THE OBSERVATORY

Vol. 129 2009 AUGUST No. 1211

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2009 January 9th at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

A. C. FABIAN, *President*
in the Chair

The President. I would like to wish everyone a happy New Year.

First, I'd like to announce the awards for 2009, as agreed at the last Council Meeting. The Gold Medal of the Society for astrophysics is awarded to Professor David Williams of University College London, and the Gold Medal for geophysics to Professor Eric Priest of St. Andrews University. The Eddington Medal is awarded to Professor James Pringle of Cambridge University, the Jackson-Gwilt Medal to Professor Peter Ade of Cardiff University, the Price Medal to Professor Malcolm Sambridge of the Australian National University, the Fowler Award for Astronomy to Dr. Sarah Bridle of University College London, and the Fowler Award for Geophysics to Dr. David Tsiklauri of the University of Salford. The Award for Service to Astronomy goes to Professor Sir Arnold Wolfendale of Durham University, and the Award for Service to Geophysics to Dr. David Kerridge of the British Geological Survey in Edinburgh. The Group Achievement Award goes to the *SCUBA* team. We've also awarded honorary Fellowships of the Society to Professor Matthew Colless of the AAO in Australia, Professor Janusz Sylwester of the Space Research Centre of the Polish Academy of Sciences, and Professor Bernard Schutz of the Gravitational Physics Institute in Potsdam. On the geophysics side, honorary Fellowships go to Professor Joe Burns of Cornell University, Professor Jitendra Goswami of the Physical Research Laboratory in Ahmedabad, India, and Dr. Athelstan Frederick Spilhaus, retiring Executive Director of the American Geophysical Union.

We'd like to congratulate two Fellows who received MBEs in the New Year Honours list: Dr. Maggie Aderin, senior project manager at Sira Technology Limited, who worked on a satellite detection system funded by ESA. She's also set up Science Innovation Ltd. to promote public understanding of science, and she's certainly very dynamic at doing that. Dr. John Mason is the former president of the BAA, a well-known broadcaster, and founding trustee of the South Downs Planetarium and Science Centre in Chichester. Congratulations to them on the award of their MBEs.

Now we move to the programme. Our first talk is by Professor Fran Bagenal, from the University of Colorado, who will be talking to us on 'The giant magnetosphere of Jupiter'.

Professor Fran Bagenal. [No summary was received at the time of going to press. The speaker described Jupiter's magnetosphere, emphasizing the important rôle of Jupiter's moon Io, which injects tonnes of ionized material into the magnetosphere. Io has been known to have a strong rôle in interacting with the Jovian magnetosphere since early observations of radio emission. Tidally driven eruptions release sulphur compounds into Io's orbit and become ionized in a torus. The UV glow of the torus, as observed by the *Cassini* spacecraft, arises from multiple ions of sulphur and oxygen which produce emission lines in the EUV from 600–1900 Å, from which the plasma properties can be deduced. Material from the Io plasma torus is observed to move between 6 and 60 Jupiter radii on a timescale of tens of days.

One manifestation of the dynamics in the magnetosphere is the aurorae observed on Jupiter: these include a persistent kidney-bean auroral oval, due to plasma from Io, and an auroral spot from electron precipitation due to Alfvén waves propagating from Io to the planet. Near the polar regions, flickering aurorae are observed, associated with the dynamics of the outer magnetosphere.

The speaker concluded by briefly describing observations of the magnetosphere by the *New Horizons* mission *en route* to Pluto, which was the first spacecraft to explore Jupiter's magnetotail, and highlighted the key objectives of the forthcoming *Juno* mission, due for launch in 2011 and arriving at Jupiter in 2016. *Juno* will operate in a low, polar, elliptical orbit, and will use particle and field instruments to study Jupiter's internal structure, gravity, and environment, providing key data on auroral processes in the polar magnetosphere, as well as measuring Jupiter's thermal radiation and the abundance of water.]

The President. Thank you. You talked about *Juno* flying over the pole in two hours; what happens to it then?

Professor Bagenal. It's in an elliptical orbit — the apojove is out at about forty planetary radii; the spacecraft passes perijove very quickly and then spends a lot of time sending the data back to Earth and recovering, and then it moves back in again. We hope to get about thirty orbits.

Professor D. Lynden-Bell. I have a question that is only peripherally related to what was said. The ionosphere of Jupiter must rotate sitting on the neutral Jupiter, which rotates about one axis. That axis differs by about ten degrees from the axis of the magnetic pole, at least the north magnetic pole. That fact that the ionosphere is trying to rotate about a different axis to the axis of the magnetic field means that there's an awkwardness there. This must drive currents through the magnetosphere of Jupiter along the field lines in order to make these two systems work together. Have those been observed or is that not a subject you know about yet?

Professor Bagenal. Well, you've touched upon a topic of great interest. If everything were rigidly co-rotating then everything would be really easy. But the reality is that the magnetosphere opposes these stresses on the ionosphere, so the ionosphere doesn't quite co-rotate. So then you have to worry about the coupling between these different systems, indeed, and that is actually where the key research is focussed. There's an added complication to this, in that if the plasma is highly confined by centrifugal forces towards the equator, there's a region at high latitudes where there's very low plasma density. And yet you want to drive currents that couple the plasma to the ionosphere, but you're suffering from a lack of current carriers. So what happens in most plasmas, the Earth's magnetosphere being the primary example, is that you develop drops in potential: electrostatic potentials that can then accelerate the particles to carry the current. So there probably is a de-coupling not only between the ionosphere and the atmosphere at some level that will indeed result in current systems, but also you have to worry about the fact that there may be a de-coupling of the magnetosphere from the ionosphere because of the lack of current carriers at high latitude. You've picked up on what is one of the interesting active areas of research. In fact the UCL group that studies magnetosphere–ionosphere coupling for the Earth has been applying this to studies of Jupiter and Saturn, and I think they're the best group in the world working on this.

Professor E. R. Priest. Is there a good explanation for that radial-transport timescale you mentioned regarding the Io disc?

Professor Bagenal. The current ideas are that you have flux-tube interchange — that is, a dense plasma that's inside is unstable for centrifugal-driven interchange and you have empty flux tubes that interchange spontaneously with full flux tubes that are inside. The exact way in which that all happens is not clear, and how you get a timescale out of all that is not obvious. I don't think we understand the detailed plasma physics of how that works. We can say that's how it happens but the reality is not so clear. The timescale of tens of days comes from modelling the physical processes and they tell you that this is the sort of timescale that you must have in order to have the densities and temperatures for a given source. I think that's a fairly robust number, but I don't think the physical process is well understood.

Dr. S. Eyres. Do the interactions you have described have long-term implications for Io's orbit? Will it gradually spiral towards or away from Jupiter?

Professor Bagenal. Yes, it is indeed losing momentum.

Professor Lynden-Bell. It is in resonance.

Professor Bagenal. It is in resonance, that's true, so one might argue that Europa might constantly be tugging on it. The question is, how does the energy that it loses because of its electrodynamic interaction compare with the energy it might gain from the gravitational coupling, the tidal coupling that it has with Europa. I don't know that number. Anybody?

Professor Lynden-Bell. I don't know it precisely but I do know that essentially what happens is that the angular momentum is delivered to Europa, and so that will gradually push Europa outward. Wait, maybe inwards! [Laughter.] Jupiter is rotating rapidly, isn't it, so it's outwards.

Professor Bagenal. My bet is that the gravity wins heavily over the electrodynamics. The electrodynamics have exciting phenomena in terms of radio and plasma physics but dynamically, not much.

The President. Thank you very much, Fran, for a very interesting talk.

Our next talk is on 'High-sensitivity polarimetry: from exoplanets to Saharan dust', by Professor Jim Hough from the University of Hertfordshire.

Professor J. Hough. To date there are no detections of the reflected light from exoplanets. Most are discovered by indirect means such as radial-velocity surveys or transits. Images of exoplanets have recently been produced but these are for young planets that are still sufficiently warm that they can be detected directly at infrared wavelengths and in nearly all cases they are at very large distances from the central star. Detecting reflected light from unresolved star–planet systems is non-trivial as the expected change in brightness of the star plus planet, as the planet orbits the star, is very small (~ 100 μmag) and beyond what can be achieved from the ground and still very challenging from space telescopes. The reflected light from exoplanets will be polarized but the fractional polarization, after dilution by the unpolarized central star, is most likely parts per million. Detecting the polarized light as a function of orbital phase angle provides the orbital inclination from the position angle of polarization, the magnitude of the peak polarization provides information on the albedo and size of the planet, and the phase of peak polarization provides information on the size and nature of the scattering particles.

As polarimetry is a differential technique it is possible to achieve very high sensitivities, and *PlanetPol*, a stand-alone polarimeter constructed by the University of Hertfordshire, has achieved fractional linear polarizations to better than one part in a million with an absolute accuracy of ~ 1%, when mounted at the Cassegrain focus of the 4·2-m *William Herschel Telescope* (*WHT*), La Palma. *PlanetPol* is an aperture polarimeter operating between 0·45 and 0·95 μm, with both object and sky channels. A major problem in making such measurements is the telescope polarization, which has never been determined for any telescope at fractional polarizations of parts per million. On the *WHT*, an alt-azimuth telescope, the polarization of a number of nearby stars was determined as a function of parallactic angle with the de-rotator enabled. In this case the telescope polarization varies while any intrinsic polarization of the star and line-of-sight interstellar polarization remains fixed. If the only polarization is that produced by the telescope then the two measured Stokes parameters will vary sinusoidally with parallactic angle, with amplitude equal to the telescope polarization and phase shifted by 45 degrees. Fortunately many stars do have very small fractional polarizations enabling this technique to work very effectively. For the *WHT* the telescope fractional polarization is only ~ $1{\cdot}5 \times 10^{-5}$, making it an ideal telescope for very-high-sensitivity polarimetry. *PlanetPol* itself is measured to have an instrumental polarization of ~ 2×10^{-6}. As a by-product of the observations a number of polarized standards have been established with fractional polarizations of a few parts per million.

To date, we do not have a positive detection of the reflected light from three observed extrasolar planets, the so-called hot-Jupiters τ Boo b and υ And b, and the hot-Neptune 55 Cnc e (hot Jupiters and hot Neptunes are typically within 0·1 AU of the central star). Upper limits on the albedos, calculated using modelling of the planetary atmospheres by Phil Lucas (Hertfordshire), are typically < 0·3 (4-σ upper limits).

During observations made in 2005 May we found that our standard, very-low-polarized, stars had fractional polarizations an order of magnitude higher than before. Initially we suspected that there was a problem with either the instrument or the telescope. It turned out, however, that a layer of Saharan dust above La Palma was producing the additional polarization. The Saharan dust event, quite visible in the sky, lasted for about three days; thereafter the polarizations of the standard stars returned to their normal values. The additional polarization increased with the zenith angle of the observed stars and correlated well with the optical depth of the dust as measured by the *Carlsberg Meridian Telescope* on La Palma and aerosol monitors such as *AERONET* on Tenerife, with the latter giving the size of the dust as typically between 1 and 10 μm. It was possible to rule out the polarization being produced by reflection of light from the Saharan dust (there was no Moon) and the only possible explanation was dichroic absorption (*i.e.*, differential absorption) of light by aligned dust in the atmosphere, a phenomenon not previously known. The observed position angle of polarization required the dust to be aligned vertically, not horizontally as would be expected from aerodynamic drag as particles fall through the atmosphere. Modelling of the distribution of particle orientations by Joseph Ulanowski (Hertfordshire) shows that an electric field of ~ 1 kV m^{-1} could align dust grains vertically for sizes of a few microns to 20μm. Smaller dust grains would not have any preferred orientation and much larger particles would align horizontally as aerodynamic drag would be the dominant force.

The electric field can be produced by charging of particles arising from the triboelectric effect, or if the dust layer is relatively thin (less than a few hundred metres) by a reduction of atmospheric conductivity arising from the presence of dust, although this is not thought likely with a dust event that was six days old by the time it reached La Palma. Knowing the distribution of dust orientations and typical size of dust grains it is possible to calculate the expected polarization, and this agreed very well with the additional polarization that was observed during the dust event.

Aerosols are presently the largest uncertainty in atmospheric and hence climate modelling, acting as an efficient source of nuclei for cloud formation and modifying both short- and long-wavelength radiation. Presently all models assume that dust particles are randomly aligned. The alignment of dust can significantly affect aerosol retrievals, changing the transmitted flux by up to 10%, the measured phase function by up to 20%, and the measured degree of polarization from satellite polarimeters such as *PARASOL* on *A-Train* and the polarimetry sensor on *GLORY*. The charging of dust might also explain why large dust particles are found far from the source of the dust event, as these should have gravitationally settled. The coupling of large and small particles *via* the electric field effectively slows down the settling of larger particles (the larger particles tend to be positively charged and the smaller particles negatively charged).

Further work includes additional observation of exoplanets with improved detectors, now included in *PlanetPol*, and NERC-supported field campaigns in the Middle East to study the origin, magnitude, extent, and impact of the phenomenon of the alignment of atmospheric dust particles.

Professor D. W. Kurtz. When you look at the dust under high-resolution microscopy, what is its shape?

