verrier on 1846 September 23 asking him to search the predicted position for a new planet. Galle sought permission from Encke, the Director of the Berlin Observatory, to make the search. D'Arrest, then a young student, asked to be allowed to assist. The discovery was reported by Encke[1] and thirty years later Galle[2] gave his own account. Later Dreyer[3] pointed out that d'Arrest had played an important part in the discovery but never received proper credit and Galle[4] then acknowledged that that was so.

Discovery time

Encke[1] reported Galle's first observation on 1846 September 23, coincidentally his own birthday, at Berlin Sidereal Time $22^h\ 52^m$. The same night, two more observations were made by Galle and one by Encke. The mean of the four observation times was 1846 September $23^d\ 12^h\ 0^m\ 14^s{\cdot}6$ Berlin Mean Time. Allowing for the longitude of Berlin and the fact that the astronomical day began at noon before 1925, the GMT was 1846 September $23^d{\cdot}9629$ when Neptune's heliocentric longitude was $329^\circ{\cdot}1020$ J2000·0[5]. Neptune returns to the same longitude on 2011 July $12^d{\cdot}9000$.

Reference star

Encke[1] reported the first observed positions of Neptune relative to an anonymous star. He presented all Right Ascensions in arc measure, converted here to time to comply with the modern usage. His adopted position was $21^h\ 51^m\ 47^s{\cdot}76$, $-13^\circ\ 26'\ 9''{\cdot}6$, B1846·0. The star *Tycho-2* 5808-117-1[6], extracted by *VizieR*[7], has position $21^h\ 51^m\ 47^s{\cdot}60$, $-13^\circ\ 26'\ 14''{\cdot}2$, B1846·0 and it can be confidently identified with Encke's star because of the lack of comparable stars in the vicinity. The difference in position is $0^s{\cdot}16$, $4''{\cdot}6$.

Discovery position

Encke[1] reports the mean of the four observed positions of Neptune on the first night, relative to the reference star, as $\Delta\alpha\ +1^m\ 25^s{\cdot}43$, $\Delta\delta\ +1'\,36''{\cdot}8$, which corresponds to $22^h\ 1^m\ 30^s{\cdot}64$, $-12^\circ\ 40'\ 21''{\cdot}4$, J2000·0 (only 4^m, $11'$ from Le Verrier's prediction).

Encke makes no mention of how the observations were made. The observers probably stopped the drive and clamped the telescope a little in advance of the reference star, and rotated the micrometer so that a star ran down one web of the micrometer. They would then have timed the passage of the reference star as it crossed one or more webs at right angles, soon followed by Neptune. The difference in declinations was measured with the micrometer screw. That technique has been described by Chauvenet[8]. Encke makes no mention of differential refraction, which would have been negligible.

A similar technique may well have been used in the original search. Galle was at the eyepiece describing what he saw to d'Arrest, who had in front of him Hora XXI of the *Berliner Akademische Sternkarte*. It would be natural to start on the predicted declination at a right ascension a few minutes in advance, with the telescope fixed. If the field had a diameter of more than 22′, then Neptune would have drifted into view only four minutes after the predicted time. That would be consonant with the speed with which the discovery was made, only a few hours between dusk and seeing the unmapped object.

Ephemeris 2010/11

The ephemeris of topocentric (Berlin) ecliptic longitude was taken from Giorgini[5]. Fig. 1 plots the difference in longitude between Neptune and its discovery position in degrees over the period 2010 March 1 to 2012 March 1. Also shown are the angular distance between Neptune and its discovery position and the distance from the Sun. The last appears only as short spikes to identify the times of conjunction. There are five dates when the longitudes are equal; they are given in Table I. The path of Neptune through the discovery field is shown in Fig. 2.

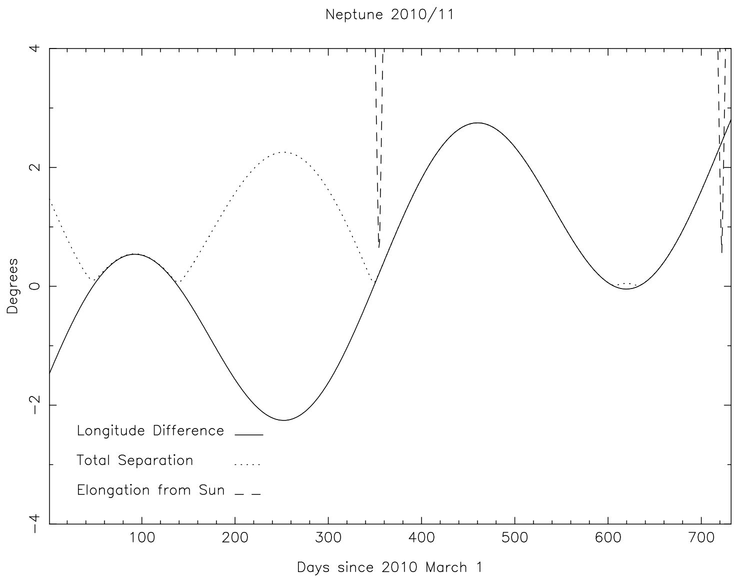

FIG. 1

Difference in geocentric longitude in degrees between Neptune and its discovery position, plotted against time in days since 2010 March 1.

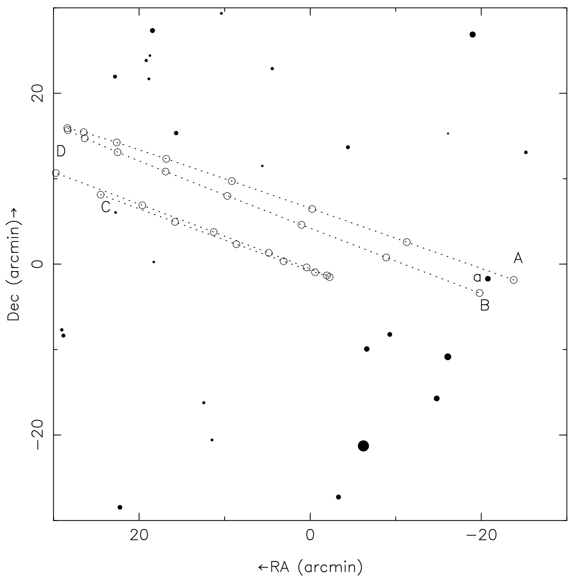

FIG. 2

1° field centred on the discovery position of Neptune. The positions of Neptune are plotted as open circles at 8-day intervals. The 2010 loop runs from A on April 2 to B on July 31. The 2011 loop runs from C on 2011 September 28 to D on December 25. The filled circles are stars from the *Tycho-2* catalogue; that marked 'a' is Encke's reference star.

TABLE I

Dates of conjunction with discovery position

Date	*Distance to discovery position (arcsec)*	*Elongation from Sun (degrees)*
2010 Apr. 17	372	58·7
2010 July 17	235	147·1
2011 Feb. 12	209	5·3
2011 Oct. 28	44	114·0
2011 Nov. 22	33	87·9

Acknowledgements

The SLALIB Positional Astronomy Library, written by P. T. Wallace[9], was extensively used in preparing this note.

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CORRESPONDENCE

To the Editors of 'The Observatory'

The Story of Gascoigne's Leap

The obituary of Ben Gascoigne in these pages (**130**, 274, 2010) started with 'Gascoigne's Leap', and later mentioned his severe stammer. Readers may be interested in some more details of what might have been a tragic accident, and a connection between that and Ben's stammer.

For the first few weeks after *AAT*'s first light, the fixed railing around the inner dome catwalk, just inside the 360 degrees of railing that rotated with the dome, was not complete on the South side. Anyone using the dome, especially at night, was warned of this; however, in hindsight, its absence created a severe hazard.

Ben and Roderick Willstrop were the astronomers working on that near-fatal Friday night, 1974 June 21, with John Rock, a draughtsman in the *AAT* Project Office temporarily acting as Night Assistant. I had a 'phone call from John late in the evening to say he feared, having been alerted by Roderick from the prime-focus cage, that Ben had fallen in the dome. Derek Fern (Freeman Fox and

Partners Resident Engineer, who was then living in Coonabarabran and was visiting the mountain) and I hastened to the dome. By then, John had the dome lights on and we found Ben lying in the very limited wooden floor space within the handling trolley for the primary mirror and cell. He was conscious but very dazed and had obviously fallen about 6 metres from the interior catwalk. We brought a chair and lifted Ben onto it. He was supporting his left elbow, which must have been painful, with his right hand. We then took him down in the service lift.

As the telescope was still under manual control, not computer, it could not be brought down to the access-park position just by pushing a single button, and Roderick was stuck in the prime-focus cage until someone had time to release him. Derek brought his car to the *AAT* goods entrance and departed immediately with Ben and Roderick to the Coonabarabran hospital, while another car with one or more astronomers also drove down. Roderick recalls Ben chatting fluently and asking several times on the journey whether his wife had been informed. Roderick also remembers the nursing staff trying to discover whether Ben had concussion by asking him "Can you remember what day of the week it is?", a question most astronomers working at night would find it difficult to answer.

I rang Hermann Wehner (Project Manager, in Canberra) and informed him of the incident. Hermann instructed me to have the railing section that was intended to prevent such a fall (and which was resting on the ground floor after its recent delivery) installed that weekend. I arranged for Coonabarabran Engineering, who had made the rail, to do so.

As early as I dared in the morning, I 'phoned the hospital and inquired after Ben's condition. I was greatly relieved when I was asked if I'd like to speak to him. He was in good spirits and I noted that his speech seemed less affected by his stutter than usual. However, we later learned that the nurses were all very concerned about the dear professor, who'd had the fall on the mountain that had affected his speech so badly!

Ben's reconstruction of the incident was that he had gone out one of the four doors onto the outside catwalk somewhere near the control room. He closed the outer door and proceeded to walk around the catwalk. He re-entered the dome through what he thought was the same door and avoided lighting his torch because he knew Roderick was exposing a red-sensitive plate. He was about 90 degrees off in azimuth. In the dark, he opened the nearest gate (in the outer railing) and stepped forward onto what he thought was the wooden flooring near the control room. But he stepped into space.

Ben was extremely lucky to have landed as centrally in the trolley as he did. As he lay, his head was within about 50 cm of the very rigid steel frame of the trolley (built to carry 46 tons). His feet were at least as close to other heavy steel-work with square edges. The gateway he opened (on the slowly rotating dome) must have been accurately aligned due South.