Professor Hough. Well, it's certainly not spherical. The answer to that is that you get all sorts of shapes, all non-spherical. They're really just extremely varied.

Professor Kurtz. The reason I ask is that you gave the example of a piece of paper floating down, but of course ice crystals which are needle-like do align vertically.

Professor Hough. Certainly it has been known for some time that cirrus ice particles can become aligned. Apparently in storm conditions, where you get extremely large electric fields, the cirrus ice particles can align vertically. But they're much bigger than what we're talking about here, with much bigger electric fields involved, tens of kV per metre, whereas we're talking about much smaller fields. And of course the Saharan dust is much longer-lived.

Rev. G. Barber. How does this relate to studies of dust particles and dust storms on Mars, and possibly ice particles?

Professor Hough. Well, it's interesting that people have postulated whether alignment of Martian dust might actually be taking place. One of the papers published a few years ago of *HST* polarimetry observations of a Martian dust storm postulated that we might be seeing alignment of dust grains in the Martian atmosphere. But they actually thought it was too speculative and withdrew that speculation from the published paper. But we do have an interest in comparing what we're seeing with what might be happening in the Martian atmosphere.

Professor T. Ray. Why do the large grains charge positively and the small ones negatively?

Professor Hough. Good question. It's not clear from the literature that people totally understand this, so it's more of an observational result rather than an entirely theoretical one. Apparently what happens is that particles rub past each other, and what actually gets transferred depends very much on the nature of the surface at the point of interaction, for example, how round the surface is at the point of interaction. Essentially what you're trying to do is to equalize the electro-chemical potential between the two grains, but quite what happens isn't clear, or if it's clear to other people it isn't entirely clear to me. It seems to be quite easy to think of circumstances where you get entirely different polarity on the dust grains. As I understand it, in the atmosphere, invariably it is the large grains which have the positive charge. Does anybody know anything more about it? Oh well, maybe I'm safe in saying that! [Laughter.]

Professor M. Lockwood. It's not a question, just a comment: I think it's the same as the reason why the dust moves on the Moon. What the astronauts reported was much higher than people expected.

The President. Jim, how far away are you from detecting an extrasolar planet? Are you a factor of three away, or of ten away?

Professor Hough. Well I suppose we don't know because we don't really know what the albedos of these planets are. My best guess would be less than a factor of two. In fact, although we haven't had any time spent yet, for a number of reasons, we have put new detectors on the instrument which give a 40% improvement in signal to noise. So we think we stand a fair chance, with a better selection of objects.

If we could get on an 8-m-class telescope it would be much easier, but this is a huge problem: very few 8-m-class telescopes welcome private instruments, particularly at the Cassegrain focus. To give a plug for the *WHT*, I have to say it is one of the few telescopes, albeit 4-m aperture, that not only allows private instruments but welcomes them. I think that's extremely important, and the *WHT* does actually have a fair number of private instruments, whereas most other observatories shun them and simply don't want to know. Perhaps they would put them on at Nasmyth, but for the work we do the Nasmyth focus just isn't appropriate.

The President. Our next talk is by Professor Iwan Williams of Queen Mary College, and it is on 'Fireballs, meteorites, and parent bodies.'

Professor I. P. Williams. Over the last few months a number of sightings of fireballs have been reported, a particularly noteworthy one being over Edmonton, Canada, on 2008 November 21 and filmed by a police video camera. A meteorite associated with this fireball has since been located on the ground. Also newsworthy was the impact over Sudan on 2008 October 7 that had been predicted following the observations of the parent from an NEO-monitoring site. Fireballs are at the brighter end of the meteor spectrum, traditionally taken to be brighter than Venus, or roughly a magnitude of –5. Just to place this in context, the Moon is –12·7 and the Sun –26·8. As a very rough guide, a typical meteor of magnitude 4·5 is the end phase of a mm-sized object while a 1-cm object will produce a meteor of magnitude –3. For a fireball to be as bright as the Moon, the original meteoroid would be about 20 cm, while a fireball of magnitude –18 would represent a metre-sized object. To be as bright as the

Sun (which is what the Tunguska event probably was) the progenitor would be about 50 m in size.

Now, the maximum size of object that can lifted from the surface of a comet by gas drag is of the order of a few centimetres. Hence traditionally it is assumed that most meteors are associated with comets but that the larger ones are associated with asteroids. The only way of confirming the likely parentage is by determining the pre-encounter orbit. This requires multiple observations of the fireball from a number of different sites, so that the height in the atmosphere can be determined by parallax. Starting in the late 1950s, several fireball networks were set up, the most successful being the European Network, the Prairie Network, and MORP. The last two have long since closed down but the European Network has been recently expanded and modernized and a new network set up in Australia, and there are plans for one in Tajikistan.

The first success was the observation and recovery of the Pribram Fireball and meteorite observed fifty years ago on 1959 April 7. The Prairie Net and MORP also had a success each with Lost City on 1970 January 4 and Innisfree on 1977 February 6. There are now nine fireballs that have a determined orbit and an associated meteorite located. Worthy of mention amongst these is the Neuschwanstein Fireball seen on 2002 April 6, at an almost identical time of year to Pribram. The orbits of these two are also virtually identical. However, their cosmic-ray-exposure ages are very different, at 18 and 48 My. It is generally believed that two orbits cannot remain similar on this type of orbit for that length of time owing to Jovian perturbations. However, it has been suggested that both were originally surface features on a 'rubble-pile' type of asteroid and released as independent bodies much later.

Of particular interest is the fireball observed by a team led by Dr. Josep Trigo-Rodriguez from the Spanish Meteor and Fireball Network on 2008 July 11 and now called the Bejar Fireball. Details of this can be found in a recent paper by Trigo-Rodriguez and his colleagues in *Monthly Notices*. This fireball reached a maximum brightness of –18 and the atmospheric deceleration indicates a size of a little over 1 metre. The usual calculations indicate a strength of the order of 10 MPa, much in line with that associated with the other fireballs mentioned. The reason why this particular fireball is interesting lies in its orbit. All the previous fireballs have their aphelia within the orbit of Jupiter and a Tisserand Criterion in excess of 3, in other words a typical near-Earth-asteroid orbit. The Bejar Fireball orbit extends out to the Saturnian orbit and has a Tisserand Criterion of 2·2, in other words a typical Jupiter-family comet orbit. Further, the orbit is very similar to the mean orbit of the o Draconid meteor stream that is also usually observed in early July. Of course, at first sight, this presents a problem since I have already stated that metre-sized bodies cannot be lifted off a comet nucleus. However, recently a number of papers have suggested that the disintegration of a comet nucleus can also be a meteor-stream-forming mechanism, the Quadrantids being the best known. We have recently witnessed several fragmentations and outbursts in comets, Shoemaker–Levy 9, 17P/Holmes, and 73P/Schwassmann–Wachmann being obvious examples. The only suggested parent for the o Draconid stream is comet C/1919 Q2 Metcalf, a comet that indeed disintegrated. Whether or not Metcalf is actually the parent is almost irrelevant — the parent is almost certainly a comet that disintegrated. Hence at least one cometary nucleus must have had a metre-sized body of considerable strength residing within it. It should be remarked that several bodies of a similar size or greater are also associated with the Quadrantid and Taurid streams. Whether such bodies are common in all cometary nuclei or whether their presence is a function of the collisional history of the particular nucleus as it evolves from the outer Solar System to its present status as a Jupiter-family comet is an interesting question for the future. The *CONSERT* experiment on *Rosetta* might throw light on this, at least to the extent of telling us whether or not one other comet nucleus has such large boulders within it.

Dr. S. Russell. Is it possible to say anything from the colour or spectrum of the fireball about the composition of the object?

Professor Williams. Spectra are somewhat lacking, unfortunately. As you can appreciate, observations of fireballs are serendipitous. There is a sort of spectrum available from small telescopes, but the spectrum is not all that different from any other spectrum of a fireball.

A Fellow. On the television earlier this week, there was a picture of a windmill which had got one of its blades bent, and in today's paper it said it was an object the size of a cow! [Laughter.]

Professor I. P. Roxburgh. It depends on which newspaper! [Laughter.]

Professor Bagenal. If it was a metre-sized object coming in, how big do you think it is on the ground?

Professor Williams. It fragmented. There were four distinct explosions, which tells you something about the strength. So it fragmented four times, and the final one was the brightest. So it disintegrated into smaller bits.

Professor Bagenal. Do you expect to see something the size of a cricket ball?

Professor Williams. Yes, so we're now looking for things the size of cricket balls.

Professor Bagenal. So my other question is, what about observations from space? Have you asked the appropriate authority whether they saw anything? Or maybe we'd have to kill you if you answered that! [Laughter.]

Professor Williams. Yes! There were three detections during that period from space.

Mr. C. R. Barclay. How far in advance of the impact did the detection of the Sudan NEO occur?

Professor Williams. It was a matter of days.

A Fellow. I'd like to offer an alternative to the idea that you have to have a rocky chunk coming out of the Kuiper Belt. One thing we've done is to use aerogels to capture the high-speed particles from comets. I'm just wondering if the comet itself, which is relatively weak, can act like an aerogel and sweep up something from the asteroid belt as it happened to go through it?

Professor Williams. People show pictures of the asteroid belt being highly congested, but I think the probability of actually hitting an asteroid or comet is small.

Mr. H. Regnart. This, I think, is very much a footnote. I had one experience of seeing a fireball, very bright indeed, where the meteorite was actually found somewhere in Ireland later. It turned out to be at the time an experience of terror as well as extreme beauty, as it happened at a time of particularly high tension between Russia and the United States, and we feared that it was actually re-entry of a thermo-nuclear warhead. Fortunately we were mistaken!

Professor Williams. There was also a similar case, back in 1956 I think, in Beddgelert, North Wales, where a meteorite actually fell through the roof of the Prince Llewelyn Hotel, and landed on a bed in a bedroom.

I should also clarify the point that there are lots of cases of meteorites found on the ground which can be paired with fireballs. It's only nine where we have orbits too.

The President. Comet Holmes also did something twice: is that another example of comets not being simple?

Professor Williams. Yes.

The President. I'd now like to present the Michael Penston astronomy thesis prize to Dr. Joern Geisbuesch, of the Cavendish astrophysics group, for his thesis entitled, 'Cosmology with Sunyaev–Zeldovich cluster surveys'. [Applause.]

Dr. J. Geisbuesch. Perhaps I could say a few words. First of all, I am very honoured and grateful to receive this prize. I want to thank the people who supported me — my supervisor, my research group in Cambridge, and my parents. To say a few words about my thesis: the evolution and number counts of galaxy clusters very much depend on the assumed underlying cosmology, so you can use them as a tool to constrain cosmological models. For my thesis, I used the S–Z effect to detect galaxy clusters. I calculated simulations of the S–Z effect and created pipelines for upcoming instruments to simulate the observations, as well as how many clusters will be detected and to what redshift. The S–Z effect is especially useful because it is redshift-independent, and it is good for detecting these clusters; and I used these estimates to show how they depend on cluster physics and which predictions you might get from upcoming surveys to help constrain cosmological parameters.

The President. Thank you very much, Joern. We're very grateful to David Kipping of UCL, who gave a talk at one of the specialist discussion meetings today, and will speak to us on 'The detection of exomoons'.

Mr. D. Kipping. Four-hundred years ago Galileo Galilei discovered the moons of Jupiter and I think it is fitting today to start to think about moons outside of our Solar System, so-called exomoons. Over the last decade the number of known extrasolar planets has soared from a handful to well over 300 but we have yet to embark on any kind of serious investigation as to whether such planets harbour satellite systems. Based on our Solar System and adopting a Copernican view, we would presuppose that exomoons are common throughout the Universe but only today can we really start to test this hypothesis.

From an astrobiological viewpoint, exomoons could be incredibly interesting and provide two key functions. Firstly, it is quite possible that exomoons could be more frequently habitable environments than exoplanets themselves. Out of the 300-plus exoplanets discovered, around 30 of them are classed as being in the habitable zone of their host star. This means that these planets have equilibrium temperatures between the boiling and freezing points of water. Despite this, they are not actually habitable planets because they are gas giants like Saturn and Jupiter and it is generally believed such planets are not suitable for complex life. However, consider that if just one of these 30 exoplanets had one or more large moons, you would have to say that this would be an extremely good candidate for a life-supporting environment.

A second point about habitability and exomoons is the case examples of the Earth and Mars. Mars does not have a large moon like the Earth does and this is believed to have significant implications for the planet's habitability. The Earth's Moon is sufficiently massive that it acts to stabilize the precession and axial tilt of the Earth. This stabilization may in fact be vital for complex life. Without a large moon, like with Mars, the axial tilt varies dramatically over millions of years causing extremely variable climatic conditions. Although simple life would surely adapt to such changes, it is thought that more complex life may suffer.

I hope I have persuaded you that such a search is worthwhile, but you may rightly ask how one conducts such an audacious search when even detecting exoplanets is challenging. The first thing you might try is direct imaging, which has recently demonstrated impressive results with the Fomalhaut and HR 8799 systems. However, these detections were of young planetary systems on distant orbits and do not represent typical planets. One figure to bear in mind is that the angular size of the Earth is around 0·02 microarcseconds seen from 10 parsecs, whereas current interferometric precision is around 25 microarcseconds. So although direct imaging is possible, it is likely not to be feasible until improved interferometers are available.