Ben was quite proud of having a stainless pin inserted as part of the repair to his elbow. With the compensation payment, he bought an HP programmable calculator, about the size of a portable typewriter. Furthermore, his accountant advised him of a tax benefit then in force, with which Ben was able to buy the plotter that mated to the calculator, and still come out ahead. He claimed that, if he knew the outcome would be the same, he'd be prepared to do it again! A week or two after his fall, Ben sent a message to Jack Rothwell (Project Office electrical engineer) who'd tripped over a crate when traversing coudé West in the dark and chipped a bone: "Anything you can do, I can do better."

A plaque to mark 'Gascoigne's Leap' was made and installed in time to be unveiled by Ben on his last visit to the *AAT* before his retirement from Mount Stromlo and Siding Spring Observatories in late 1980.

Yours faithfully,
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with additional information from RODERICK WILLSTROP

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2010 October 4

The Astronomer Royal's XI versus The World, 1971

Both the obituary of Walter Stibbs by David Stickland in this *Magazine* (**130**, 272 & 336, 2010) and that by Tom Lloyd Evans in *Astronomy & Geophysics* (**51**, 4·41, 2010) mention the cricket match played in the delightful grounds of Herstmonceux Castle at the time of the conference held to mark the retirement of Sir Richard Woolley from the post of Astronomer Royal and Directorship of the Royal Greenwich Observatory. It may therefore be of some interest to place on record a copy of the original score sheet which was issued at the time.

It was a very enjoyable occasion. However a certain amount of improvisation was required. If I remember correctly, Donald Lynden-Bell was able (to our amazement) to produce several pairs of white flannels (one of which I wore). Early on the day of the match, some well-known figures (including Allan Sandage, whose death has just been announced — see p. 109) were taken down to the pitch for a crash course on a game about which they appeared to know rather little. To compensate for this, The World team were allowed two extra players. In the event, the neophytes gave creditable performances. Three of the umpires probably knew the game rather well. How much the fourth (Olin Eggen) knew is not clear, but I do not remember any of his decisions being queried. Those familiar with the names of the people involved will notice that scoring was placed in the secure hands of a cosmologist and an expert on the philosophy of time, though it is not clear whether he or a typist was responsible for depriving Bill Martin of a wicket.

Yours faithfully,
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University of Cape Town

and

South African Astronomical Observatory

2010 October 4

The Astronomer Royal's XI v. The World

Herstmonceux Castle, 18th August 1971

AR's XI won by 67 runs

AR's XI

B. E. J. Pagel	b. Feast	0
R. G. Bingham	b. Gascoigne	0
C. A. Murray	c. Harding b. Stibbs	33
R. Wood	b. Harding	1
B. D. Yallop	c. Toomre b. Kinman	11
Sir Richard Woolley	c. Schmidt b. Stibbs	13
D. Lynden-Bell	c. Harding b. Stibbs	14
S. V. M. Clube	c. and b. Stibbs	11
A. L. T. Powell	b. Stibbs	9
A. J. Penny	b. Harding	17
W. L. Martin	not out	0
Extras (byes 6, wides 12, no balls 4)		22
	Total	131

FALL OF WICKETS: 6, 14, 23, 43, 68, 104, 104, 131, 131

BOWLING:	O.	M.	R.	W.
Feast	4	1	3	1
Gascoigne	4	0	7	1
Harding	7	0	41	2
Kinman	3	1	17	1
Tayler	5	0	21	0
Stibbs	5.1	0	20	5

THE WORLD

P. A. Wayman	c. Yallop b. Woolley	0
R. J. Tayler	b. Lynden-Bell	6
M. Schmidt	b. Murray	1
A. R. Sandage	c. & b. Lynden-Bell	7
D. W. N. Stibbs	c. L-Bell b. Clube	12
G. A. Harding	b. Martin	4
M. W. Feast	b. Martin	10
T. G. Hawarden	c. L-Bell b. Penny	1
A. D. Thackeray	c. L-Bell b. Woolley	7
S. C. B. Gascoigne	b. Penny	1
W. Gliese	not out	1
A. Toomre	b. Woolley	1
T. D. Kinman	c. Clube b. Penny	1
Extras (byes 7, wides 1, no balls 4)		12
	Total	64

FALL OF WICKETS: 0, 2, 18, 24, 37, 51, 51, 53, 59, 62, 63, 64

BOWLING:	O.	M.	R.	W.
Woolley	9	1	20	3
Murray	3	0	3	1
Lynden-Bell	3	0	7	2
Clube	5	1	17	1
Martin	4	1	4	1
Penny	3.2	1	1	3

CAPTAINS: AR's XI, Sir Richard Woolley
The World, G. A. Harding

UMPIRES: R. O. Redman, D. H. Sadler
O. J. Eggen, W. Nicholson

SCORER: G. J. Whitrow

REVIEWS

Galileo: Watcher of the Skies, by David Wootton (Yale University Press, London), 2010. Pp. 354, 24 × 17 cm. Price £25 (hardbound; ISBN 978 0 300 12536 8).

Galileo is one of those rare individuals: a scientist known to the general public. This is an arguably very short list which includes Archimedes, Newton, and Einstein. Whereas Newton is seen as stern and frankly rather weird and Einstein as a jolly bicycling grandfather figure, in Galileo's popular appeal there is a vague feeling that he is somehow a force for good. He is famously highlighted in Queen's ground-breaking and incomprehensible rock song *Bohemian Rhapsody* (a song which crucially includes the lyric "open your eyes, look up to the skies and see"). Much of this appeal has to do with the mythology which represents him as the rebellious, iconic figure of rational, forward-marching science in conflict with the authoritarian and irrational forces of a backward-looking religion.

As such a well-known character whose life story has been trawled over many times (the first biography was published in 1717 by his last student, Viviani), what is new that can be said about Galileo? David Wootton makes the case that although there are elements of truth in the Galileo myth in fact he was a much more complex character than just the rational scientist. This complexity was the necessary component in Galileo's psychology that enabled him to see the Universe in a new way and allows us to describe him quite fairly as the inventor of science — all science as we know it today. This multipart character, Wootton suggests, was in part a consequence of his childhood, with a musician father who conducted experiments on strings and the sound they produced, and a mother who might charitably be described as difficult. Her malign influence seems to have affected all of her children in regard to an acceptance of conflict. For example, Galileo's brother, also a musician, wrote a book in Italian on *avant-garde*, discordant, lute music, published in Germany and requiring a French lute — it was not a best seller.

Wootton presents his work in a highly readable account which begins with an attention-grabbing description of how we know anything at all about Galileo's own inner thoughts. A series of accidents and carelessness, by various of his descendants, led to Galileo's notes and documents being used as wrapping paper in a butcher's shop. They were serendipitously discovered when a learned picnicker unwrapped his ham on a sunny Italian hillside in the spring of 1750. The book focusses mainly on Galileo's life rather than his science, although the science is not ignored. There is a beautiful description of Galileo's first book, *The Little Balance*, written, according to Wootton, to correct the impression that Archimedes' 'eureka moment' was nothing more than a volume-measuring exercise. Galileo believed that Archimedes was a far greater scientist than that, and that he had in fact developed a technique for comparing specific gravity and for measuring the ratios of materials in an amalgam.

The trial and conviction of Galileo by the Inquisition in 1633 is part of the legend and there are two views about it. One view has it that the overly ambitious, egocentric scientist pretty much brought it all upon himself by ridiculing his sympathetic old friend Pope Urban VIII. The alternative view is that the mild-mannered genius, although unworldly, was a genuinely pious seeker after truth who was double-crossed by the Inquisition and their bigoted leader. Wootton explores and illuminates these positions with ample references to the texts and vignettes of the lives of contemporary scientists and cardinals.

He comes down in favour of the view that Galileo got off lightly in the context of the times — only 30 years earlier the forthright Giordano Bruno was burnt at the stake. That Galileo got off with merely house arrest may be due to a deal offered by the Inquisition's prosecutor in which Galileo agreed to recant his potentially heretical Copernicanism in exchange for which his absolutely heretical atomism would be conveniently forgotten. For the trial to happen at all was probably the result of Galileo's miscalculation and misunderstanding about his highly-placed friend's sympathies. His stubborn insistence on his place in history and perhaps his selective deafness to those same friends' frequent warnings did not help. So at the very least he could have been more accommodating and could have forestalled the conflict without compromising the fundamental science. Wootton's view is that the trial of Galileo was not an inevitable dispute between science and the Church but should be seen rather as an escalating dispute between friends (Galileo and Urban VIII). He might also point again to that difficult conflict-filled childhood.

At the beginning of this review I mentioned the positive view of Galileo in popular culture; Wootton also mentions a darker side also represented in the arts. Brecht's play, *The Life of Galileo*, has Galileo's recantation beginning the process that leads to science and scientists becoming the puppets of the powerful, which ultimately leads to a view of science as a force for evil; as not only the source of atomic weapons but also implicated in their misuse.

As science practitioners in these times in which creationism is being taught in some schools and the phrase "only a theory" is being used to denigrate every scientific proposal from climate-change to evolution, we need to care about how science and its champions from the past are presented. Wootton's excellent biography should caution us against representing Galileo as the champion of rationalism, fighting the forces of unthinking belief. Today's mass communication needs simple messages, and a complex, overreaching, egotistical, and ultimately flawed individual may not be the straightforward hero for now.

Wootton has written a thoughtful biography full of Renaissance detail in which he shows Galileo as a towering figure of genius, a man whose science was conditioned by his character, and whose character enabled him to formulate a unique view of the Universe and man's place in it. But also that that character led to unnecessary conflict and his eventual trial by the Inquisition. With punchy, highly readable chapters, each with new and unexpected details on Galileo's life, 40 pages of notes, an 11-page bibliography, and a thorough index, this must be the definitive Galileo biography for the general reader. — BARRY KENT.

Hindsight and Popular Astronomy, by Alan B. Whiting (World Scientific, Singapore), 2010. Pp. 273, 23·5 × 16 cm. Price £25/$41 (hardbound; ISBN 978 981 4307 91 8).

Let us go back a few tens of decades and imagine a lay person reading a book on popular astronomy. Not just any popular astronomy book, but one written by one of the premier astronomers of the day. Think John Herschel, George Airy, Simon Newcomb, Robert Ball, James Jeans, and Arthur Eddington. Just how much did these illustrious gentlemen get wrong? How much could the lay person trust? And were the uncertain sections in those popular books correctly highlighted and differentiated from the more solid ground?

What about the right/wrong ratio in these popularizations? Did it vary with time, and between authors? And is it about the same today as it was then? These are the questions posed by Alan Whiting as, full of the modern hindsight of a mathematical physicist, he critically dissects Sir John Herschel's *Treatise on Astronomy* (1833), Sir George Airy's *Popular Astronomy* (1848), Sir John Herschel's *Outline of Astronomy* (1869), Simon Newcomb's *Popular Astronomy* (1878), Sir Robert Ball's *In the High Heavens* (1893), Simon Newcomb's *Astronomy for Everybody* (1902), Sir James Jeans' *The Universe Around Us, 1st Edition* (1929), Sir Arthur Eddington's *Stars and Atoms* (1928), and Sir James Jeans' *The Universe Around Us, 4th Edition* (1944). This was a revolutionary period of astronomical history, a time when the physical insights into thermodynamics, quantum mechanics, radioactivity, relativity, and nuclear fusion transformed our subject; a time when stars were converted from being mysterious bodies with unknown beginnings, into gaseous spheres of reasonably well-understood life histories.

Hindsight and Popular Astronomy works superbly on two levels. Firstly, it skilfully and entertainingly reveals the recent history of our subject, not through the eyes of the scientific historian, but through the words of the people who actually had to tackle the unknown and who tried to explain it to non-scientists. Secondly, it is a warning to us all. Let me remind you of the words of Donald Rumsfeld, the former United States Secretary of State for Defence: "There are known knowns — there are things we know that we know. There are known unknowns — that is to say, there are things that we know we don't know. But there are also unknown unknowns — there are things we do not know we don't know." Astronomy, between 1833 and 1944, was blessed by topics that fell into all three categories. And in this fascinating, groundbreaking, and highly readable book Alan Whiting reminds us that very little has changed. We must be warned and we must tread with care. — DAVID W. HUGHES.

History of Astronomy in Finland 1828–1918, by Raimo Lehti and Tapkio Markkanen (Societas Scientiarum Fennica, Helsinki), 2010. Pp. 269, 23·5 × 15 cm. Price €28 (about £23) (paperback; ISBN 978 951 653 379 0).

This is a thin book, although with 231 pages of actual text, only moderately so. But the period covered is really from some time in the 17th Century (when the coloured bits on your map now known as Finland had been Swedish for a century or more) to Finland's joining the European Southern Observatory in 2004. She has also participated in the *Nordic Optical Telescope* in the Canary Islands and the *Swedish–European Submillimeter Telescope* (*SEST*) in Chile. The IAU delegation has about 62 people, comparable with the memberships from Chile, Denmark, Hungary, and South Africa.

If you intend to read this volume or others in the series on the history of learning and science in Finland during the time of the Imperial Alexander University (1828–1918), it is useful to remember that many places have Swedish, Russian, and Finnish names, so that Åbo is Turku, also known as Turun (and not to be confused with Toruń in Poland), and so forth.

Since a truly brief review cannot do justice to the marvellous illustrations and the trials and tribulations they depict, let me concentrate on Anders Donner (1854–1939). Educated in Helsinki, he was, at age 27, the only person who submitted a 'professorial thesis' to apply for the astronomy professorship there. He participated in David Gill's 1883 campaign to determine accurate positions of the asteroids Victoria and Sappho, and then set out to acquire a large (15-inch or so) refractor for the observatory. His thoughts were crystallized by the 1887 Paris meeting that gave rise to the Carte du Ciel and astrographic-catalogue projects. Of the 56 astronomers from 20 countries at the conference, he was one of the youngest (and I think the best looking!). Remarkably, he persuaded the Senate of the University to come up with the 89300 old Finnmarks that would be required to acquire and house a double astrographic refractor of the sort designed in Paris for the Carte du Ciel project. The equivalent 2010 purchasing power is about €440000 (much the same as the cost of the Helsinki Metsahovi Observatory built in the 1970s). Donner carried the lenses back with him from Paris, him in the upper berth on train and ship and them in the lower one. The Helsinki zone of the Carte was declinations +39° to +47°. Donner personally inspected virtually all the plates, and the Helsinki zone was completed in 1937, long after Donner's retirement and shortly before his death. He had been subsidizing the project for many years, and some of the workers were women, like Pickering's 'harem' at Harvard (but neither Harvard nor any other US observatory participated in the Carte du Ciel). One of the six Donner children lived until 1979 and provided a map showing how the ground floor of the Observatory building was shared between astronomical purposes and the family's living space. If you have visited Cambridge, you will be reminded of the old Observatory building, with its 'Eddington' rooms. — VIRGINIA TRIMBLE.

Turning Dust to Gold: Building a Future on the Moon and Mars, by H. Benaroya (Springer, Heidelberg), 2010. Pp. 402, 24 × 17 cm. Price £26·99/$39·95/€39·95 (paperback; ISBN 978 0 4419 0870 4).

The dust to be turned to gold in the title of this book is the lunar regolith, and the book is essentially one long argument for the exploration, settlement, and economic utilization of Earth's natural satellite. Although Mars is also mentioned in the title, the planet isn't really addressed in any depth until the penultimate chapter, and then almost as an afterthought. The Moon is the principal focus throughout, as it deserves to be given its likely pivotal rôle in humanity's expansion into the cosmos. In this I completely agree with the author, who has spent a large part of his professional career developing engineering concepts for lunar exploration and settlement.

The argument is developed from an original perspective: as a retrospective history written in the year 2169 (*i.e.*, the 200th anniversary of man's first landing on the Moon), describing developments leading to the existence of a flourishing lunar civilization by that date. Technical, social, and economic developments needed to get us to that point are described in some detail. Some of these are more plausible than others. For example, I personally doubt that lunar ^{3}He will ever be an economically exploitable resource (except possibly for niche applications in space-based nuclear-power systems). On the other hand, the author's arguments that lunar outposts could lead to the development of an industrial infrastructure in cis-lunar space (by providing fuel and energy for commercial space operations), and would act as foci for both scientific research and space tourism, are on a much firmer footing. Hopefully, these will be sufficient to develop the kind of lunar-based civilization envisaged in this book.

A significant thread of the future history sketched in the book is predicated on the development of space elevators. There is little doubt that, should they ever prove practical, space elevators would revolutionize the economics of space travel, and thereby greatly facilitate the growth of a space-based civilization.

However, given the enormity of the engineering challenge, it is at present premature to assume that they will contribute to humanity's expansion into space in the manner envisaged here. That said, the author quotes Arthur C. Clarke to the effect that "the space elevator will be built about 50 years after everyone stops laughing" and, to the extent that many people *have* stopped laughing, I agree with the author that the concept is worthy of serious engineering and economic appraisal. If space elevators prove infeasible then some other method of reliable, and relatively cheap, transportation from the Earth's surface to at least low Earth orbit (*e.g.*, by single-stage-to-orbit space planes) will have to be developed if space colonization is to proceed along the lines sketched here.

Alongside the future-history narrative, and effectively integrated into it, are two other aspects which enhance the interest of the book and its value as a contribution to the space-exploration literature. The first is inclusion of a number of political speeches setting out the reasons for key policy decisions related to space exploration, presented in the guise of important historical documents from the perspective of 2169. These include the full texts of President Kennedy's 1961 speech to Congress in 1961 May ("I believe that this nation should commit itself ..."), and his 1962 September speech at Rice University ("We choose to go to the Moon in this decade, and do the other things, not because they are easy but because they are hard ..."). Short extracts from these speeches are often reproduced in books about space exploration, but having the full texts reproduced is very useful, as they illustrate the socio-political context within which the space programme developed. They also demonstrate the importance of political *leadership* in developing space capabilities, something which is all too lacking at present.

The second innovation is the inclusion of interviews with contemporary individuals who have played, or continue to play, prominent rôles in areas of space technology, economics, and policy relevant to lunar settlement (including a rare interview with Neil Armstrong). These interviews provide numerous useful insights, and as a set provide a valuable snapshot of informed early 21st-Century opinion on matters relating to space exploration. Indeed, the whole premise of the book, and one with which I very much agree, is summed up by one of those interviewees, the NASA astronaut and flight surgeon Lee Morin, when he writes: "if we are to become a spacefaring civilisation we must have sustained presence off the Earth. If we can't get that done on the Moon, we certainly aren't going to get it done anywhere else."

Turning Dust to Gold makes a powerful case for the importance of lunar colonization, and outlines the steps human civilization is likely to have to make in order to achieve that goal. It contains much food for thought, and I recommend it highly to anyone with an interest in the future of human space exploration. — Ian Crawford.

Pluto: Sentinel of the Outer Solar System, by B. W. Jones (Cambridge University Press), 2010. Pp. 231, 23·5 × 16 cm. Price £25/$35·99 (hardbound; ISBN 978 0 521 19436 5).

This assuaging presentation of Pluto's promotion to, then demotion from, planetary status is intended to generate broad interest in the outer Solar System. The stories of history and discovery are enjoyable and informative. The methods by which astronomers learn what they do purely by observation is presented in a manner that is fascinating to the lay reader.

The first chapter is a loosely organized collection of generalities that is clearly intended to provide background information on the Solar System and relevant concepts to readers with a rudimentary understanding of astronomy. In his effort to ease such readers into the subject, Professor Jones introduces scientific concepts in such simplistic terms and generalities that his subsequent in-depth explanations, often chapters later, cause confusion. One example of this is the author placing so much importance on the Titius–Bode rule for the discovery of Neptune, then does not state it. While a generalization may be sufficient to understand why Adams and Le Verrier, as well as others, were looking for an eighth planet, the reader is left to wonder how, on page 55, Neptune at 38 AU is so far off the mark, yet on page 61, it suddenly "... fits the rule rather well ...".

Other misleading simplifications include: over-use of the term 'asteroid' as a catch-all, which results in the absurd sentence "[1992 QB_1] remains with its provisional name, though it has been given the asteroid number 15760 but, of course, it's not an asteroid" (there is no such thing as an asteroid number; it's a minor-planet number); implying all comets "develop huge, spectacular tails" (page 12) when this is the rare exception; and stating "(except for the recently discovered ring of Saturn)" with no further explanation (page 31). How many readers will question, "recently discovered? But, back in school we were told ..."