What about radial velocity? Astronomers have detected the bulk of exoplanets by watching the reflex motion of stars, and so it may seem sensible to try to extend this incredibly successful technique to exomoons. Unfortunately, the reflex motion of a star due to Jupiter plus all of its satellites is no different from the reflex motion due to a slightly heavier version of Jupiter. In other words, radial velocity effectively only sees planets as point masses and thus no information about potential satellite systems can be extracted.

The next approach you might try is the transit method. When a planet passes in front of a star, it causes a dip in the amount of light and this can be used to determine the planetary radius. The same principle holds true for moons but here the planet and moon signal are convolved together. Since the planet signal will always dominate, the slight moon-dip can only truly be seen once the planet has left the limb of the star. This means that the maximum time you can see the moon dip is the orbital distance between the planet and the moon divided by the velocity of the planet around the star. This time is typically on the order of one percent or less of the transit duration and that means a much shorter integration time. So even with something like the *Kepler* space telescope, which is designed to spot Earth-like planets with six hours of integration time, you are highly unlikely to detect a moon photometrically.

Before we leave the transit method for dead though, there is some light at the end of the tunnel. Transits should occur once every orbital period, but if there is a moon present it can make the transits occur slightly earlier or slightly later than expected. This transit-time-variation effect, or TTV, is based on the simple principle that a planet and a moon orbit a common centre of gravity which itself orbits the host star. The TTV signals can be up to a minute in amplitude, which is quite detectable from even ground-based observatories. Unfortunately, even TTV is not immune to serious problems in exomoon detection.

The first problem is that not only moons cause TTV. Perturbing planets, stellar peculiar motion, General Relativistic effects, stellar quadrupole moments, parallax effects, and Kozai migration, to name a few, are all predicted also to cause transit-timing effects. Determining whether what you are seeing is a moon or one of the above is profoundly difficult given the low signal-to-noise amplitudes we are dealing with. A second problem with TTV is that it only provides the mass of the exomoon multiplied by the orbital distance of the moon. In other words, TTV is unable to tell us the masses of exomoons.

It is at this point that my work steps in and I tried to solve these problems. The solution lies in considering the planetary motion once again. TTV is all about the position of the planet changing, so what about velocity? It is easy to see that the velocity of the planet also changes depending on the phase of the accompanying moon. Sometimes the planet receives a speed boost and sometimes a decrease in speed. Changes in speed mean changes in transit duration which implies transit-duration variation, or TDV.

Combine TDV and TTV together and you have something quite beautiful.

Firstly, they are 90 degrees out of phase with one another, leading to a unique signature for moon identification. Secondly, the ratio of TDV and TTV provides the mass and the orbital distance of the exomoon separately. Thus we have resolved both of the original drawbacks of the TTV effect by using TDV in combination.

In conclusion, we now have the tools to detect exomoons through careful monitoring of exoplanets and in the coming years we can look forward to answering the question as to whether exomoons exist. I think this again outlines how rapidly the field of exoplanets is evolving in our struggle to understand our place in the Universe and hopefully to provide answers within a generation.

Professor Kurtz. In your last case where you're considering an M-type dwarf, did you put into your model the problem of stellar activity, and what that's going to do?

Mr. Kipping. No, but we assume that even with stellar activity you can currently measure the transit to a precision of 10–20 seconds.

Professor Kurtz. A case for you to look at perhaps, if you haven't already, is Corot Exo-2b, as many transits have been measured but the star is also very active.

Mr. Kipping. In this case I considered a Gliese-436b-like planet, and I don't think the activity of that M-dwarf is too large, so if you have a quiet M dwarf you'd certainly be OK. For others it could be a problem.

The President. In terms of having large planets, if it's like our own Solar System they could well have lots of moons. Have you thought about the consequence of that?

Mr. Kipping. Yes, if you have lots of moons it actually dampens out your signal; but if they're in resonance with each other then you can still observe a signal. If they're not in resonance then it just washes out. So you do need some ideal targets.

The President. Maybe we'll get lucky! Thank you very much again, David. The meeting is now going to close, and the next meeting will be on Friday, February 13.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2009 February 13th at $16^{h}\ 00^{m}$
in the Geological Society Lecture Theatre, Burlington House

A. C. FABIAN, *President*
in the Chair

The President. Welcome to the A and G meeting. Please make sure all mobiles are switched off, which is what I'm doing now. The first talk today is by Dr. Susan Whitfield from the British Library and Professor Jean-Marc Bonnet-Bidaud on the 'The Dunhuang Star Chart — a mystery of Chinese astronomy'.

Dr. Susan Whitfield. The Dunhuang Star Chart is a unique document, testament to the rich historical tradition of Chinese astronomy. Research on the Star Chart over the past four years has confirmed its position as the earliest manuscript star chart and has pushed back the date of its origin by over 250 years. Today, we will give a very brief introduction to how and where this manuscript was found and then look at it in terms of the Chinese astronomical tradition, and as a scientific document.

Dunhuang was an important market and military town on the Silk Road in the first millennium of our era. It is now in Gansu Province in western China. Buddhism thrived along the Eastern Silk Road and believers paid for the construction of cave temples, like those in India. One such complex was constructed about twelve miles southeast of Dunhuang, consisting in its heyday, the 7th and 8th Centuries, of almost one thousand cave temples honeycombing a cliff face overlooking a river. The cave walls and ceiling were completely covered with Buddhist paintings, and they also contained statue groupings.

Cave 16 was built by the local Buddhist abbot in the 9th Century. He constructed a small meditation chamber off the corridor. After his death, this was converted into a memorial chapel. For some unknown reason, in the 10th Century his statue was moved out and the small chamber was filled floor to ceiling with manuscripts and paintings, 40 000 in total and mainly Buddhist works. The cave door was then sealed, plastered over, and hidden by a painting. This was about 1000 AD. It was not rediscovered until 1900. The Star Chart was among this cache. It is now held in the British Library in London.

Professor J.-M. Bonnet-Bidaud. It is a great honour for me to be invited by the RAS to give a brief summary of the astronomical content of the Dunhuang Star Chart. You have already heard from Susan Whitfield about the circumstances of its discovery. Now, before coming to the Chart itself, as most of you are probably not familiar with ancient Chinese astronomy, I will give here a short introduction. China is a very special country and totally unique in a sense that it has maintained continuous astronomical observations for more than 4 000 years. Compared to ancient Greece, the Islamic civilization, or even modern astronomy, that all have an existence of only a few centuries, astronomy was not only present in China for at least 40 centuries but also is fully documented by numerous ancient texts with still-extant copies. An already remarkable account was given by the sinologist Joseph Needham in 1959 but is now complemented by many more recent archaeological discoveries, such as the Taosi stone observatory, dated at year 2000 BC, or the 'Silk book' containing a remarkable atlas of comets, dated 169 BC. The Dunhuang Star Chart, made of very thin, delicate paper, is among these remarkable artefacts. The astronomical part of the document is 2·1 metres long and 25 centimetres wide. It shows the details of the Chinese constellations distributed along 12 vertical panels followed by a circumpolar map. A total of 1340 stars was counted, distributed in 257 asterisms. This is the first known representation of the whole Chinese sky. The panels are accurately ordered in right ascension along the equator, starting at the position of the Sun in February, at the beginning of the Chinese year.

This remarkable layout is in all points similar to our modern map with the so-called cylindrical projection. The circumpolar map is also displayed in an apparent azimuthal projection. You have to remember that solving the problem of the projection of a sphere onto a plane is by no means trivial. It was correctly solved in Europe only around the 16th Century by cartographers like Mercator.

Part of our study was devoted to the test of the accuracy of these projections. To do this, we have selected a sample of 15–20 brightest stars per panel and tested their measured positions on the map with predicted positions, using selected projection methods. This statistical analysis proved to be quite conclusive. The star positions are in good agreement with an accuracy of 1·5 to 4 degrees. This demonstrates that this hand-made document, despite its relatively small size, was carefully designed to preserve accurate information and is based on mathematical projection methods. Approximations in the drawing of some asterisms may, however, indicate that it is probably a copy.

Achieving a date for the document was another goal. Needham, who briefly quotes the existence of the Chart, gave it the date 940 AD on grounds that we were unable to trace, and this information has been constantly reproduced since. We have considered several other strong arguments that point to a different date. In the first part of the roll concerning cloud divinations, mention is made of a possible author as 'Chunfeng', for a well-known mathematician and astronomer Li Chunfeng (602–670 AD) of the early Tang dynasty. Another clue is given by the use of the so-called 'taboo' characters, Chinese characters, which are forbidden to be used during the reign of a given emperor, that were replaced by other ones. Scans of the texts accompanying the Chart points to a period after the emperor Taizong (649 AD) and before Ruizong (684 AD). Finally the projection study of the circumpolar map allows determination of the pole position at the date of the Chart as consistent with 650 AD, though with quite a large uncertainty. The most probable date is therefore 650–670 AD. Following Dunhuang, a series of maps were produced in China, including the well-known Suzhou Planisphere (1193 AD).

To put the Dunhuang Chart in a more general context, it is interesting to compare it to two other earlier works often referred to as star maps: the Denderah Zodiac, dated 50 BC, and the Farnese Globe (150 AD). Both are carved on stone but only show artistic drawings of constellations without any individual stars. Outside China, the first star chart produced was the work of the Persian Al-Sufi (986 AD), showing for the first time individual stars but distributed into separated constellations with no information on their relative positions. In Western Europe, only the later Vienna Manuscript (1440 AD) shows star positions in a global representation.

The Dunhuang Chart therefore not only predates other works by several centuries but it shows a remarkable achievement in terms of mathematical projection methods in China, a point somewhat overlooked till now.

Before ending this report, I would like to underline the important rôle played by the International Dunhuang Project at the British Library, a very valuable tool for both education and research. By making available to scholars high-quality numerical copies of the precious Dunhuang documents, it allows studies like the one presented here to be performed easily. It has helped to rescue from oblivion this key document in the history of astronomy.

We are sad to announce that in the progress of this work, one of our colleagues, Françoise Praderie, an outstanding European astronomer, passed away in Paris on 2009 January 28.

The President. Thanks to both of you. Any questions?

Dr. G. Q. G. Stanley. The methodology that produces cylindrical projection — have you found out how that happened? Or is there an artifact they appear to have used? Is there any evidence for a Cartesian layout to actually produce those on the scrolls?

Professor Bonnet-Bidaud. If I understand the question correctly, what you are asking is what method was used to plot on a piece of paper, and you are expecting that some lines in the form of a grid would have first been drawn to allow the plotting of the stars. And the reason that there are not is because this is not the original but a copy. It may have been copied using very thin tracing paper or by just transcribing from the existing documents side by side. Does that answer your question?

Dr. Stanley. Yes it does, thank you. So the reason they went into cylindrical projection — was that an artefact of the instruments they were using?

Professor Bonnet-Bidaud. Obviously they were collecting coordinates of the stars with respect to the equator, which is already something simpler than the ecliptic, which was often used by Greek astronomers. The instrument was giving them the right-ascension position on the equator, and not the angle from the equator but the angle from the pole. Once you have collected the data, it's another problem to project that onto the map. It is really a mathematical treatment beyond that. It is exactly the same point for us today. If you give me a collection of right ascensions and declinations and you ask me to plot them, then there are different ways of doing it but I have to use some way to convert astronomical coordinates to something like x and y.

Professor D. Lynden-Bell. It's well known since the days of Proctor that four or five stars of the Plough move together and one or two of them do not. Can you use the shape of the Plough on the map to get an estimate of the date when it was made?

Professor Bonnet-Bidaud. But we are dealing here with only a few centuries, so let me check.

A Fellow. How far south of the equator does it go?

Professor Bonnet-Bidaud. It's very interesting because it is thought that you can derive from that the location where the Chart was produced. I don't remember exactly how far it goes, but it's about 44 degrees south, so you can see it is compatible with the latitude of Dunhuang and the main capitals of China — but not to very high accuracy; we can't use that as a proof.

Mr. H. Regnart. You showed us a telescopic mounting from China. But was there any method of imaging and magnification involved or was it just a matter of stable sighting?

Professor Bonnet-Bidaud. To my knowledge, until we find something else, there was no magnification, just a tube. There were divisions on the circle to give the coordinates but no magnification at all.

The President. I'm going to ask one last question. Susan said it was highly worn so it was used a lot. So what was it used for?

Dr. Whitfield. That is the mystery of the Chart because something like this was politically very sensitive in China. It wasn't seen as a scientific document, it was seen as a political document. That is why the Chinese were so careful to make astronomical measurements and recordings. The movements of the stars reflected the actions of the Emperor and could therefore be used to criticize his actions. It was very important to get it right. This was highly sensitive, top-secret knowledge which would have only been kept in the imperial court by the imperial astronomer. The reason why this document exists a thousand miles away from the imperial court in an outpost of the empire on quite a tatty copy — because this is very messy calligraphy — is a complete mystery. And it is the only object we have from this time. The original would have been used as a political tool, but this one, we don't know.

The President. Thank you very much.

We now have Paul Denton from the British Geological Survey (BGS) who is going to talk to us about 'Seismology in schools'.

Mr. P. Denton. [The speaker started by saying he had been running the Schools Seismology Programme (SSP) for two years. The idea is to get secondary-school students into science by doing experiments, in this case, the detection of earthquakes all over the globe. In addition, other classic experiments involving the Earth and seismology are also encouraged. This has been done in the USA for a few years and arose out of the tradition of amateur scientists recording earthquakes from the Pacific region. In early 2005 the BGS started to develop an instrument for the SSP with a few simple aims in mind, the most important of which is to make science more interesting. Earthquakes have a natural attraction for youngsters and, once they are hooked, we can then progress to teaching physics in the crucial post-16 period. We are also trying to promote geoscience in schools and increase the uptake of geological science at university.

The classroom experiments are simple — looking at building resonance and supplying kits which allow students to make their own seismometers, which are all written up in the booklet of the Science Enhancement Programme (SEP). Key collaborators have produced high-quality material for science teachers and 3000 copies have been distributed throughout the UK.

The seismometer itself is simple and equivalent to the state of the experimental art about 100 years ago. A pendulum detects ground motion with a 20-second natural period which is tuned to surface waves from large earthquakes on the other side of the globe. Using these instruments also requires an understanding of electromagnetic principles such as eddy currents.

The instruments have been manufactured by Middlesex University Teaching Resources in collaboration with SEP. The cost is £320 and the instrument uses a standard PC for control. Last year on May 12 a signal which lasted 1·5 to 2 hours was picked up from a magnitude-7·9 earthquake in China. The software in the PC allows the student to identify primary (P) and secondary (S) waves on the signal, and by analysing the delay between the P and S waves, the distance to the event can be calculated. Once the distance is known, an estimate of the magnitude of the event can be obtained by measuring the amplitude of the surface waves, and this can agree to within 0·5 units of the measurement made by the United States Geological Survey.

Schools involved in the project have been getting press coverage, particularly with local newspapers. There was particular attention after the UK earthquake in Market Rasen last year and that was a source of inspiration to students.

The principal supporters of the scheme are the BGS (funded by the Natural Environment Research Council) for the delivery phases, and in 2009 we will get funding from Petroleum Exploration Society of GB and the Scottish Oil Club to buy seismometers for schools. Working in collaboration with university geological-science departments at Leicester, Imperial College London, Keele, Plymouth, and Leeds, workshops have been run for a cluster of schools: teachers get a day-long seminar and take away a seismometer to set up in their schools, and to date 150 instruments have been set up. There has been a lot of positive feedback. The BGS website for schools is at http://www.bgs.ac.uk/schoolseismology/ and each time an earthquake event occurs data appear on the website — predicted arrival times of P and S waves, for instance, allow them to be interpreted. The website data can also be used to narrow down the characteristics of an earthquake even from a single station. Last year, schools identified 85 earthquakes, and combining these data generates a spirit of competition amongst the schools. There is also a common blog area, http://scispace.net/schoolseismo/weblog/, which is an unmoderated free space for teachers to communicate with BGS and each other.

The speaker ended his presentation by showing a short video (available from www.bgs.ac.uk/schoolseismology) which was shot during a training workshop for teachers held at Imperial College in London and at Paulet High School in Burton-on-Trent.]

The President. That was a nice talk. Last term we had a talk about outreach with telescopes and now outreach with seismometers. Any questions?

Professor M. A. Khan. Not a question but a comment. Just to mention that the RAS had a rôle in this. Firstly, a few years ago there was a report on geophysical education in the UK which revealed the fact that the university courses in geophysics were closing down because of a shortage of applicants in physics and mathematics in spite of the fact that there was a growing need for geophysicists. One of the things highlighted in the RAS report was the promotion of projects like this. The second point is that some of the funding for the instruments being given to schools has come from petroleum-exploration exercises and that the fund is managed by the RAS, which is actively involved in this business; I think this will benefit everybody concerned.

Professor P. G. Murdin. We were talking yesterday about putting a seismometer in our premises in Burlington House as a showpiece. Does the fact that there are tube trains running underneath it cause problems?

Mr. Denton. Ah, it makes it more interesting because you can not only detect earthquakes but also tube trains …

A Fellow. One every three minutes!

Mr. Denton. … but presumably they stop in the middle of the night. You have the same problem in schools because during the day the environments can be quite noisy with students rushing about all over the place. We're not doing this project to get high-quality seismic data, we're doing it to try and provide a bit of excitement for the schools.

A Fellow. You were saying that you wanted to get data from schools over a wider area; have you tried rolling it out through the international-school associations?

Mr. Denton. Well, at the moment we have a couple of British international schools that we're setting up with seismometers. There's one in Jakarta and one in Jeddah in Saudi Arabia, and we're also trying to initiate some projects through the British Council with schools in Africa, so it's on the 'to do' list for 2009.

Mr. C. J. North. How do you position the seismometers? Do you have a special support or special foundation on which they are based?

Mr. Denton. Because they are horizontal instruments they are very sensitive to ground tilt so they have to be on the ground floor and it has to be a concrete floor, it can't be a suspended timber floor. Beyond that it's up to the school as to where they can find somewhere that's convenient and reasonably secure.

Mr. North. I was thinking of a balance room, so the supports were separate from the rest of the building. You don't put the seismometer on a similar type of support?

Mr. Denton. Well, if you had a balance room you could, but you're not going to build one especially for this project. Wherever you site these things there's a compromise between having the seismometer somewhere where the students can see it and somewhere where it will be reasonably quiet. Schools are actually empty for 70% of the time so it's not as big a problem as you might think.

The President. Thanks again. [Applause.]

Professor Murdin. I'm taking over the Chair for about the next hour and I would like to introduce someone who needs no introduction — our President, Andrew Fabian, who will give this year's Presidential Address on 'Black holes at work'. [Applause.]

The President. [A summary of this talk has appeared in *Astronomy & Geophysics,* **50**, 3.18, 2009.]

Professor Murdin. Thanks very much, Andy. It's bang on six o'clock and we're supposed to leave but I'm not going to because I want to talk about this fascinating lecture.

A Fellow. A marvellously entertaining talk. Tell me, why is our night sky not dominated by a black hole?

The President. Good question. In the centre of our Galaxy there is a black hole of four-million solar masses and some of you might remember that Fred Hoyle wrote a book about it turning into an active galaxy, but at present the black hole at the centre of our Galaxy is extremely quiescent: the total power of what comes out of that is tens of solar luminosities. One of the biggest puzzles that has been around for the last twenty-five years in astronomy is why this four-million-solar-mass black hole is not more active. Intriguingly, a few-hundred light-years distant from the black hole are various molecular clouds, and when studied by X-ray spectrometers these clouds show fluorescent ion emission suggesting that the black hole was actually switched on just a few-hundred years ago. So it could be that it's just temporarily switched off. When we look at active nuclei in other galaxies we find they are extremely variable and sometimes they do go into what is called a low state, and in fact in one case last year we found it was down by a factor of 50. What we would have to argue in the case of our Galactic Centre is that it has switched off by a factor of almost a million. Inevitably there are a lot of papers and ideas about what has caused this, but it is possible if you remember that just a few years ago our Galaxy was much more active. There is a problem, though, in terms of us possibly seeing it. If the centre of the Galaxy were to switch on tomorrow, would we then see this bright light in the sky? Of course, we live in a spiral galaxy with lots of dust and so forth in the plane so we probably wouldn't see very much at all.

The Fellow. And whereabouts is it in the night sky?

The President. Sagittarius — in the summer sky.

Mr. N. Calder. Can your sound wave interact with dark matter?

The President. I don't think so. It would represent a change of density of about five to ten per cent. There would therefore be some gravitational coupling but I would imagine it's very slight.

Dr. A. Ball. Could you give me a feeling for the partition of energy between emission in electromagnetic, gravitational, and sound waves for this object?

The President. Yes. Curiously for these objects, most of the power is coming out in jets. And this is one of the things that I would argue made us take the wrong path for a long time as it turns out that jets can be enormously radiatively inefficient. Somehow a black hole can make an enormously energetic outflow and yet the amount of radiation that comes out of that outflow is less than one part in a thousand. In M 87, the luminosity of the radio jets is very low compared to the enormous power that must be going up those jets. So that tends to be a property of black holes when they are accreting at a relatively low rate. When you go to a quasar, most of it is coming out as electromagnetic radiation. When you turn down the wick, as it were, then they seem to switch to a more energetic mode which is very efficient in terms of kinetic energy, but that's hand-waving!

Professor Murdin. I've never told Andy this, but a few years ago, perhaps ten years ago, these observations of the iron line in the MCG galaxy got me about ten million pounds. Not for myself personally, but we had one of our repeated and perpetual problems at the British National Space Centre in which money had been owed to the European Space Agency. I went to Lord Sainsbury and tried to talk him into solving it for us, so to soften him up I showed him Andy's work and I deliberately took along scratchy old viewgraphs that Andy had given me to illustrate it, to show we weren't wasting any money. [Laughter.] I told him that Andy had found material which was within a few Schwarzschild radii of the black hole and was moving at half the speed of light and he said "Good God", and then I went on to do my pitch and got money out of him. I think the observations that Andy talked about are obviously pretty expensive, if they're mega-seconds then they must be mega-bucks. But actually the return on them is perhaps ten times bigger than it was on that occasion. So thank you very much Andy for your fantastic talk. [Applause.]

The President. Can I remind you all that there is a drinks reception over in Burlington House. The next meeting will be on Friday, March 13 — the second Friday the thirteenth in a row!

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 207: 58 PISCIUM, 31 VULPECULAE, AND 70 PEGASI

By R. F. Griffin
Cambridge Observatories

Orbits are presented for three stars bright enough to carry constellation designations. They are all late-G giants; 58 Psc was long regarded as being of luminosity class II but was shown by *Hipparcos* to have $M_V \sim +1^m$. It has an orbit of moderate eccentricity (0·39) and a period of 843 days; elements have been given previously on the basis of very precise radial velocities obtained at only a few epochs. 31 Vul has an orbit of similar eccentricity and a period of just over 5 years. The orbit of 70 Peg is of high eccentricity (0·71) and has a period of 940 days which is determined to within a matter of hours.

Introduction

It is one of the marvels of modern astronomy that, while so many observers are straining after the faintest and most elusive (illusive?) objects, there remain plenty of stars that used — when the night sky was dark — to be visible to the naked eye and that are known to be spectroscopic binaries but have never been observed systematically enough for their orbits to be determined. In some cases, moreover, their binary nature has been discovered only comparatively recently, and doubtless there are other cases in which it has not been discovered at all.

The three giant stars discussed in this paper were selected, in a fashion more particularly explained in the introduction to Paper 188[1] of this series, from the work of de Medeiros & Mayor[2] after those authors provided through the Centre de Données Stellaires a listing of their individual radial-velocity measurements.

The stars were thought to have been newly discovered as binaries and were in need of having their orbits determined, and on that basis they were all placed on the observing programme of the Cambridge *Coravel* in 2002. The writer was then unaware of Butler's[3] orbit for 58 Psc, which was given as long ago as 1998 in a paper whose title does not suggest an interest in orbits, and which was not entered in any catalogue of spectroscopic orbits (nor is it even now, at the time of writing). De Medeiros & Mayor themselves were equally unaware of it, since their listing[2] distinguishes binaries with orbits (designated SBO) from those without (SB); the latter designation is attached to 58 Psc. The star is explicitly listed as single in a column headed 'Binarity'* in a paper published in 2000 by Smith & Shetrone[4].

The *Bright Star Catalogue*[5] suffixes the mean radial velocity of 31 Vul with the note "SB", for which the principal evidence is not easily retrieved. Subsequently (1986) Beavers & Eitter[6], too, concluded from a set of 13 measurements that the velocity is variable, but the present writer admits to having considered that evidence at that time and not being wholly convinced by it. The *Catalogue*[5] notes the velocity of 70 Peg with "V?", indicating possible variability; the velocity was at one time claimed at Lick[7] to be variable, but in the final publication[8] of all the Lick velocities the claim was not repeated and was thereby implicitly rescinded.

58 Piscium (*HR 213, HD 4482*)

Like the other stars discussed in this paper, 58 Psc is not only marked but is individually identified in star atlases such as *Norton*[9] and *Tirion*[10], so it is superfluous to describe here where they are to be found in the sky.

Despite its brightness, 58 Psc seems only once — and belatedly, at that — to have been the subject of properly published *UBV* photometry: the most recent compilation[11] cites the values $V = 5^{m}\cdot 50$, $(B-V) = 0^{m}\cdot 97$, as deriving from private communication(s) in 1969–71 from Häggkvist. The same values appeared already in Nicolet's[12] 1978 compilation and in the *Bright Star Catalogue* (ref. 5, p. 12). They were in fact published in 1987, in a paper whose title refers only to *narrow-band* photometry, by Häggkvist & Oja[13]. Quite similar V and $(B-V)$ values were given by *Hipparcos*[14] from its own and *Tycho* photometry, respectively, but there is still no measure of the ultraviolet colour.