Box 6.1 has a factual error. The subscript '1' in a provisional designation denotes that the alphabet has already been cycled through once. Therefore, 1992 QB_1 was the twenty-seventh (the letter 'I' is omitted) object discovered in the second half of August of that year, not the second as stated. 1992 QB (no subscript, the actual second object reported in late August) has the minor-planet number 18393.

A few other factual errors were noted: "Trojans were the inhabitants of the ancient city of Troy" (page 168) is only half the story. Littleton should be Lyttleton (page 105). Among people not mentioned for their contributions, I note the absence of the Berkeley group Armin O. Leuscher, Fred Whipple, and Ernest Clare Bower, who had a much better initial orbit for Pluto; furthermore, in announcing the discovery of 1992 QB_1 (page 150), it was actually Brian Marsden who specified the range of 37–59 AU from the Earth at discovery.

As it is intended, this book will likely appeal to the general public seeking the history of Pluto's discovery, as well as those interested in the basic science behind how astronomers know what they do about the outer Solar System. — C. L. MARSDEN.

Planetary Science, 2nd Edition, by Imke De Pater & Jack J. Lissauer (Cambridge University Press), 2010. Pp. 647, 25 × 19·5 cm. Price £50/$90 (hardbound; ISBN 978 0 521 85371 2).

The first edition of this superb comparative-planetology textbook came into print in 2001 and now, nine years later, we are presented with the second edition, and an increase in page numbers from 528 to 647. What has happened in this nine-year period to justify this jump? Well, the short answer is "lots". And these advances have been abundantly scattered throughout the whole Solar System.

NASA's *Messenger* spacecraft started investigating Mercury in 2008 January and not only confirmed the extent of the volcanic alteration of the surface, seen by its predecessor *Mariner 10* over four decades before, but has also found water-ice at the planet's sun-less poles. ESA's *Venus Express* went into orbit in 2006 April and has been investigating the winds in the cloud regions and monitoring possible temporal compositional changes in the Venus atmosphere. The impact, in 2009 October, of the satellite *LCROSS* with the lunar south-pole crater Cabeus revealed several absorption features of water and hydroxyl molecules in the resultant water plume. NASA's *Opportunity* and *Spirit* have been roving around Mars since 2004 January, producing revealing images of the effects of wind weathering and aqueous alteration.

The Japanese probe *Hayabusa* and NASA's *Near-Earth Rendezvous* mission have returned stunning images of asteroids 25143 Itokawa and 433 Eros. The *Hubble Space Telescope* has imaged atmospheric debris when an unexpected comet or asteroid hit Jupiter on 2009 July 19. The space probe *Deep Impact* provided high-resolution images of the surface features of the nucleus of comet C/Tempel 1. The *Cassini* orbiter continues to probe the ring structure of Saturn and has produced stunning pictures of Calypso and Telesto, as well as detailed radar images of the surface of Titan. The tally of Kuiper Belt objects has increased considerably as has the number of known extra-solar planets. This has produced a commendable boost in the study of planetary-system-origin processes as well as in the celestial mechanics of orbital perturbation and long-term parameter variation.

And all along there has been the steady improvement in the accuracy of planetary parameters. Enquirers as to, say, the mass of the main asteroid belt and Saturn's rings are not now met with an apologetic shrug.

What is really impressive about the second edition of this landmark textbook is the thoroughness of the revision. I can seriously say that I could find no section that had not been up-dated. *Planetary Science* is a first-class introduction to the topic, accurately aimed at the enquiring undergraduate university student. It is succinct, well written, and generously illustrated. The equations are there, as are the suggestions as to further reading, and the examples to hone your examination skills. Anyone with even a vague interest in our planetary environment will benefit hugely by studying this exemplary tome. — DAVID W. HUGHES.

Pathways Towards Habitable Planets (ASP Conference Series, Vol. 430), edited by V. Coudé du Foresto, D. M. Gelino & I. Ribas (Astronomical Society of the Pacific, San Francisco), 2010. Pp. 570, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 740 7).

Many areas of specialized human activity, particularly engineering and the sciences, are blessed with more than their fair share of acronyms. Astronomy and astrophysics is no exception, and is littered with examples which have sprung up to ease the writing of research papers, and often the squeezing of science cases into space-limited grant and telescope applications. Many of these acronyms are widely used and understood (CCD, CMD, VLT, dSph), whereas others seem at first glance to be highly contrived (TATOOINE, CASSOWARY, SQIID, FRODOspec and several instances of GIRAFFE). This reviewer has spent many hours trying to fit his latest ideas for a project into a slightly suspicious acronym, with little success (SPASM, CLUNK, and DRIBBLE came the closest to working). The contrived acronym can be great fun as well as acting as a memory aid (see, for example, episode 3, series 3, of the British TV programme *Red Dwarf*).

How does one go about searching for life on planets outside our own Solar System? This question has increased in importance since the discovery of the first extrasolar planet in 1995, and the Blue Dots Initiative (www.blue-dots.net) is dedicated to mapping out the best ways to find and characterize habitable planets. The subject of this review is the proceedings of a conference held in Barcelona in 2009 September entitled 'Pathways Towards Habitable Planets'. The search for extrasolar life touches on very different scientific communities, including astrophysics, biology, astrobiology, and planetary science. The conference was therefore wide-ranging, and the proceedings includes a diverse set of papers. After a review on how to find a habitable planet, by James Kasting, there are reports from several working groups pondering that goal, the consideration of some scientific aspects, then a very healthy section on current and forthcoming instruments. Further sections consider broader scientific points, future astronomical instrumentation, and finally a welcome number of poster presentations. Within this structure are diverse concepts such as habitable moons (not a new idea!), alien life in different cultures, exo-zodiacal dust, abiotic production of ozone on planets around M-dwarfs, and comet and asteroid impacts on young planets.

The book itself is, like others in the ASP Conference Series, agreeably weighty and made from high-quality paper. I was particularly impressed at how few typographical errors there were (one contribution excepted) and how well the figures and images were reproduced. The main error is the lack of labels for the graphs in one paper, which was balanced by another whose axis labels were huge.

This reviewer's dedication to the cause began well, with copious notes taken on many of the longer contributions. But as I moved into section ten (poster presentations) the notes gradually transformed into a daunting list of the acronyms used in the book. These range from classical ideals (PLATO, EUCLID, DAVINCI, EPIC, ALADDIN) *via* physical objects (SPICA, GRAVITY, GARLIC, SEEDS, DUNES) to disparate possibilities (NAHUAL, SPHERE, ATLAST, ACCESS, NULLTIMATE, MESSy, SAFARI, BETTII).

This book provides a useful insight into a research area which is highly interesting not only to scientists but to the general public, and will be a welcome addition to the shelves of many institutional libraries. The search for habitable planets is moving forward PDQ, so I urge interested readers to browse their copy of this book ASAP. — JOHN SOUTHWORTH.

Exoplanet Atmospheres: Physical Processes, by S. Seager (Princeton University Press, Woodstock), 2010. Pp. 243, 23·5 × 15·5 cm. Price £30·95 (paperback; ISBN 978 0 691 14645 4).

Upon hearing about this book I was excited by the idea that the study of exoplanets — planets orbiting stars other than our Sun — had reached the point where a whole book could be written about their atmospheres. I thought I knew that the observation and even the detection of these planets is so difficult that obtaining much detail on their atmospheres had to be still some way off. It is also the case that planets as small as the Earth remain beyond the detection limit for the time being, although probably not for long. The 'hot Jupiters' — gas-giant planets orbiting close to their parent stars — make up the bulk of the discoveries, and these have been found by some observers to have water, methane, and carbon dioxide, as well as hydrogen and helium, in their atmospheres, although the detections are marginal and remain controversial at present.

That is pretty much it — where could a whole book come from? The answer is in the subtitle, 'Physical Processes', in small print on the cover. The chapters are about processes we know about from the atmospheres of the Earth and planets of the Solar System, and since of course we expect the physics to be the same elsewhere in the Galaxy, these processes must also apply in exoplanet atmospheres. Normally, it is anathema for a reviewer to quote chapter titles and contents but in this case I think it is appropriate and essential: 'Intensity and flux' (an introduction to radiation theory); 'Temperature, albedo and flux ratios' (the radiative energy balance of a planet); 'Composition of a planetary atmosphere'; 'Radiative transfer' (the derivation of, and simplified solutions to, the radiative-transfer equation); 'Polarization'; 'Opacities' (molecular bands and transitions); 'Vertical structure' (adiabatic lapse rates, radiative equilibrium in a gray atmosphere); and 'Atmospheric circulation'. The likely relevance of all these to exoplanet atmospheres is mentioned briefly in most cases.

As a basic textbook for an introductory course in atmospheric physics for undergraduates this book is fine; it's nicely and clearly written and includes model exam questions. The real book on exoplanet atmospheres is still to be written, probably about ten years hence. I hope the present author takes it on. — F. W. TAYLOR.

Transiting Exoplanets, by C. A. Haswell (Cambridge University Press), 2010. Pp. 335, 26·5 × 21 cm. Price £75/$130 (hardbound; ISBN 978 0 521 19183 8), £35/$60 (paperback; ISBN 978 0 521 13938 0).

In the last 15 years we've seen the emergence of the study of exoplanets as one of the most exciting areas of contemporary astrophysics. It is such a dynamic subject that any book written on the subject risks being obsolete before it even reaches the bookshop. While some parts of this book have already suffered that fate, most of it will stand the rigours of time (well, for a few years anyway). The reason for this is that it is a textbook, and while the author still manages to impart the excitement of the subject to the reader she manages to cover material that will be fundamental for all courses on exoplanets for the foreseeable future.

So why concentrate on *transiting* planets? The answer is that, in general, these are the only planets for which we can determine accurate parameters (actually in most cases, the parameters are determined relative to those of the host star, for which our knowledge is being revealed as inadequate — but that's a different story), as well as, possibly, their atmospheric constitution. While most planets have been discovered through the reflex motion of their host star, without their orbital inclination little can be said (with any accuracy) about the planets themselves. Other methods (*e.g.*, microlensing, direct imaging) have added a small but important number of objects.

The book is composed of eight chapters dealing with all aspects of the subject. As you would expect of a good textbook, problems and calculations are interspersed throughout the book, and, as is typical of Open University volumes, worked problems and background material is also provided. Figures and pictures are abundant throughout the book. The underlying physics and mathematical models for, *e.g.*, transit and Rossiter–McLaughlin effects, are well catered for and will serve as an appetizer for those researching these areas. While the author uses these results to demonstrate planetary composition, this section is already dated and, with *Kepler* results coming, will be historical very soon. Nonetheless there is plenty of valuable material here.