Systematic spectral classification was undertaken at Harvard well over 100 years ago for stars as bright as those discussed in this paper; it was published in the *Draper Catalogue*[15] in 1890 before being extended to about the ninth magnitude in the *Henry Draper Catalogue*[16], published in successive volumes in the interval 1918–1924. The types given for 58 Psc in the two catalogues are H and G5, respectively. Although H is one of the types that was dropped in the correction and refinement of the early classifications, it is easy to see that in spirit it is altogether reasonable, coming as it does between G and K.

The type of 58 Psc was first given on the MK system in 1969 by Harlan[17], who on the basis of classification spectra taken at a dispersion of 75 Å mm^{-1} at Hγ on the Lick refractor called it G8 II. In 1979 Cowley & Bidelman[18], using a rather analogous spectrograph on the 37-inch Ann Arbor reflector[19], gave the type as K0 III. The high luminosity implied by the earlier classification[17] was actually a mistake, but no matter how many times that may have been suggested or even demonstrated it seems impossible to shift it from the literature, probably because the G8 II type was selected for inclusion in the *Bright Star Catalogue*.

*A word not to be found in English dictionaries, but evidently anglicized from the corresponding French word and preferred by some to the perfectly good English equivalent, 'duplicity', which, however, also has a darker connotation which may be regarded as objectionable.

Eggen made a specific note in one[20] of his tables, saying "Not a bright giant", and in a subsequent one[21] he listed it as G8 III (though where he obtained that type is an unresolved question — possibly he just took Harlan's type and corrected the luminosity class!) and gave its distance modulus as $4^{m}\cdot60$. That is actually in perfect agreement with the *Hipparcos*[14] result of $4^{m}\cdot53 \pm 0^{m}\cdot14$, which puts the absolute magnitude very close to $+1^{m}\cdot0$. All the same, Butler[3] listed it as G8 II and went on to assess its mass at 5 $M_{\odot}$; Balona & Dziembowski[22] regarded its absolute magnitude as $-2^{m}\cdot3$ and — thus misled — deduced a possible pulsation period according to a period–luminosity law as if it were a Cepheid variable; Smith & Shetrone[4] listed it as G8 II; Larsen *et al.*[23] used it as a 'template star' of that type in a study of a globular star cluster, and Morel & Micela[24] adopted it as a 'presumably single' G8 II 'control star' for comparison with a number of active-chromosphere binaries. The erroneous MK luminosity classification which has caused such confusion and error stands in ironic contrast with the luminosity estimates made the best part of a century ago by Rimmer[25] ($+1^{m}\cdot0$) and Adams *et al.*[26] ($+0^{m}\cdot7$).

Radial velocities and orbits for 58 Psc

The radial velocity of 58 Psc was first measured at Mount Wilson, whence Christie & (O. C.) Wilson[27] published a mean of $-0\cdot3$ km s^{-1} with a 'probable error' of $1\cdot0$ km s^{-1} in 1938; much later Abt[28] very helpfully provided the individual dates and velocities, showing that the observations were made with a spectrograph giving 36 Å mm^{-1} at Hγ on the 60-inch reflector in 1928 and 1933. There were two observations only a few days apart in late 1928, and by great misfortune the single 1933 observation was made after a lapse that was very exactly two cycles of the orbital variation, which therefore remained undiscovered.

In 1998 Butler[3] published some very precise radial velocities and a set of orbital elements for 58 Psc in the paper that has until now been overlooked by the present writer and others, partly perhaps through their being misdirected by its title, 'A [high-]precision velocity study of photometrically stable stars in the Cepheid instability strip'; the strip to which reference is made is in a part of the H–R diagram in which 58 Psc assuredly does *not* fall.

The following year, de Medeiros & Mayor[2] included 58 Psc in a large list of stars for which they gave mean values of radial and rotational velocities, and they appeared to be concluding that they had discovered it to be a spectroscopic binary. When, in 2002, they provided through the Centre de Données Stellaires a listing of the individual observations of all the stars, including six measurements of 58 Psc, that was one of the stars selected by the writer for observation at Cambridge and was placed on the observing programme of the *Coravel* there. Since then, it has been observed 42 times and been seen round nearly three revolutions of its orbit. The observations are listed in Table I, in which those of Christie & Wilson[27] and of de Medeiros & Mayor[2] are included at the head. Butler's observations are not included, because they have already been published by their author; they have not been utilized in the orbit derived here, so as to maintain independence between the two orbital solutions. In an effort to place all the observations on the scale usually adopted in this series of papers, the usual adjustment of $+0\cdot8$ km s^{-1} has been made to the Mount Wilson[27] and OHP[2] velocities, although in the former case the effect is merely cosmetic because the observations concerned have been zero-weighted in the solution of the orbit. The Cambridge velocities have been corrected by

TABLE I

Radial-velocity observations of 58 Piscium

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

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O–C)* *km s*$^{-1}$
1928 Dec. 21·12*	25601·12	+0·9	$\overline{32}$·516	+2·4
30·15*	610·15	−2·7	·527	−1·1
1933 Aug. 12·49*	27296·49	+2·4	$\overline{30}$·528	+4·0
1986 Aug. 20·15†	46662·15	−0·9	$\overline{7}$·499	+0·4
21·08†	663·08	−1·3	·501	0·0
Oct. 8·97†	711·97	−2·4	·559	−0·4
1987 Aug. 14·04†	47021·04	−10·6	$\overline{7}$·925	−0·2
1992 Aug. 20·15†	48854·15	−3·2	$\overline{4}$·100	−0·5
1993 Nov. 22·83†	49313·83	−3·6	$\overline{4}$·645	−0·3
2002 Aug. 2·13	52488·13	−0·5	0·410	0·0
Sept. 2·11	519·11	−0·6	·447	+0·2
Oct. 27·97	574·97	−1·6	·513	−0·1
2003 Jan. 5·79	52644·79	−2·4	0·596	+0·2
Feb. 17·78	687·78	−3·4	·647	0·0
Aug. 3·12	854·12	−8·1	·844	0·0
Sept. 14·09	896·09	−9·9	·894	−0·3
Oct. 12·06	924·06	−10·5	·927	−0·1
Nov. 5·95	948·95	−10·5	·957	+0·1
Dec. 7·92	980·92	−9·1	·995	+0·3
2004 Jan. 2·83	53006·83	−7·4	1·026	0·0
Feb. 25·76	060·76	−3·3	·090	−0·2
Aug. 7·13	224·13	+0·1	·283	−0·1
Sept. 5·07	253·07	+0·2	·318	+0·1
Oct. 7·06	285·06	−0·2	·356	−0·1
26·05	304·05	−0·4	·378	−0·2
2005 Jan. 8·80	53378·80	−1·0	1·467	0·0
July 18·11	569·11	−3·9	·693	+0·3
Aug. 13·14	595·14	−4·6	·723	+0·3
Sept. 7·10	620·10	−5·4	·753	+0·1
28·09	641·09	−6·3	·778	−0·1
Oct. 26·00	669·00	−7·0	·811	+0·1
Nov. 9·94	683·94	−7·7	·829	−0·1
Dec. 8·90	712·90	−8·7	·863	0·0
2006 Jan. 4·75	53739·75	−9·5	1·895	+0·2
Feb. 8·76	774·76	−10·5	·937	+0·1
July 15·12	931·12	−1·6	2·122	+0·2
Aug. 11·15	958·15	−0·8	·154	+0·1
30·09	977·09	−0·3	·177	+0·1
Sept. 23·04	54001·04	0·0	·205	+0·1
Oct. 26·98	034·98	+0·2	·245	+0·1
2007 June 28·10	54279·10	−2·1	2·535	−0·4
July 27·12	308·12	−2·3	·569	−0·1
Sept. 8·12	351·12	−2·9	·620	0·0

TABLE I (*concluded*)

Date (UT)	MJD	Velocity km s⁻¹	Phase	(O–C) km s⁻¹
2008 Jan. 7·87	54472·87	−6·0	2·765	−0·2
July 13·13	660·13	−10·0	·987	−0·2
Aug. 4·10	682·10	−8·5	3·013	−0·2
30·15	708·15	−6·0	·044	+0·1
Sept. 14·04	723·04	−4·7	·061	+0·1
28·05	737·05	−3·9	·078	−0·1
Oct. 22·01	761·01	−2·4	·106	0·0
Nov. 18·96	788·96	−1·3	·140	−0·1

*Mt. Wilson observation[27,28]; wt. 0.
†OHP observation[2]; weight 0·25.

−0·2 km s⁻¹ from the values 'as initially reduced', as has been found appropriate to velocities for objects of the colour of 58 Psc previously, particularly in Paper 190[29] of this series. In solving the orbit the OHP velocities have been given ¼ of the weight of the Cambridge ones, to produce approximate equality in the weighted variances. Table II gives the derived orbital elements, and for comparison it lists the elements given by Butler[3]. Fig. 1 shows the computed orbit corresponding to the Cambridge elements, but Butler's observations are also plotted (with open symbols) even though they were not taken into account in computing the solution; their zero-point does not seem to be one of their strong points — indeed no γ-velocity was given for the orbit — and an empirical offset of −1·33 km s⁻¹ has been applied to them to bring them into systematic agreement with the Cambridge measurements.

TABLE II

Orbital elements for 58 Psc

Element		Butler[3]	**This paper**
P	(days)	843 ± 4	**843·0 ± 0·5**
T	(MJD)	49601·62 ± 3·5	**53828·3 ± 1·9**
γ	(km s⁻¹)	—	**−3·71 ± 0·03**
K	(km s⁻¹)	5·277 ± 0·010	**5·39 ± 0·04**
e		0·386 ± 0·013	**0·390 ± 0·006**
ω	(degrees)	219·6 ± 0.6	**223·5 ± 1·1**
$a_1 \sin i$	(Gm)	(0·377 AU)	**57·5 ± 0·4**
$f(m)$	($M_\odot$)	0·0101	**0·01068 ± 0·00025**
R.m.s. residual (wt. 1) (km s⁻¹)		0·0333	**0·15**

The two sets of orbital elements shown in Table II provide a comparison that is perhaps more interesting than it is instructive. They are based upon mutually comparable numbers of observations, but one set has something like ten times the precision of the other. That set, however, is not well distributed in phase, being effectively confined to five distinct epochs at annual intervals; at each epoch several observations were taken in quick succession, often at 24-hour intervals. The advantage of the high precision appears thereby to be largely lost; indeed, the temporal elements (at least) appear to be less accurately determined than by the observations of poorer precision but better time-distribution. Another troublesome feature of the situation is that a re-computation of the Butler orbit by the present writer does not give the same elements as those

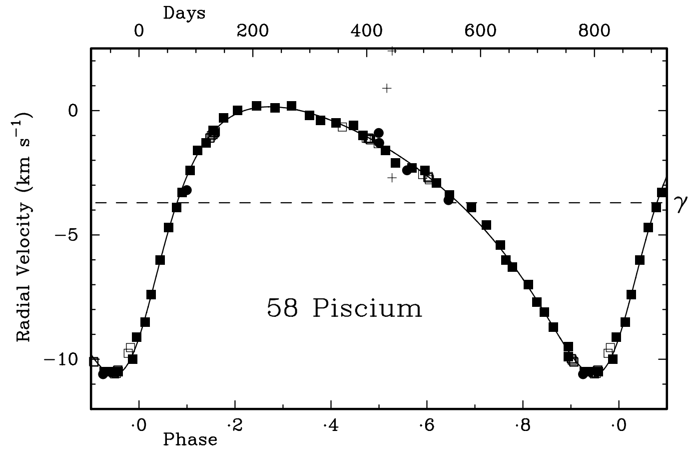

FIG. 1

The observed radial velocities of 58 Psc plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The observations plotted as filled squares are those made by the author with the Cambridge *Coravel*. Filled circles represent the six measurements from the OHP *Coravel*[2]; they were given weight 0·25 in the solution of the orbit. No weight was given to the three Mount Wilson photographic measurements[27,28], which appear as plusses. The very precise measurements which constitute the basis for the independent orbit published by Butler[3] are plotted as open squares but were not included in the solution

found by Butler himself; the discrepancies are of the same order as those between the sets of elements listed in Table II, both in the elements themselves and in the uncertainties. The Cambridge calculation results in a smaller mean-square deviation for the velocities than Butler's own solution does.

It is possible that the Butler uncertainties take into account the correlations between the different elements, which are bound to be fierce when the observations are restricted to so few independent epochs; in any case, the writer is not disposed to promulgate an alternative set of 'Butler' elements here. The question arises, however, as to whether Butler really did obtain the best set of elements that his observations could support. An anomaly that may be noticed in his paper is that he says that there are 34 observations but then he tabulates 36. He could have changed the elements appreciably by rejecting two, but there is no telling whether he did that, or (if so) which two he may have rejected. One can see very obviously in Fig. 1 that two of his observations, just after the velocity minimum, show bad residuals according to the Cambridge orbit, but that does not necessarily demonstrate that they are particularly at fault. Butler tabulates an uncertainty in K_1 of only 10 m s^{-1} (0·010 km s^{-1}; misprinted as 0·001 by Massarotti *et al.*[30]), but one of his observational epochs falls exactly at the minimum-velocity node, and according to his own paper all six observations made at that epoch have negative residuals averaging about –0·05 km s^{-1}, suggesting that an increase in K might well be an improvement. Indeed, the Cambridge orbit computation from the Butler data does yield a value of K_1 that is larger by 0·019 km s^{-1}, nearly twice the listed standard deviation of that quantity.

Especially since the two sets of observations do not overlap in time (except insofar as the 'Cambridge' set includes a few OHP measures of low weight), it could be expected that pooling the data would produce an orbit better than is obtainable from either of the constituent series. Unfortunately it is not at all clear how to combine data of such disparate characters. When they are pooled, the Butler measurements give residuals that are so much smaller than those of the Cambridge ones that they seem to demand increased weighting, and when that principle is followed their weights continue to warrant successive increases until the solution might as well not have included the Cambridge or OHP data at all. The problem is one that ought not to be considered soluble just by logic and mathematics, and it is regretted that no good answer to it can be advanced here.

The mass function is too small to be informative: if the primary is attributed a mass of 2 $M_\odot$ the secondary has only to be about 0·4 $M_\odot$ as a minimum, so it could be a star as far down the main sequence as about M2.

The absolute magnitude is shown by the parallax[14] to be very close to $+1^m \cdot 0$; it is clear from Keenan & Barnbaum's[31] Fig. 1 that that places it in luminosity class IIIb, which largely consists of helium-burning 'clump giants'. In papers dealing with elemental abundances in the atmospheres of cool giant stars, Mishenina *et al.*[32] first found that elements up to Fe have close to solar abundances, and then in a second paper[33] found slightly sub-solar abundances for several neutron-capture (rare-earth) elements ranging from Y to Eu. They considered it then not to be a clump giant but an "ascending-giant-branch star" that just happens to be in the clump region.

31 Vulpeculae (*HR 7995, HD 198809*)

The *UBV* magnitudes of 31 Vul have been measured, as befits such a bright star, by quite a number of observers, who will not be cited individually here; representative mean values are $V = 4^m \cdot 57$, $(B-V) = 0^m \cdot 83$, $(U-B) = 0^m \cdot 47$. The star appears in the *New Catalogue of Suspected Variables*[34] as NSV 13373; indeed, *Simbad*'s favoured name for the object, no matter under what designation one enquires for it, is the *NSV* one, and *Simbad*'s main characterization of 31 Vul is as a variable star. There is little evidence of variability among the optical measures; in fact the r.m.s. deviation of the *Hipparcos* 'epoch photometry', consisting of 150 measures, is only $0^m \cdot 006$. To put that in perspective it may be mentioned that, among the analogous values for the other 99 stars on the same page of the *Hipparcos* catalogue (ref. 14, **9**, 2071) there is only one that is smaller. The evidence cited in the *NSV* for variability is that the *I* magnitude is said to range from $3^m \cdot 77$: to $4^m \cdot 08$:, and the reference given for that information is to the early *Two-Micron Sky Survey*[35] (also known as the *IRC* or *InfraRed Catalogue*; not to be confused with the *2MASS* (*Two-Micron All-Sky Survey*) catalogue[36]). The quantities listed in the *IRC* for the errors of the mean magnitudes seem not to agree with those that the present writer would have derived from the same input data, but they have presumably all been computed in the same manner, and among the 50 stars listed on the page (p. 206) that includes 31 Vul as no. 30458 there are only 15 that have smaller values for the error of the mean *I* magnitude. Since 31 Vul is brighter than most of the stars, however, the discrepancy between its two measurements is considered somewhat significant in the χ^2 sense and is so flagged. It seems a very slender basis upon which to accuse 31 Vul of variability, and in the absence of more compelling evidence there might be merit in *Simbad*'s ceasing to emphasize photometric variability as the pre-eminent characteristic of 31 Vul.

The type of 31 Vul is given as H in the early *Draper Catalogue*[15] and as G5 in the *Henry Draper Catalogue*[16]. Even before the publication of the relevant volume (**98**) of the *HD* in 1923, Adams *et al.*[37] at Mount Wilson had made their own classification and an initial effort at estimating the spectroscopic luminosity of 31 Vul. They gave the type as G2 according to the usual procedure of comparing the spectrum with those of standard stars, and as G3 according to 'measurement' — comparison of the strengths of certain Balmer lines with those of specified metallic lines. Comparison of luminosity-sensitive pairs of lines led them to give an absolute magnitude of $-1^{m}\cdot 6$. Soon afterwards Rimmer[25], of the Norman Lockyer Observatory, found the absolute magnitude to be $-1^{m}\cdot 2$, whereas Young & Harper[38] at the DAO gave much more moderate values, estimated by each of them independently, of $+1^{m}\cdot 3$ and $+0^{m}\cdot 5$. Subsequently Adams *et al.*[26] fell into line by a major revision of their estimate to $+0^{m}\cdot 9$; they still gave the spectral type as G2.

An MK type of G5 III was found by Nassau & van Albada[39] from an objective-prism spectrogram of very low dispersion (280 Å mm^{-1} at Hγ) obtained with a 4° prism on the *Burrell Schmidt*[40], but slit spectrograms of more usual dispersions for classification purposes have given somewhat later types (still with luminosity class III). The first was by Miss Roman[41], who gave the type as G8 III. *Simbad* lists several other identical classifications, seeming not to distinguish between original work and quotations; one of the citations is to the present writer's own work in collaboration with Redman[42], where (just as in the other cases cited by *Simbad*) we simply included a column for spectral types, which we took from the literature, in tabular material related to the stars that we were discussing. On the other hand, although the bibliographic part of *Simbad* retrieves the several papers in which Keenan and his collaborators repeatedly gave fresh (sometimes slightly different) classifications for a lot of MK standard stars, it omits them in the 'measurements' section that includes the citations to papers such as that by Griffin & Redman[42].

The first of the revised classifications was a study by Keenan & Wilson[43] at coudé dispersion, which enabled them to detect small enhancements or deficiencies in certain lines; they gave the type of 31 Vul as G7 III Cn [*sic*] –1 Ba 0·2:. Their paper was a sequel to the one by Wilson[44], published the previous year (1976), in which he gave '*K*-line absolute magnitudes' for many stars, including 31 Vul, for which he listed $M_V = +1^{m}\cdot 3 \pm 0^{m}\cdot 3$. From the coudé study, however, Keenan & Wilson derived a spectroscopic absolute magnitude of $+0^{m}\cdot 35$. The very slight barium enhancement that Keenan & Wilson thought they detected does not feature again in any subsequent classifications; Keenan explained the reason for that in Paper 105[45] of the present series, saying in effect that since such a marginal enhancement could not be recognized at typical classification dispersions it was better not to include it in the official type of a standard star, although the actual validity of the detection was not thereby implicitly repudiated. In 1980 Keenan & Pitts[46], therefore, gave the type of 31 Vul, among a large list of standards, as G7 III CN –1, a type repeated in a 1983 listing[47] by Keenan alone. Since that was the type that was favoured at the time that the current edition of the *Bright Star Catalogue*[4] was compiled, that is the type that is listed there. It may be mentioned that CN did not seem particularly weak in the quantitative measurements made by Griffin & Redman[42] of the λ4200-Å band of that molecule; among 54 stars of type G8 III (as 31 Vul was supposed to be at that time), whose 'CN ratios' ranged from 2·33 down to 2·01, 31 Vul was the 30th star down the list, with a ratio of 2·18, so it could be said to be close to the middle of the list both in CN intensity and in ranking. Any discrepancy between that fact and the classification, is, however, not necessarily conclusive, as the classifier certainly would not be defining CN strength in the same manner as the objective measurements did.

In 1985 Keenan & Yorka[48] substituted G7 III Fe–1 for the previously adopted type, and in 1988 they[49] further adjusted it to G7·5 III Fe–1, but the following year Keenan & McNeil[50] restored it to G7 III Fe–1. There the matter rests, since although Keenan was engaged in a further revision[31] of the spectral classifications of late-type MK standards at the time of his lamented demise, he was working through it in right-ascension order and had not got as far as 31 Vul, which is at nearly 21^h RA.

It is noticeable that the colour indices of 31 Vul, at $0^m \cdot 83$ and $0^m \cdot 47$, are considerably bluer than those of the majority of stars of its spectral type; in fact they are bluer than those of, for example, HR 6840 (G3 III) and ω^2 Sco (HR 5997, G3 II–III), and the $(B-V)$ distribution of stars classified as G7 III goes at least as far redwards as the $1^m \cdot 26$ of HR 6590. It may be significant that the early classifications of 31 Vul from Mount Wilson[37,26] were as early as G2, and even the 'measured' one was G3. There seems never to have been any suggestion that the late-type spectrum is adulterated by any admixture with an early-type one. Hansen & Kjærgaard[51], however, derived from narrow-band measurements obtained by Dickow *et al.*[52] in a photometric system developed at Copenhagen, a value of $0^m \cdot 035$ for the quantity *res* (k). That quantity is a diagnostic of compositeness, and its value as observed for 31 Vul is only just short of the level ($0^m \cdot 04$) that is considered[51] really significant, implying that the object concerned is either binary or heavily reddened.

The parallax given by *Hipparcos*[14] appears to fix the distance modulus at $4^m \cdot 11 \pm 0^m \cdot 09$, and therefore to constrain the absolute magnitude to be similarly close to $+0^m \cdot 46$. The parallax was, however, derived from an 'acceleration solution' and might be more trustworthy if it were re-computed with the actual orbit parameters, as established here, taken into account.

The literature includes quite a number of references to the use of 31 Vul as a comparison star for various purposes, including photometrically as a standard for such stars as SV Vul and T Vul, and also spectroscopically. It even appears *twice* in a list of 20 — actually, therefore, only 19 — reference stars used by Soubiran *et al.*[53] in connection with the open cluster NGC 2355. As a spectroscopic reference it has featured in no fewer than four of the papers on composite spectra in which the present writer has collaborated with Dr. R. E. M. Griffin; it was compared with the late-type components of π Aql[54] and HR 5983[55], and was actually used as a surrogate for the corresponding component in the cases of α Equ[56] and 93 Leo[57]. In both of the latter papers there is a figure that includes a tracing of λλ 4250–4450 Å from a 10-Å mm^{-1} photographic spectrogram of 31 Vul, and in the 93 Leo case there is in addition one covering λλ 4420–4558 Å. A reproduction of a spectrogram of 31 Vul at classification dispersion (40 Å mm^{-1}) appears in the atlas by Ginestet *et al.*[58].

Radial velocities and orbit for 31 Vul

The first measurement of the radial velocity of 31 Vul was made[8] at Lick with the *New Mills Spectrograph* on the 36-inch refractor in 1906; the first publication of it was in the *Lick Observatory Bulletins*[59] in 1913, where a single figure of −0·2 km s^{-1} was given to represent the mean from a number of plates that was unspecified but was implied to be at least three. It features in a section of the listing devoted to stars of type K, but it is not clear whether that was intended to be a considered classification. The full and final publication of the Lick results, in which the observations are listed individually, shows that there were altogether six measurements, of which five could be expected to have been included in the 1913 mean. The six have a mean of –0·5 ± 0·4 km s⁻¹; the spread of the individual measurements is 1·0 km s⁻¹ r.m.s., which is worse than average for the Lick velocities but not so much as to suggest real variability.

In 1914, soon after the initial Lick paper[59] was published, Küstner[60] presented five radial velocities measured from plates taken with a spectrograph giving 29·5 Å mm⁻¹ at Hγ on the 12-inch *Repsold* refractor at Bonn. Küstner's measurements, like the Lick ones, have proved very reliable; their spread of 9 km s⁻¹ in the case of the 31 Vul observations more than justifies the substitution in his table of results of "var.?" in place of a mean value. In the following year, Adams[61] gave a mean Mount Wilson velocity for 31 Vul, under the alias of Boss[62] 5373, of +3·3 km s⁻¹. He listed the spectral type as G8, but said that the classifications were 'mostly' made by Kohlschütter, and he evidently did not consider them to be of sufficient merit to warrant co-authorship or even thanks, so they are perhaps not to be regarded as seriously as those (considerably different) ones given[37,26] later by Adams and his collaborators. Long afterwards, Abt[63] published the Mount Wilson radial velocities individually; there are six of them, all made in 1914 or 1915. They have a mean of +3·8 km s⁻¹ with a formal standard error of only 0·6 km s⁻¹, which must, however, be flukily small, because the dispersion of the individual values is much less than the internally estimated 'probable errors' of half of them.

Harper[64] subsequently obtained a single measurement of the radial velocity of 31 Vul at the DAO; his paper refers to comparisons of his own observations with those in the Lick catalogue[8] but with no others, so it is not clear whether he was taking the Bonn and/or Mount Wilson velocities into account when he noted, "Suspect s[pectroscopic].b[inary]." His own measure was considerably different from the Lick mean, and proves to have an unusually bad residual in the orbit solution below.