This is an excellent and beautifully written book (I wouldn't have expected anything else from the author), ideal for advanced undergraduate courses or new PhD students. I will certainly be recommending to my students that they get this book — I would imagine for the next few years at least it will be vital reading. — DON POLLACCO.

Hot and Cool: Bridging Gaps in Massive Star Evolution (ASP Conference Series, Vol. 425), edited by C. Leitherer, P. D. Bennett, P. W. Morris & J. Th. Van Loon (Astronomical Society of the Pacific, San Francisco), 2010. Pp. 294, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 730 8).

This volume presents the papers given at a workshop held at CalTech in 2008 November, attended by about 50 participants drawn from the research communities studying massive stars in the hot and cool regions of the upper part of the H–R diagram (HRD). The main aim of the organizers was to bring together hot-star and cool-star pundits who traditionally have held their own, separate meetings in the past. Whilst there is a separation in temperature in the blue/red parts of the upper HRD, the stars themselves are intimately linked *via* stellar evolution, and both display extended atmospheres, mass loss, and stellar winds, which affects their atmospheric properties and evolution.

The conference covered several important themes embracing: (*i*) the latest evolution models and tracks, including the effects of rotation, improved opacities and (current) estimates of mass-loss rates, addressing the blue–red–blue evolution in different metallicity environments, from the main sequence, through luminous-blue-variable and red-supergiant phases, to final supernovae explosions; (*ii*) models and observations of their stellar atmospheres, to refine temperatures, luminosities, and chemical abundances; (*iii*) mass-loss properties, with particular emphasis on episodic events and non-spherical outflows, the importance of which may outweigh the effects of steady stellar winds; (*iv*) circumstellar environments and dust formation. In each area there is a healthy combination of papers dealing with the latest theoretical developments and new observations across the electromagnetic spectrum.

The proceedings will be of interest to all those working in the fields of hot- and cool-star research, to get people, including new students, up to speed in the latest developments, both in theory/modelling and observations, and is a must for libraries. — ALLAN WILLIS.

X-ray Polarimetry: A New Window in Astrophysics, edited by R. Bellazzini, E. Costa, G. Matt & G. Tagliaferri (Cambridge University Press), 2010. Pp. 362, 25 × 18 cm. Price £70/$125 (hardbound; ISBN 978 0 521 19184 5).

Our understanding of the X-ray Universe is based almost entirely on the techniques of imaging, spectroscopy, and timing. There is, however, another type of measurement which can reveal a great deal of information hidden from these more conventional approaches. But astrophysical X-ray polarimetry — measuring the angle of the plane in which the X-ray electromagnetic wave from an object oscillates — remains almost as unexplored now as it was 30 years ago. To date only one unambiguous measurement of polarization in a cosmic X-ray source has been made — that of 19% linear polarization at 2·6 keV for the Crab Nebula. The low photon fluxes received from astronomical objects, combined with the limitations of existing instrumentation, have rendered polarization observations insensitive to all but the brightest sources.

X-ray Polarimetry: A New Window In Astrophysics is the proceedings of an international conference on the subject held in Rome in 2009, prompted by the prospect of a new generation of missions and instruments which may finally unlock the information hidden in the light we receive from some of the most exotic objects in the cosmos.

Following an introduction by Martin Weisskopf, one of the team who measured polarization in the Crab Nebula three decades ago, the book is divided into three sections, the first of which addresses the physical techniques used to detect X-ray (and gamma-ray) polarization. Some papers include background information which will assist readers new to these concepts, but since this is a conference proceedings, much of the material is written for a readership familiar with the field. The 12 contributions in Part I include a discussion of new developments in conventional techniques such as Bragg reflection and Thomson and Compton scattering used on previous missions. But today, photoelectric methods of detecting polarization are leading to new, more capable instrumentation which exploits the correlation between the polarization angle of the incoming photon and the direction into which the resulting photoelectron is ejected. In the mid–late 1990s, work had already begun on the use of this effect in pixellated detectors such as CCDs, and recent developments in this area are covered in one paper. More recently attention has turned to gas electron-multiplication (GEM) detectors, which use a gas-filled volume to produce an avalanche which amplifies the effect. GEM detectors are now in development for a number of missions and so it is appropriate that several papers are presented addressing different aspects of this method.

In addition to the detection of polarization, we also find a chapter on the use of Laue lenses to collect photons from gamma-ray and hard-X-ray sources. Given the low number of photons which are received from most of these sources, this is an important issue to address; indeed, the resurgence of interest in X-ray polarimetry is driven in part by new mission proposals which offer, for the first time, sufficient collecting area to make the technique possible. But it is also disappointing to find an important omission in this section. An alternative approach to the detection of X-ray polarization is the use of dichroic filters — new, highly ordered materials currently under development which act like the X-ray equivalent of Polaroid filters at visible wavelengths, offering a compact, low-mass approach to polarimetry for certain missions. While a brief reference to this research is given in Weisskopf's introduction, no papers on X-ray dichroics are presented.

Part II contains 23 papers covering the origin of polarized high-energy photons in astronomical sources, and the information which can be obtained when this polarization is measured. Readers involved in the development of polarimeters will find a number of particularly useful papers discussing sensitivity requirements for future instruments. Several authors address the science that will be possible with particular detectors and missions, but there is a wealth of more general information on the value of polarimetry in the observation of specific object classes, including gamma-ray bursts, white dwarfs, neutron stars, and active galactic nuclei. Internet links are also provided to several codes which can be used to model the polarization expected in particular objects. As the book title suggests, emphasis is placed on compact objects and cosmological sources, and readers concerned with non-degenerate stellar sources, or objects in the Solar System, must look for information elsewhere.

Part III consists of 17 papers on future missions carrying X- or gamma-ray polarimeters, including balloon experiments and precursors to larger space missions. Here, the pace of development means that some details have inevitably changed since the papers were written. The GEM instrument for *IXO* is described in one paper, but the future of *IXO* (whose large mirror area offered the most promising opportunity to measure X-ray polarization in a significant number of objects) is now uncertain following the recommendations of the US Decadal Survey on Astronomy and Astrophysics announced last August. Launch dates for missions such as *PoGOLite* have been pushed back, while more positive updates can be reported elsewhere: the

Japanese solar-sail mission *IKAROS*, with its *Gamma-Ray-Burst Polarimeter*, has not only been launched since the paper was written but has detected a GRB event. And it is good to see that the authors of the paper on the US *Gravity and Extreme Magnetism* (*GEMS*) mission updated their contribution prior to publication to reflect the fact that the mission has now been selected for flight in 2014.

Overall, it is hard to find significant fault with *X-ray Polarimetry*. Aside from the omission of X-ray dichroic filters, all of the major instrumentation and mission developments are covered, while Part II contains what is probably the most comprehensive summary of work on astronomical X-ray polarization published to date. Perhaps unusually for a conference proceedings, this is a book which should be essential reading for any graduate student or researcher involved in the development of X-ray and gamma-ray polarimeters, or in the study of objects and processes which produce polarized X- and gamma-ray signatures. — NIGEL BANNISTER.

Dark Energy: Theory and Observations, by L. Amendola & S. Tsujikawa (Cambridge University Press), 2010. Pp. 491, 25 × 18 cm. Price £45/$75 (hardbound; ISBN 978 0 521 51600 6).

Dark energy is the name given to the unknown phenomenon that is causing the expansion of the Universe to accelerate. Dark energy accounts for over 70% of the total energy content of the Universe. These two observations, that the Universal expansion is accelerating and the approximate proportion of dark energy, account for our current knowledge of dark energy. Indeed, the mysterious name is indicative of our lack of understanding of what this phenomenon is. In this scenario, it may seem premature to write a book on a substance of which we know almost nothing. However, the authors of this book rise to this challenge and have published a comprehensive account of what dark energy *could* be, and how cosmologists are planning experiments to solve this problem. Indeed, in the authors' own words, "this book is not as much about results as it is about suggestions and methods".

The first four chapters set the scene by reviewing some of the foundations of observational cosmology; these chapters are well written and up-to-date, but for a beginner in the field I would recommend other textbooks that have a broader remit; there is also some repetition between these chapters and the final chapter on future directions.

The observational chapter is limited to twenty pages, which was a disappointment: while cosmology has yet to answer the question, "what is dark energy?", there is overwhelming evidence from cosmology that dark energy exists. The authors start with supernovae observations, and as such miss an opportunity to review some interesting historical precedents.

The bulk of the book systematically presents each of the main physical theories that may explain dark energy. They are a welcome complement to the textbook literature in cosmology. The text is up-to-date, covering the most conventional explanation of dark energy, the cosmological constant (with all its problems, including a generous discussion on anthropic reasoning), through to string-theory-inspired modifications to General Relativity. In order to cover this broad scope there is little room for introductory material, and the explanations are technical, aimed at the post-graduate level or active researchers in the field. However, the problems at the end of each chapter are tractable, given an undergraduate education in physics or mathematics, and would be a good place to start if one is new to the field.

The final chapters look to the future. There is a refreshing overview of predictive statistical techniques, and the future observational chapter reviews the main probes introduced in the first chapters but within the context of planned experiments.

This is a timely book, and fills a niche in the textbook library by presenting a map of where dark energy may take our understanding of the Universe. I would recommend this book to new post-graduates and researchers working on the dark-energy problem, and to enthusiastic undergraduates. For a researcher at the cutting edge of dark-energy research, this book is a welcome companion. — THOMAS KITCHING.

An Illustrated Guide to Relativity, by T. Takeuchi (Cambridge University Press), 2010. Pp. 256, 22·5 × 15 cm. Price £45/$75 (hardbound; ISBN 978 0 521 76394 3), £16·99/$28·99 (paperback; ISBN 978 0 521 14100 0).

This charming book sets out to explain the basics of Einstein's Theory of Special Relativity using only figures. It achieves its goal, although limited mainly to relativistic kinematics. The theory is geometric in its essence, and, as is well recognized, space–time diagrams lend themselves to particularly lucid depictions of relativistic phenomena. What is impressive about this book is its dedication to space–time diagrams, amusingly presented, as an effective teaching tool. No equations are relied upon to provide a qualitative discussion of the main results of the theory.

The book is suited for non-scientists, requiring no more than an ability to read graphs and understand elementary geometry. At the same time, it is well suited as a supplementary text to a beginning physics course including Special Relativity. The diagrammatic approach would help a student develop an intuition for relativity. It starts with a gentle introduction to space–time diagrams for Galilean kinematics. The extension to Special Relativity is made natural once the constancy of the speed of light independent of inertial frame is accepted. The proofs of the qualitative aspects of the addition of velocities, time dilation, length contraction, and the Doppler shift are rigorous. Notes to the text include the Lorentz transformations from which a mathematically inclined student could work out the effects quantitatively. A large set of entertaining exercises, with solutions, is included that would strengthen the student's understanding. A final section touches on dynamical aspects of the theory by developing the relativistic four-momentum geometrically, similarly to the kinematics, culminating in a discussion of $E = mc^2$, although this section is much sketchier than the lengthier kinematics section and necessarily resorts to equations.

The text unfortunately has a couple of gaffes. Most notable is the attribution of the cosmological redshift to the Doppler effect rather than correctly to the expansion of the Universe. The Doppler formula coincides with the cosmological only for scales small compared with the curvature of the Universe. The attribution of the energy generated from nuclear reactions to the release of the binding energy of the nucleons rather than the conversion of mass into energy is less a mistake than somewhat misleading. The inertial mass of the nucleus arises in part from the binding of the nucleons, but also from their motions and the motions of their constituents (quarks). The point that should have been made is the equivalence of mass and energy: Einstein's famous relation shows the two cannot always be clearly distinguished.

These shortcomings detract very little from the considerable merits of the book. A lecturer need only mention them to the students to be confident of having an effective text for teaching Special Relativity unique in its conception. — AVERY MEIKSIN.

Advances in Hellenic Astronomy during the IYA09 (ASP Conference Series, Vol. 424), edited by K. Tsinganos, D. Hatzidimitriou & T. Matsakos (Astronomical Society of the Pacific, San Francisco), 2010. Pp. 515, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 728 5).

Advances in Hellenic Astronomy during the IYA09 contains the conference proceedings of the 2009 International Conference of the Hellenic Astronomical Society. These conferences take place every two years, and bring together mostly Greek but also international researchers in the broad areas of astronomy and astrophysics. This volume therefore contains a range of articles organized within six broad thematic categories, four of which cover most areas in astrophysics, from the Solar System to the Galaxy and beyond. The review papers are thorough and up to date, including a slightly exotic description of 'stickiness' by G. Contopoulos and M. Harsoula, and there are small treasures amongst the contributed papers, such as that on 'Gravitational waves', by Kokkotas *et al.*, that on 'Cosmic infrared and cosmic optical background', by Georganopoulos *et al.*, and that on the 'Cherenkov telescope array' by Emmanoulopoulos *et al.* This last paper is found in the section on 'Astronomical instrumentation'. Unfortunately, large European projects such as *LOFAR* (current) and the *ELT* and *SKA* (future) are somewhat conspicuous by their absence. Nevertheless, the book presents an accurate representation of astrophysical research within the Hellenic community. — Aris Karastergiou.

Emerging Space Powers, by B. Harvey, H. H. F. Smid & T. Pirard (Springer, Heidelberg), 2010. Pp. 732, 24 × 17 cm. Price £27·50/$44·95/€44·95 (paperback; ISBN 978 0 4419 0873 5).

In the 1950s and 60s, space activities were dominated by the two superpowers, the United States and the Soviet Union. By the 1970s, Europe, Japan, and China had begun to recognize the technological and scientific benefits to be realized from significant investments in their domestic space endeavours. Towards the end of the 20th Century and in the first decade of the current century, new players arrived on the scene, notably India, Korea (North and South), Brazil, Israel, and Iran. These up-and-coming space powers are the subject of this comprehensive account.

The present book has evolved considerably from the original 1999 volume about the Indian and Japanese space programmes, authored by Brian Harvey. Although Harvey has enlarged and updated his account of their important contributions and ambitions, the book has now been expanded to include contributions by two other well-known writers in the field of space activities. Each of them has been responsible for a different section of the book, summarizing the technical advances in rocketry and satellite development in these less-familiar space programmes.

Not surprisingly, almost half of the book is devoted to Japan and India. This is as much a reflection of the availability of open technical literature as the technical complexity of each programme. In the cases of North Korea and Iran, interpretation of the scarce — and sometimes misleading — information released by each country inevitably leads to considerable speculation. The launches of 'ghost' satellites by North Korea are a classic example.

Well illustrated throughout, this hefty tome is a valuable one-stop reference for the status of the programmes in emerging space nations up to the end of 2009. Although most of the book is devoted to technical aspects of each programme, there is some effort to address the motivations and priorities of each nation.

Appendices include a full list of launches involving these countries, plus details of space institutes in Iran, Brazil, and North Korea. — PETER BOND.

Prepare for Launch, by Erik Seedhouse, (Springer, Heidelberg), 2010. Pp. 250, 24 × 17 cm. Price £31·99/$34·95/€34·95 (paperback; ISBN 978 1 4419 1349 4).

Many young folk dream of being an astronaut. Imagine the advert, "wanted for hazardous journey. Low wages, bitter cold, long hours of complete darkness. Safe return doubtful. Honour and recognition in event of success". This was, in fact, the advert said to be posted by Sir Ernest Shackleton, in 1914, but it could work equally well to recruit future space explorers. In *Prepare for Launch* you can read a detailed and thorough account written by someone who tried very hard to change his astronaut dream into a reality.

Seedhouse's book contains the nuts and bolts of astronaut training in North America and Europe. It is the view from the inside written by a man who really wanted to make it to space. Seedhouse put himself through the rigours of life in the 2nd Battalion of the Parachute Regiment, was trained by the Special Air Service in the art of jungle and desert warfare, became a professional triathlete, and also obtained a first degree in Sports Science, a Master's degree in Medical Science, and a PhD in Space Medicine before applying twice to become an astronaut with the Canadian Space Agency.

He leads us step by step through the protracted process of becoming an astronaut. We are briefed as to how to fill in the initial application form and given clues as to the basic selection criteria that are applied. Then, when you become an astronaut candidate (typically shortened by the space agency to 'ascan'), we are told what happens during probationary training, the joys of the 'vomit comet', the neutral-buoyancy laboratory, survival training in the Rockies, space-food tasting, and 'pulling the Gs' in the centrifuge, before moving on to the details of mission training and finally pre-launch preparation. The tasks and exercises and deprivations of space life are then revealed in chilling detail.

Recruitment of the first astronauts was much simpler; they were selected from the pool of service test pilots. Later on things became less militaristic, but without a PhD, super fitness, excellent health, good looks, an outgoing character, and the psychological balance of a saint it seems to be a waste of time starting.

It is quite clear that there are easier jobs to get, but someone, sometime, has to live on the *Space Station*, explore the Moon (again), and go to Mars. If that someone is you, this book tells you how to get started. And even if it is not you, this book tells you what you might be missing. Seedhouse has written an intriguing account of the training process, and his book is a joy to read. You are left in no doubt that being an astronaut is one of the most challenging and demanding jobs in the world. And what of Seedhouse — did he make it? He plans to travel into space on one of the first privately owned space-planes. — CAROLE STOTT.

Carnarvon and Apollo: One Giant Leap for a Small Australian Town, by P. Dench & A. Gregg (Rosenberg, Kenthurst, NSW 2156; through Gazelle, Lancaster), 2010. Pp. 303, 22·5 × 15 cm. Price £20 (paperback; ISBN 978 1 87705897 4).

Hundreds, perhaps thousands, of books have been written about the Apollo programme to land humans on the Moon. That multibillion-dollar effort, which involved the mobilization and cooperation of hundreds of companies, as well as hundreds of thousands of engineers and scientists, is rightly regarded as a triumph for US technology. What is less appreciated is the rôle played by a few-hundred spacecraft trackers and technical staff in the Australian outback.

This book is the story of Carnarvon, a small, isolated town in north-west Australia where communications were *via* short-wave radio or a manned telephone exchange, and television was non-existent. Until 1962, when NASA officials came looking for the ideal site for a tracking, telemetry, and control station, Carnarvon was best known for its prawns, bananas, and sheep stations. Suddenly the sleepy, isolated settlement of 2000 inhabitants became a vital communications hub in the American drive to send humans into space and beat the Soviets to the Moon.

From 1964 to 1975, Carnarvon was home to the largest NASA tracking station outside mainland America. Every manned mission in the Gemini, Apollo, and Skylab programmes was tracked and monitored from the station, not to mention important landmarks in unmanned exploits, such as the *Echo 2* balloon, and the *Ranger-6* and *Helios-A* spacecraft.

In the early days, Carnarvon acted as a magnet, drawing in engineers from overseas, as well as the largest Australian cities. With this sudden influx of technical staff and their families, the entire social structure of the town was transformed. This book, written by two people who were intimately involved in the high-tech transformation of this remote corner of Australia, tells two stories: the evolution and successes of the tracking station from its birth to retirement, and the social revolution which it brought. Meticulously researched and nicely illustrated, the authors provide a detailed, but affectionate, account of how the most exciting episode in human spaceflight opened up a whole new world for thousands of people 'down under'. — PETER BOND.

Plumes vs Plates: A Geological Controversy, by Gillian R. Foulger (Wiley-Blackwell, Chichester), 2010. Pp. 328, 24·5 × 19 cm. Price £37·50 (paperback; ISBN 978 1 405 16148 0).

This new work represents a major contribution to an active and intense debate about the rôle of deep-seated mantle plumes in understanding global tectonics and particularly the origin of 'large igneous provinces'. The concept that magmatism away from plate boundaries might be generated by mantle plumes was first proposed in 1971 by Jason Morgan and since that time the model has provided an increasingly widely applied mechanism for explaining many types of magmatic, thermal, or subsidence anomalies. Foulger has been a spokesperson for a growing number of geoscientists who have come to query the rather uncritical application of the plume concept to many geological settings. In this contribution she reviews a series of different geological, geophysical, and geochemical aspects of plumes and tests whether all the supposed criteria related to plumes can be recognized or not. She notes that, in many cases, only a few of the expected characteristics of plumes can be recognized and then proposes alternative, tectonic-based explanations to account for the apparently anomalous behaviour. The text is mostly convincing and the reader is left with the feeling that the plume model has been over-applied, especially by geochemists in recent years. The seismic tomographic images that largely fail to show deep-seated velocity anomalies are especially telling. Nonetheless, I was left with a feeling that, while many of the supposed plumes can be explained by plate processes, there may still yet be room for some of the largest plumes, such as Hawaii, in our revised model of the Earth. This debate is sure to resonate for several years yet, and this book allows readers to come quickly up to date with the central issues.