On the strength of Küstner's and/or Harper's suspicions, the radial velocity of 31 Vul was noted as "var?" in the second edition[65] (1940) of the *Bright Star Catalogue*, and seemingly without any further radial-velocity evidence it solidified into the specific assertion, "The star is a spectroscopic binary" in the third[66] (1964). Before the current (fourth) edition[5], in which 31 Vul is again noted as a spectroscopic binary, was published, there was probably some additional input in the form of information supplied privately by Beavers, who in collaboration with Eitter subsequently published[6] 13 observations of the star. They were made with the photoelectric radial-velocity spectrometer[67] at the Iowa State University at Ames. The very first of the Ames velocities, though accorded quality 'B', is definitely discordant with all the rest, which are in tolerable mutual agreement both with one another and with the Lick and Mount Wilson velocities. The present author did consider, a long time ago, whether the Beavers & Eitter material[6] was sufficiently convincing to make 31 Vul an attractive candidate for observation as a binary, but he decided *not*. The Bonn observations[60] would have clinched the case, but the writer was not aware of them at that time, because they were overlooked in the normally very complete Abt & Biggs bibliography[68] and so did not come to hand in a casual review of the literature. What did convince him, much later, was the pair of velocities, definitely discordant with one another and one of them discordant with all previous known measures apart from the outlying Ames one, by de Medeiros & Mayor[2]. When those authors sent their material to the Centre de Données Stellaires so that the observations could be seen individually, 31 Vul was one of the stars selected for addition to the Cambridge binary programme.

TABLE III

Radial-velocity observations of 31 Vulpeculae

The sources of the observations are as follows:
1906–1908 and 1926 — Lick[8]; 1909–1913 — Bonn[60]; both weight 0·05;
1914/5 — Mt. Wilson[61,63]; 1923 — DAO[64]; both weight 0;
1976–1983 — Ames[6], weight 0·025; 1986/7 — OHP[2], weight 1;
2002–2007 — Cambridge Coravel , weight 1.

Date (UT)	*MJD*	*Velocity* km s^{-1}	*Phase*	$(O-C)$ km s^{-1}
1906 Sept. 11·32	17464·32	−1·2	$\overline{19}$·269	−1·8
1907 July 11·35	17767·35	+0·3	$\overline{19}$·432	+0·3
Aug. 19·27	806·27	−0·2	·453	−0·2
Sept. 9·30	827·30	+0·3	·464	+0·3
1908 July 21·42	18143·42	+0·7	$\overline{19}$·634	−0·3
1909 Oct. 18·83	18597·83	+6·8	$\overline{19}$·878	+1·0
Nov. 19·72	629·72	+5·9	·895	−0·6
1911 Nov. 7·73	19347·73	0·0	$\overline{18}$·281	−0·5
23·73	363·73	−1·3	·290	−1·7
1913 Sept. 25·86	20035·86	+1·2	$\overline{18}$·651	+0·1
1914 Sept. 2·29	20377·29	+2·8	$\overline{18}$·834	−1·7
6·25	381·25	+5·2	·837	+0·7
29·23	404·23	+4·1	·849	−0·8
1915 June 22·48	20670·48	+5·4	$\overline{18}$·992	−3·2
Sept. 23·17	763·17	+3·4	$\overline{17}$·042	−3·7
Nov. 27·11	828·11	+6·6	·077	+1·1
1923 Aug. 21·26	23652·26	−3·9	$\overline{16}$·595	−4·5
1926 Sept. 6·36	24764·36	+1·9	$\overline{15}$·192	+0·2
1976 Sept. 21·17	43042·17	+10·2	$\overline{5}$·016	+2·1
1977 July 6·40	43330·40	+3·2	$\overline{5}$·171	+1·1
Sept. 10·26	396·26	+2·2	·206	+0·8
Oct. 14·15	430·15	+3·4	·224	+2·3
1978 July 27·29	43716·29	+0·6	$\overline{5}$·378	+0·6
Aug. 5·28	725·28	+2·2	·383	+2·2
14·23	734·23	+1·7	·388	+1·7
23·20	743·20	+1·6	·392	+1·6
Nov. 8·05	820·05	+0·1	·434	+0·1
1979 June 30·41	44054·41	+0·1	$\overline{5}$·560	−0·3
1982 July 13·32	45163·32	+2·5	$\overline{4}$·156	0·0
1983 July 19·32	45534·32	+0·5	$\overline{4}$·355	+0·4
Sept. 7·18	584·18	+1·4	·382	+1·4
1986 Aug. 16·02	46658·02	+8·2	$\overline{4}$·959	−0·2

TABLE III (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O – C)* *km s*$^{-1}$
1987 Aug. 9·07	47016·07	+2·7	$\bar{3}$·151	+0·1
2002 July 15·06	52470·06	+5·3	0·083	+0·1
Sept. 1·99	518·99	+4·2	·109	+0·1
Oct. 4·91	551·91	+3·3	·127	−0·1
Nov. 13·72	591·72	+2·6	·148	−0·1
2003 Jan. 11·72	52650·72	+1·9	0·180	0·0
Apr. 29·11	758·11	+0·7	·238	−0·2
May 28·10	787·10	+0·7	·253	0·0
June 21·10	811·10	+0·6	·266	0·0
July 20·98	840·98	+0·6	·282	+0·1
Aug. 17·05	868·05	+0·6	·297	+0·2
Sept. 14·96	896·96	+0·4	·312	+0·1
Oct. 17·91	929·91	+0·2	·330	0·0
Nov. 27·79	970·79	0·0	·352	−0·1
2004 Jan. 2·75	53006·75	0·0	0·371	0·0
May 23·10	148·10	−0·1	·447	−0·1
June 22·11	178·11	−0·2	·463	−0·2
Aug. 8·11	225·11	+0·4	·488	+0·3
Sept. 1·01	249·01	+0·3	·501	+0·2
Oct. 21·90	299·90	+0·2	·529	0·0
Nov. 26·78	335·78	+0·1	·548	−0·2
Dec. 26·72	365·72	0·0	·564	−0·4
2005 May 23·11	53513·11	+1·0	0·643	−0·1
June 23·05	544·05	+1·5	·660	+0·3
July 18·06	569·06	+1·6	·673	+0·2
Aug. 15·07	597·07	+1·7	·688	+0·1
Sept. 14·97	627·97	+1·9	·705	+0·1
Oct. 25·90	668·90	+2·2	·727	+0·1
Nov. 18·83	692·83	+2·4	·740	+0·1
Dec. 11·75	715·75	+2·6	·752	0·0
2006 Apr. 5·19	53830·19	+3·7	0·814	−0·2
May 30·12	885·12	+4·4	·843	−0·3
July 3·10	919·10	+5·3	·861	0·0
Aug. 10·05	957·05	+6·0	·882	0·0
Sept. 8·02	986·02	+6·6	·897	+0·1
Oct. 4·99	54012·99	+6·8	·912	−0·2
31·88	039·88	+7·7	·926	+0·2
Nov. 23·79	062·79	+8·2	·939	+0·3
Dec. 16·78	085·78	+8·4	·951	+0·1
2007 Jan. 11·73	54111·73	+8·7	0·965	+0·2
Apr. 12·18	202·18	+8·2	1·014	0·0
May 1·15	221·15	+7·4	·024	−0·5
19·13	239·13	+7·3	·033	−0·2
June 1·12	252·12	+7·3	·040	+0·1
21·12	272·12	+6·8	·051	+0·1
July 8·12	289·12	+6·5	·060	+0·2
25·07	306·07	+6·1	·069	+0·3
Sept. 7·90	350·90	+4·7	·094	0·0
Oct. 20·93	393·93	+3·6	·117	−0·2
Dec. 5·83	439·83	+2·9	·141	0·0

Three isolated radial-velocity measurements of 31 Vul have been mentioned by comparatively recent authors. Stout-Batalha *et al.*[69] obtained a velocity of +8·1 km s^{-1} and commented on the discrepancy from the catalogue mean value of +1 km s^{-1}, but they gave no date; Strassmeier & Schordan[70] and Soubiran *et al.*[71] each listed one measurement. The value of isolated measurements at times not far removed from those covered by the writer's systematic observations is not great, so those measurements have not been included in the discussion here.

The number of observations made with the Cambridge *Coravel* is 49; they are listed in Table III, and cover a little more than one five-year cycle of the velocity variation. There are modest gaps in their phase coverage, but they cannot be remedied on an acceptable time-scale owing to the proximity of the period to an integral number of years. By themselves, the Cambridge observations establish the orbital period as 1859 ± 6 days, a value that can be significantly refined by the inclusion of the earlier observations. Those observations have been transcribed to the head of Table III; in an effort to place all the data on the zero-point[72] normally adopted in this series of papers, the published ones have been adjusted by +0·8 km s^{-1}, plus, in the cases of the Mount Wilson[61,63] and DAO[64] ones, the corrections suggested for G-type stars in the *Radial Velocity Catalogue*[73], and the Cambridge *Coravel* measurements have been adjusted by –0·3 km s^{-1}.

In the solution of the orbit, the variances of the different sources have been approximately equalized by giving the *Coravel* observations unit weight, the Lick and Bonn ones weight 0·05, and the Ames ones 0·025 (halved in the cases of those that the Ames authors noted as quality 'B'). The Mount Wilson and DAO velocities have not been included in the calculation, which has produced the orbit that is illustrated in Fig. 2 and has the following elements:

P	=	1860·6 ± 1·3 days	$(T)_0$	=	MJD 52316 ± 6
γ	=	+2·75 ± 0·03 km s^{-1}	$a_1 \sin i$	=	103·0 ± 1·1 Gm
K	=	4·34 ± 0·04 km s^{-1}	$f(m)$	=	0·0126 ± 0·0004 $M_{\odot}$
e	=	0·375 ± 0·009			
ω	=	15·1 ± 1·4 degrees	R.m.s. residual (wt. 1)	=	0·18 km s^{-1}

The mass function is small and does not require the secondary to be more massive than about 0·4 $M_{\odot}$, corresponding to a main-sequence type near M2, if the primary is supposed to have about twice the solar mass. The secondary could easily be a much more conspicuous object, if the orbital inclination were low. It would need an inclination only just under 30° to imply a secondary mass of 1 $M_{\odot}$, and it would not need to be much lower to be consonant with the mass of an F-type star which could be held responsible for making the system appear bluer than normally corresponds to its spectral type. The $a_1 \sin i$ value is a little more than ⅔ of an AU, so the apparent orbit of the primary star could have a size of the order of ⅔ of the parallax or 0″·010; the maximum angular separation could be expected to be at least double that, since the secondary is doubtless less massive than the observed star, and if the secondary had the minimum mass demanded by the spectroscopic orbit the separation could be several times as great. It must be recalled, however, that if the very slight barium enhancement that was suggested at one time by Keenan & Wilson[43] really signifies that 31 Vul is a 'marginal barium star', then the secondary ought to be a white dwarf.

The radial-velocity 'dips' — and correspondingly, no doubt, the spectral lines — of 31 Vul are noticeably broadened by axial rotation of the star. The $v \sin i$ value was given by de Medeiros & Mayor[2], on the basis of their two OHP

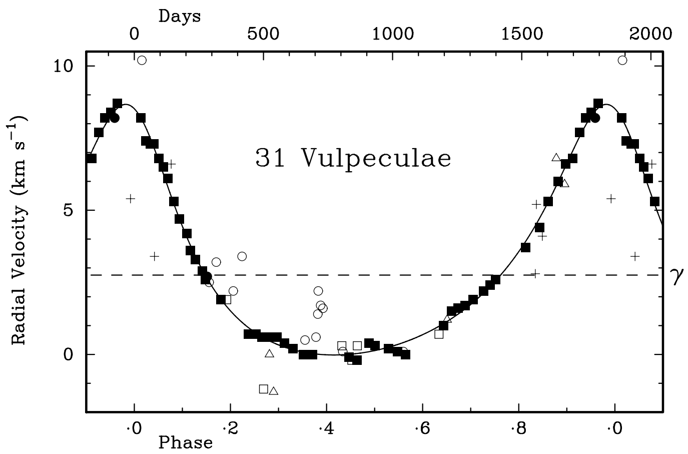

FIG. 2

The observed radial velocities of 31 Vul plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The Cambridge and OHP observations are plotted as filled squares and circles, respectively, as in Fig. 1, but in this case the OHP measures[2] were accorded full weight. Measurements from Lick[8] (open squares), Bonn[60] (open triangles), and Ames[6] (open circles) were weighted 0·05, 0·05, and 0·025, respectively, in the solution; the five Ames velocities noted with quality 'B' by their authors had their weighting halved. No weight was given to the Mount Wilson observations[61,63] (plusses) or the single DAO one[64] (off the bottom of the plot).

Coravel observations, as 4·7 ± 1·0 km s^{-1}. The values given by the Cambridge *Coravel* repeat very well from one trace to another, and with so many of them a formally very precise result of 5·78 ± 0·13 km s^{-1} is obtained, although the naïveté of the principle of the measurement, which effectively supposes non-rotational sources of broadening to affect all stars equally, means that projected rotational velocities derived from radial-velocity traces are not claimed to be accurate to better than 1 km s^{-1}. The more sophisticated method practised by Gray[74] has given $v \sin i = 5·9 \pm 0·3$ km s^{-1} for 31 Vul.

Young, Ajir & Thurman[75], in a paper from under whose principal thesis the present writer[76] considers he has comprehensively pulled the rug, included 31 Vul in a table listing 'single evolved stars' and tabulated for it a rotational period of 32 days. They say nothing about the source(s) of the entries in that column, but it may be inferred that they stem from the photometry that had for quite a number of years been carried out on the *H* and *K* lines with a photoelectric instrument[77] on the 60-inch reflector at Mount Wilson. The publication[78] of all those *H*- and *K*-line measurements did not appear until two years after the publication of the paper by Young *et al.*, but even if the latter authors were privy to its contents before publication they would have found only nine observations all made within a total span of only 16 days.