This text is well written and easy to digest for the educated reader. Bullet points make it easy to skim read and pick the sections that interest you. It probably best suits advanced undergraduates and postgraduate students and would make a good text for courses in petrology, geophysics, or basin analysis. The book is well illustrated, although some of the figures are not of the highest quality, featuring a jumble of different font styles and sizes, culled from the various source papers and generally not redrafted. The crucial seismic-tomography figures are provided as a colour insert but the black-and-white versions embedded in the text are almost useless and a little frustrating. That said, the book is a valuable edition to any geologist's library and provides material for thoughtful debate, whether you agree with Foulger or not. — PETER CLIFT.

THESIS ABSTRACTS

THE TRANSIENT RADIO SKY

By Evan Francis Keane

The high-time-resolution radio sky represents unexplored astronomical territory where the discovery potential is high. In this thesis I have studied the transient radio sky, focussing on millisecond scales. As such, this work is concerned primarily with neutron stars, the most populous member of the radio-transient parameter space. In particular, I have studied the well-known radio pulsars and the recently identified group of neutron stars which show erratic radio emission, known as RRATs, with radio bursts every few minutes to every few hours.

When RRATs burst onto the scene in 2006, it was thought that they represented a previously unknown, distinct class of sporadically emitting sources. The difficulty in their identification implies a large underlying population, perhaps larger than the radio pulsars. The first question investigated in this thesis was whether the large projected population of RRATs posed a problem, *i.e.*, could the observed supernova rate account for so many sources. In addition to pulsars and RRATs, the various other known neutron-star manifestations were considered, leading to the conclusion that distinct populations would result in a 'birthrate problem'. Evolution between the classes could solve this problem — the RRATs are not a distinct population of neutron stars.

Alternatively, perhaps the large projected population of RRATs is an overestimate. To obtain an improved estimate, the best approach is to find more sources. The Parkes Multi-beam Pulsar Survey, wherein the RRATs were initially identified, offered an opportunity to do just this. About half of the RRATs showing bursts during the survey were thought to have been missed, due to the deleterious effects of impulsive terrestrial interference signals. To remove these unwanted signals, so that we could identify the previously shrouded RRATs, we developed new interference-mitigation software and processing techniques. Having done that, the survey was completely re-processed, resulting in the discovery of 19 new sources. Of these, 12 have been re-detected on multiple occasions, whereas the others have not been seen to re-emit since the initial discovery observations, and may be very-low-burst-rate RRATs, or isolated burst events. These discoveries suggest that the initial population estimate was not excessive — RRATs, though not a distinct population, are indeed numerous.

In addition to finding new sources, characterization of their properties is vital. To this end, a campaign of regular radio observations of the newly discovered sources was mounted at the Parkes Observatory, in Australia. In addition, some of the initially identified RRATs were observed with the *Lovell Telescope* at Jodrell Bank. These have revealed glitches in J1819–1458, with anomalous post-glitch recovery of the spin-down rate. If such glitches were common, it would imply that the source was once a magnetar, neutron stars with the strongest-known magnetic fields of up to 10^{15} gauss. The observations have also been used to perform 'timing' observations of RRATs, *i.e.*, determination of their spin-down characteristics. At the beginning of this thesis, three of the original sources had 'timing solutions' determined. This has since risen to seven, and furthermore, seven of the newly discovered sources now also have timing solutions. With this knowledge, we can see where RRATs lie in period–period-derivative space. The Parkes RRATs seem to be roughly classifiable into three groupings, with high observed nulling fractions — normal pulsars, high-magnetic-field pulsars, and old, 'dying' pulsars.

It seems that RRATs and pulsars are one and the same. When a pulsar is more easily detected in searches for single bright pulses, as opposed to in periodicity searches, we label it a RRAT. Such searches impose a selection effect on the parameter space of possible sources, in both nulling fraction and rotation period. In this sense, an observational setup could be designed to make any pulsar appear as a RRAT. For realistic survey parameters, however, this is not the case, and the groups mentioned above seem to be the most likely to appear as RRATs. In fact, we can utilise RRAT searches to identify neutron stars, difficult to find by other means, in particular high-magnetic-field pulsars, and pulsars approaching the pulsar 'death valley'. Some of the RRATs are well explained as being distant/weak pulsars with a high modulation index, others seem to be nulling pulsars. This highlights the incomplete knowledge of nulling behaviour in the pulsar population. It seems that there may be a continuum of nulling durations, under a number of guises, from 'nulling pulsars' to RRATs to 'intermittent pulsars'. In fact this nulling may fit into the emerging picture, whereby pulsar magnetospheres switch between stable configurations. — *University of Manchester; accepted 2010 October.*

Building Blocks of the Galactic Stellar Halo

By Martin Niederste-Ostholt

In ΛCDM cosmology, structures form out of the gravitational collapse of dark matter. The growth of structure occurs in a hierarchical fashion from the bottom up, meaning that small, bound objects form first and then merge with other over-densities to form larger objects such as galaxies and clusters of galaxies. The theory of hierarchical structure formation is well borne out on large scales; however, the theoretical predictions are more problematic to match with observations on the scales of individual systems such as the Milky Way. Some of the fundamental questions include the number of observed satellite galaxies and their correspondence to the number of dark sub-haloes predicted in simulations, and how these satellite galaxies merge to form the stellar halo observed today.

In this thesis I take advantage of four different ways of exploring the stellar halo: using the large-scale distribution of stars, the properties of globular clusters, the properties of dwarf satellites, and numerical simulations. This allows me to look at the results of mergers and the traces left by mergers (the large-scale distribution of stars and the properties of globular clusters) as well as looking at the actual agents involved in the merging process (the dwarf galaxies and globular clusters). The primary goal of this work is to further the understanding of some of the substructure such as the ultra-faint dwarf galaxies and ultra-faint globular clusters, exploring their contribution to the Galactic halo, but also to use the available observational data to explore further a now-well-known piece of halo sub-structure, the Sagittarius stream.

Using the deep photometry from the Sloan Digital Sky Survey (SDSS) I have been able to identify extra-tidal features around the ultra-faint object Segue 1. Despite the noisy field in which Segue 1 is embedded, the features proposed are statistically significant. Independently, by fitting models to star counts around Segue 1, an excess of stars at large radii becomes apparent, again indicative of extra-tidal features. As it stands, the nature of Segue 1 remains to be determined. Neither of the three possibilities — globular cluster, dwarf galaxy, or the satellite of a dwarf galaxy — can be ruled out by the current observations. The same photometric-analysis technique, when applied to the faint dwarf spheroidal galaxies UMa II, Leo IV, Leo V, and Boo II, does not reveal any extra-tidal features. In contrast to this the photometric analysis of the ultra-faint globular cluster Pal 1 has revealed possible extra-tidal extensions. Due to Pal 1's low baryonic mass it is possible that the observed tails have formed as a result of the cluster evaporating.

The existence of tidal debris from the Sagittarius galaxy that spans vast areas of the sky is well established and does not require further confirmation. However, it is only now becoming possible to analyze its structure in detail by combining the coverage of the SDSS and 2MASS surveys. I estimate the total luminosity of the system by producing a luminosity density profile along the leading and trailing arms and extrapolating this. Sagittarius was likely the brightest and possibly the most massive of the so-called classical dwarfs. Prior to its disruption it was probably very similar to the M 31 dwarf ellipticals NGC 147 and NGC 185.

Inspired by the observed luminosity profile of Sagittarius, I investigate a number of simple *N*-body simulations of a Sagittarius-like satellite falling into a Milky Way-like potential to learn what controls the shape of the profiles and what may be learned from them aside from the total luminosity of the system. The success in matching the observed luminosity profile with a simple model containing only a baryonic component makes it unclear how much dark matter Sagittarius contained prior to disruption. — *University of Cambridge; accepted 2010 November.*

A full copy of this thesis can be requested from mno@ast.cam.ac.uk.

OBITUARIES

Brian Geoffrey Marsden (*1937–2010*)

Brian G. Marsden was born on 1937 August 5 in Cambridge, England. When, at the age of 11, he entered the Perse School in Cambridge, he was developing primitive methods for calculating the positions of the planets, leading to his introduction to the library of the Cambridge University Observatories — and his study of how eclipses, for example, could be precisely computed. Together with a couple of other students, he formed a school astronomical society, of which he served as the secretary. At the age of 16, he joined and began regularly attending the monthly London meetings of the British Astronomical Association. Brian quickly became involved with the Association's Computing Section, which was known specifically for making astronomical predictions other than those that were routinely prepared by professional astronomers for publication in almanacs around the world. During his last year of high school, he also became a junior member of the Royal Astronomical Society.

Brian was an undergraduate at New College, Oxford. In his first year there, he persuaded the British Astronomical Association to lend him a mechanical calculating machine, allowing him thereby to increase his computational productivity. By the time he received his undergraduate degree, in mathematics, he had already developed somewhat of an international reputation for the computation of orbits of comets, including new discoveries. He spent part of his first two undergraduate summer vacations working at the Nautical Almanac Office. After Oxford, he took up an invitation from Dirk Brouwer to cross the Atlantic and work at the Yale University Observatory. With the ready availability of the university's IBM 650 computer in the Yale Observatory building, he had soon programmed it to compute the orbits of comets. His PhD thesis in 1965 was on 'The motions of the Galilean satellites of Jupiter'.

At the invitation of director Fred Whipple, he joined the staff of the Smithsonian Astrophysical Observatory in Cambridge (Massachusetts) in 1965. There, Brian developed a way to incorporate non-gravitational forces directly into the equations that governed the motion of a comet. The involvement of the SAO with comets had been given a boost, shortly before Brian's arrival there, by the transfer there from Copenhagen of the office of the Central Bureau for Astronomical Telegrams, which is responsible for disseminating information worldwide about the discoveries of comets, novae, supernovae, and other objects of generally transient astronomical interest. Brian became Director of the CBAT in 1968, a post that he held until the undersigned succeeded him in 2000. In 1978, Brian assumed the directorship of the Minor Planet Center from Paul Herget (Cincinnati Observatory), which now handles observations and orbital/ephemeris computations for minor planets, outer-Solar System satellites, and comets.

Brian was instrumental in efforts to standardize how small Solar System bodies are observed and how their data are recorded and disseminated. He was a perfectionist, with little patience for sub-standard work, and he was quick to let his colleagues know of mistakes, but he also appreciated high-quality work, and he was a huge supporter of the many amateur astronomers who worked hard to reach Brian's high standards of quality in their observations. Brian was well appreciated for generously granting time to professional and amateur astronomers who wanted to learn from him, and this is surely one of his biggest legacies, as he helped many observers to hone their skills.

Brian is well known also for having used his careful assessment of historical astrometry to help observers recover numerous long-lost comets and minor planets. Brian was most proud of his correct prediction of the return of Comet Swift–Tuttle in 1992, which is the comet associated with the Perseid meteors each August. It had been discovered in 1862, but its 130-year orbit was not determined well enough to give great confidence in a predicted return. Brian suspected, however, that the 1862 comet was identical with one seen in 1737, and this assumption allowed him to predict successfully that Swift–Tuttle would return in late 1992.

Brian also worked extensively with comets that pass very close to the Sun, dealing with the coronagraph images from artificial satellites in recent decades to show that numerous dynamically similar groups appear, indicating relationships to several large individual comets that long ago broke into many smaller pieces. Brian became well known *via* the news media over the years for his involvement with 'transneptunian objects' (TNOs) — including, infamously, Pluto and its eventual numbering in 2006 as a minor planet — and with minor planets that come close to the Earth. Brian was supreme in the determination of preliminary orbits of small Solar System bodies — a skill that helped greatly with initial orbit determination for near-Sun comets and TNOs with relatively few observations.

Brian served on numerous Committees and Commissions of the International Astronomical Union. One rôle that he relished was as secretary for the past 32 years of what is now known as the IAU's Committee on Small Bodies Nomenclature. The CSBN attends to new names of minor planets (*via* the MPC) and oversees names of comets (through the CBAT). Brian very ably served to keep contention to a minimum through negotiating around many sticky naming issues.

Brian had an encyclopaedic memory for astronomical history and was keen on remembering many events and dates over the decades, including many astronomers' birthdays. Brian published prolifically over his career, and he especially enjoyed writing on historical astronomy. His editing of the MPC and CBAT publications over the years set an example for precision, giving the astronomical community immense respect for his work. Among Brian's many honours, minor planet (1877) Marsden was named in his honour by Paul Herget and colleagues.

Sadly, Brian fought a two-year battle with a bone-marrow disease that evolved into leukaemia this past summer. A pre-existing heart condition began to complicate matters, necessitating this past October a heart-valve procedure that led to a minor stroke. In mid-November, he came down with pneumonia, and his immune system could not fend off his death on November 18. A month later (as I write this), his death is still hard to comprehend. I was fortunate to have worked with Brian very closely for over 30 years. It gave me joy to do my own work because of his presence as my mentor and colleague. It will be impossible to replace his insight and experience. — DANIEL W. E. GREEN.

Allan Rex Sandage (*1926–2010*)

Astronomy lost one of its giants of the past century on 2010 November 13 with the passing of Allan Sandage.

Allan Sandage was born in Iowa City, Iowa, on 1926 June 26. His interest in astronomy began in childhood, and after attending Miami University and serving in the US Navy in the Second World War he earned a physics degree from the University of Illinois and a doctorate at Caltech, where he entered as one of its first PhD students in astronomy.

Few astronomers of any generation accomplished so much so early in their careers. Sandage studied under two other giants of 20th-Century astronomy, Walter Baade and Edwin Hubble, and worked as a postdoc at Princeton with Martin Schwarzschild. The combination of his thesis work on colour–magnitude diagrams of globular clusters with the modelling of post-main-sequence stars with Schwarzschild provided stunning new measurements of the cosmic age scale, and defined a new state of the art for stellar-evolution modelling. The work earned Sandage and Schwarzschild the Eddington Medal of the RAS in 1963, for outstanding merit in theoretical astrophysics.

However, it was as an observer where Sandage made his greatest mark. He effectively inherited Edwin Hubble's research agenda after Hubble's death in 1953, but Sandage's exquisite skill at the telescope was more reminiscent of his thesis supervisor, Walter Baade. He became a master at producing superb images out of the temperamental optics of the 200-inch *Hale* and 100-inch *Hooker* telescopes on Palomar Mountain and Mount Wilson, and unleashed their power on cosmology by its broadest definition: measuring the cosmic distance scale, the stellar age scale, and the deceleration of the Universe, exploring the collapse history of the Milky Way Galaxy, identifying Cepheids and other massive variable stars in galaxies, discovering and identifying radio-quiet quasars, and unravelling the morphological and physical natures of the Hubble sequence of galaxies. The extraordinary flood of seminal papers which followed fulfilled Hubble's agenda and beyond, and laid the foundations for much of today's extragalactic astronomy. Few observational astronomers have made such an enormous impact over such a wide range of subjects in their careers, much less over two decades. A series of honours followed, including the Gold Medal of the RAS, which was awarded to Sandage in 1967, at age 41.

Sandage's Halley Lecture for 1967, which is recorded in the pages of this *Magazine* (**88**, 91), reveals insights into his scientific vision and motivation. He was deeply impressed with the concordance between the expansion age of the Universe, the ages of the oldest stars, and the nuclear chronometric ages of the heavy elements as best measured at that time, and identified these with the creation of the world. As new telescopes on the ground and in space brought improved techniques to bear on these measurements the numbers changed, and with it the underlying cosmology paradigm, but the concordance of measurements that so impressed Sandage remains. The latter stages of Sandage's career were dominated by a protracted debate over the distance and age scales, and over time his repeated claims of Hubble constants measured with accuracies of a few percent, when independent determinations differed by factors of two or more, may have diminished a bit of the lustre of his earlier accomplishments. Nevertheless, Sandage continued to make major contributions to astronomy, not least his work on calibrating type-Ia supernovae as cosmological standard candles (now one of the prime paths to both the Hubble constant and the acceleration measurements), and a magnificent series of atlases of galaxies which distil a career of wisdom and insight into how galaxies are put together, and realize the culmination of yet another unfinished project he inherited from Hubble.

Sandage's professional *persona* mirrored his almost larger-than-life impact on our field in the latter half of the 20th Century. He had a commanding presence which would dominate any gathering he attended, and an irrepressible enthusiasm for discovery and learning that rubbed off on anyone who interacted with him. My own interactions with Sandage reveal some insights into this complex and brilliant man. As a child I read a popular account of Sandage observing on the 200-inch telescope, and from then I aspired to become an astronomer myself. My PhD thesis was on the Hubble constant, re-examining Sandage's measurements of H II regions which were a critical element of the distance ladder at the time. When I finally met the great man in 1978, as a new Carnegie Fellow in Pasadena, he greeted me as if I were a family member, and offered to meet me at any time to discuss any topic — except for the Hubble constant, where he took strong issue with my work. He was good to his word on both counts. Although we met frequently in the succeeding 25 years we never did discuss the distance scale, and he never accepted my work; yet he supported the work without fail, on the Kitt Peak committee that awarded the observing time, in the Fellowship selection process, even sending me his research notebooks so I could compare our raw measurements in detail. That is the way I shall remember him as well, as someone with whom I often disagreed, but whom I respected and admired immensely.

Allan Sandage is survived by his wife, Mary Connelly, and sons David and John. Many among us will join them in missing this extraordinary astronomer and individual. — ROBERT KENNICUTT.

John Baldwin FRS (*1931–2010*)

John Baldwin, who died on 2010 December 7, had an enormous influence on the development of radio astronomy, and later of optical interferometry. He was also a wonderful and inspiring colleague.

John went to Queens' College Cambridge in 1949, and on graduation in 1952 joined the group led by Martin Ryle in the Cavendish Laboratory, as part of what was then known as the 'Radio Group', from its long history of work on ionospheric physics. He was one of the 'second generation' of radio astronomers; Martin Ryle was his supervisor. John worked largely on Galactic objects and radio emission in those days, and he took his PhD in 1956. At first he used the *Cambridge Radio Telescope*, the 4-element interferometer which had been used for (among other things) the 2C and 3C surveys. At about that time the Group acquired the Lord's Bridge site, known as the Mullard Radio Astronomy Observatory from the generous benefaction from Mullard Ltd. One of the first projects on that site was an ambitious 38-MHz 'T' synthesis telescope, largely home-made, with which John, Phil Williams, and Sidney Kenderdine produced the major survey of the Northern sky.

In the early 1960s, the *One-Mile* telescope was built on the site as the first purpose-built Earth-rotation synthesis instrument. John saw an opportunity: here was a beautiful piece of rail-track, not much traffic, and he found a way (aided by the Royal Society's Paul Instrument Fund) to construct the *Half-Mile* telescope on it, using small, second-hand dishes — two at first, then four. The revolutionary part of that project was that it had a spectrometer as a receiver, and here was the first 21-cm-line synthesis telescope, carefully matched in its properties to the well-known nearby spirals. For a while during this period he was also an Editor of *The Observatory*.

John's interest in low-frequency astronomy continued, and over many years he conceived and brought to completion the 6C, 7C, and 8C surveys, the last two being observed with the *CLFST* (*Cambridge Low-Frequency Synthesis*

Telescope) built in the 'spare' sections of the old railway track that was used for Martin Ryle's *5-km Telescope* (another example of spotting a useful piece of infrastructure). However, John was one of the few who really did understand interferometry, and his thoughts turned to the optical part of the spectrum. The principles might be similar, but the practice is very different. He made observations with large optical telescopes using aperture masks, and then set about building a test-bed interferometer at Lord's Bridge, with baselines up to about 100 m and eventually five 50-cm apertures. The direct descendant of this device is now being built as a joint project with the Cavendish in New Mexico, the *Magdalena Ridge Optical Interferometer*.

John had been promoted to Reader in 1981, and Professor in 1991. He formally retired in 1999, but of course kept on working. We will remember John's good humour, generosity, and friendship. We all send our condolences to Joyce Baldwin. — GUY POOLEY.

Here and There

... AND THEN SOME!

In the case of KBO 55636, the authors deduced an albedo ranging from 82% to more than 100%, making it one of the most reflective objects in the Solar System. — *Nature*, **465**, 879, 2010.

BETTER CALL A TECHNICIAN THEN!

29 glitches detected at Urumqui Observatory — *MNRAS*, **404**, 289, 2010.