70 Pegasi (HR 8923, HD 221115)

UBV photometry of 70 Peg has been published by Argue[79] and by Johnson *et al.*[80], and (for *B* and *V* only) by Häggkvist & Oja[81]; the agreement is not too good, but mean values could be given as $V = 4^{m}·54$, $(B-V) = 0^{m}·94$,

$(U{-}B)$ = 0^{m}·73. Eggen[82] has given values that are discordant, at 4^{m}·59, 0^{m}·91, and 0^{m}·82, respectively. By a curious coincidence, all four sets of measurements were published in the same year, 1966. It could well be significant that, later, Eggen[83] seemed to prefer to quote others' photometry rather than his own.

The earliest classification of the spectrum of 70 Peg was the 'H?' of the *Draper Catalogue*[15], but the earliest after the Harvard system had settled down appears to be the work of Adams *et al.*[37], who were making wholesale measurements of spectroscopic luminosities. They found the type to be G6 by 'estimation' against spectra of standard stars and G5 by 'measurement' — quantitative comparison of Balmer-line and metallic-line strengths; they deduced an absolute magnitude of +0^{m}·5. Considerably later, partly the same Mount Wilson syndicate[26] revised the type to G9, but found the same luminosity as before. Meanwhile Young & Harper[38], at the DAO, had given the type as K0 and had made independent estimates of +1^{m}·0 and +1^{m}·2, respectively, for the absolute magnitude, Rimmer[25] had given it as +0^{m}·7, and the type had been given in the *Henry Draper Catalogue* as G5.

The first MK type appears to have been given by Miss Roman[41], as G8 III. She added the suffix 'wk-l' [weak-line], which a careful reading of her paper shows is intended to mean that Ca I λ 4227 Å and the *G* band are both *stronger* than average. Shortly afterwards, Halliday[84] offered a classification (made by the normal visual comparison method) of G9 III. In addition, by means of a system introduced by Hossack[85] and employing a microphotometer, he obtained a 'luminosity number' — just a decimal representation of the MK luminosity class, to allow a continuous run of classes instead of quantization at discrete intervals — of 2·8. From a luminosity calibration set up by Miss Roman[41] he deduced an absolute magnitude of –0^{m}·15. 70 Peg was one of the stars that featured in Keenan's several lists of MK standard stars in the 1980s, wherein it held steady at G7+ III[46–48] until the final listing by Keenan & McNeil[50], in which it was revised to G8 IIIa.

It is perhaps a bit ironic that that revision raised 70 Peg to the high-luminosity division of the giant class, subsequently demonstrated by Keenan & Barnbaum[31] from *Hipparcos* parallaxes[14] to have typical luminosities near –1^{m}·5, whereas the star is shown by its own parallax to have M_V = +0^{m}·87 ± 0^{m}·09, indicating that it has the luminosity of a 'clump giant' of luminosity class IIIb. Indeed, in the post-*Hipparcos* era it has been explicitly included in discussions of clump giants by Alves[86] and Zhao *et al.*[87]. *Hipparcos* did notice the orbital motion of 70 Peg, since the parallax is noted as stemming from an 'acceleration solution', but such a solution (even though it includes a cubic term) cannot be an accurate representation of motion in an orbit that was completed in its entirety in less than the duration of the observations. No doubt a re-discussion of the *Hipparcos* data in the light of the orbit given here would be an improvement, but perhaps not a very great one because the $a_1 \sin i$ value found here is less than 0·2 AU.

Radial velocities and orbit for 70 Peg

Just as in the case of 31 Vul, the radial velocity of 70 Peg was measured early on at Lick[7,8] and Bonn[60]. It was first mentioned in a paper[88] by Campbell in the *Lick Observatory Bulletins* in 1911, giving measurements of 68 binary stars newly detected in the course of the then-on-going survey of the radial velocities of all stars brighter than 5^{m}·5. Six observations of 70 Peg, dating from 1907 to 1910, were included; they showed significant discordance, having a range of 5·6 km s^{-1}. The discovery of the binary character of 70 Peg was credited to Miss Hobe, who was the most prolific of all the Lick plate measurers. The star featured again in an interim listing[7] of mean velocities in 1913, wherein it is attributed a mean of −14 km s^{-1}; the number is given without a decimal place, and is followed by a colon together with the note, "Sp. bi.; estimated velocity of the center of mass." In the final publication[8] of the results of the Lick survey, however, when careful correction of systematic errors had been carried out, the range of the six measurements was reduced to 3·9 km s^{-1} and was not increased by two additional measurements made after 1910; nothing was said about variability, and the mean velocity was given complete with one decimal place like most of the other entries. Evidently it was considered that the announcement of variability had been premature. But that is not necessarily so; after all, the radial velocity of 70 Peg *is* variable, and its variability certainly contributed substantially to the scatter of the six measurements upon which the initial claim was based. The variance of the six velocities about their own mean (so there are five degrees of freedom left) was 5·35 (km s^{-1})^2 per individual observation in the original version[88], reduced to 3·05 in the final one[8]. The corresponding quantity representing the residuals from the orbit determined below, in which the Lick measurements contribute so little weight that the number of degrees of freedom remains practically at six, is only 0·62 (km s^{-1})^2. That represents an unbiassed estimate of the true observational errors, without inflation by actual change of the quantity under observation. Armed with those figures, we can use Snedecor's[89] F test to obtain the variance ratio $F_{5,6} = 8{\cdot}6$ in the first case[88] and 4·9 in the second[8]. The 1%-significance point of $F_{5,6}$ is[89] 8·75 and the 5% point 4·39. In retrospect, therefore, we can see that the claim of variability was made on quite strong grounds, and could well have been maintained in the final publication[8].

Three radial-velocity observations of 70 Peg were published from Bonn by Küstner[60] in 1914; although they are in mutual agreement and also agree with the Lick mean, the note "var.", presumably taking its cue from Lick[88], is substituted for a mean value. Harper[64] gave a single measurement from the DAO, coincidentally obtained on the same night as his one observation of 31 Vul. Young[90] obtained five measurements with the David Dunlap Observatory 74-inch reflector, but presented them only as an undated mean value, so they cannot be utilized here. The mean is given as −13·1, with a 'probable error' of 1·0 km s^{-1}, suggesting an r.m.s. spread of about 3 km s^{-1} for the individual velocities.

Much later — indeed, after a gap of more than 50 years since the latest of the previous dated measurements was published — 14 radial velocities from the Fick Observatory spectrometer[67] at Ames, Iowa, were given by Beavers & Eitter[6]. They show a range, unusually large for that instrument, of nearly 7 km s^{-1} (it is more than twice the range of the residuals of the same observations from the orbit below), but no claim of real variation was made. Most recently, de Medeiros & Mayor[2] referred to two velocities obtained of 70 Peg with the OHP *Coravel*; they were subsequently included in a comprehensive listing, lodged with the Centre de Données Stellaires, of the data underlying that paper, and it was from that material that 70 Peg was selected for observation at Cambridge. The same two velocities were referred to again (but without being specified) by another, overlapping, *Coravel* syndicate[91].

70 Peg was placed on the Cambridge radial-velocity observing programme in 1982 and has been observed reasonably systematically since then, 46 measurements having been made. The period is near a half-integral number of years (2½ years), facilitating good phase coverage. To begin with, the variation of velocity seemed to be small and slow, but an abrupt change in 2003 gave

TABLE IV

Radial-velocity observations of 70 Pegasi

Except as noted, the sources of the observations are as follows:
1907–1923 — Lick[8]; 1976–1983 — Ames[6]; both weight 0·025;
2002–2009 — Cambridge Coravel , weight 1.

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O – C)* *km s*$^{-1}$
1907 Sept. 9·40	17827·40	–13·2	$\overline{37}$·813	–0·8
1908 July 30·49	18152·49	–15·8	$\overline{36}$·158	–0·3
Oct. 22·27	236·27	–15·8	·247	–0·6
Nov. 17·21	262·21	–15·5	·275	–0·4
1909 July 10·47	18497·47	–12·7	$\overline{36}$·525	+1·4
Nov. 19·85*	629·85	–12·8	·665	+0·7
27·82*	637·82	–12·7	·674	+0·7
1910 Oct. 3·32	18947·32	–11·9	$\overline{35}$·003	–0·7
1911 Sept. 1·29	19280·29	–15·0	$\overline{35}$·357	–0·2
1912 Oct. 10·94*	19685·94	–13·9	$\overline{35}$·788	–1·2
1923 Aug. 21·35†	23652·35	–10·5	$\overline{30}$·003	+0·6
Oct. 19·30	711·30	–13·4	·065	+2·1
1976 Oct. 8·23	43059·23	–13·6	$\overline{10}$·626	+0·1
10·22	061·22	–14·4	·628	–0·7
1977 Sept. 11·33	43397·33	–9·1	$\overline{10}$·985	+0·3
Oct. 21·20	437·20	–15·0	$\overline{9}$·027	–0·7
1978 Aug. 9·37	43729·37	–14·1	$\overline{9}$·338	+0·7
24·35	744·35	–15·4	·354	–0·6
Nov. 3·17	815·17	–14·8	·429	–0·3
1980 Oct. 5·20	44517·20	–14·6	$\overline{8}$·175	+0·8
1982 Aug. 12·36	45193·36	–12·0	$\overline{8}$·893	–0·6
Sept. 4·31	216·31	–10·5	·918	+0·4
Oct. 8·19	250·19	–11·3	·954	–1·4
1983 Aug. 25·36	45571·36	–13·0	$\overline{7}$·295	+2·0
Sept. 7·29	584·29	–15·8	·309	–0·9
8·25	585·25	–15·9	·310	–1·0
1989 Oct. 22·84‡	47821·84	–14·2	$\overline{5}$·687	–0·9
1990 June 21·10‡	48063·10	–10·8	$\overline{5}$·943	–0·6
2002 July 21·08	52476·08	–13·4	0·632	+0·2
Sept. 2·05	519·05	–13·4	·678	0·0
11·05	528·05	–13·2	·688	+0·1
Nov. 12·94	590·94	–13·0	·754	–0·1
2003 Jan. 11·82	52650·82	–12·7	0·818	–0·3
July 13·10	833·10	–12·8	1·012	–0·2
Sept. 16·02	898·02	–15·7	·081	–0·1
Oct. 8·07	920·07	–15·7	·104	–0·1
Nov. 5·93	948·93	–15·8	·135	–0·2
Dec. 7·80	980·80	–15·4	·169	+0·1

TABLE IV (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2004 Jan. 2·78	53006·78	–15·6	1·196	–0·2
June 13·11	169·11	–14·9	·369	–0·2
July 6·11	192·11	–14·5	·393	+0·1
Aug. 7·16	224·16	–14·3	·427	+0·2
Sept. 2·04	250·04	–14·4	·455	0·0
Oct. 6·03	284·03	–14·2	·491	0·0
Nov. 12·93	321·93	–14·1	·531	0·0
Dec. 16·81	355·81	–13·9	·567	0·0
2005 Jan. 13·75	53383·75	–13·9	1·597	–0·1
July 18·11	569·11	–12·3	·794	+0·3
Sept. 3·08	616·08	–12·1	·844	0·0
28·07	641·07	–11·7	·870	+0·1
Oct. 25·96	668·96	–11·3	·900	0·0
Nov. 17·85	691·85	–10·8	·924	–0·1
Dec. 8·80	712·80	–10·0	·947	+0·1
2006 Jan. 4·74	53739·74	–9·3	1·975	0·0
28·74	763·74	–10·7	2·001	+0·2
Aug. 30·05	977·05	–15·1	·227	+0·1
Sept. 30·10	54008·10	–15·1	·260	0·0
Oct. 26·97	034·97	–15·2	·289	–0·2
Nov. 29·79	068·79	–14·6	·325	+0·3
Dec. 28·83	097·83	–14·7	·356	+0·1
2007 Dec. 7·92	54441·92	–13·2	2·721	–0·1
2008 July 30·13	54677·13	–9·3	2·971	+0·1
Aug. 15·12	693·12	–9·7	·988	–0·1
30·08	708·08	–11·4	3·004	0·0
Sept. 10·04	719·04	–13·2	·016	–0·1
11·99	720·99	–13·3	·018	+0·1
19·02	728·02	–14·1	·025	0·0
26·99	735·99	–14·6	·034	+0·1
Oct. 1·99	740·99	–14·9	·039	+0·1
10·95	749·95	–15·2	·049	+0·1
21·95	760·95	–15·4	·060	+0·1
31·95	770·95	–15·7	·071	–0·1
Dec. 17·81	817·81	–15·7	·121	–0·1
2009 Jan. 18·72	54849·72	–15·5	3·155	0·0

*Bonn observation[60], weight 0·025
†DAO observation[64], weight 0·025
‡OHP observation[2], weight 0·25

notice that the eccentricity is high and that a periastron event had been missed. The rise to a sharp maximum of velocity was seen right at the end of the 2005/6 observing season, and the descent from it was carefully followed early in the 2008/9 one. The observations are listed in Table IV, together with the published ones (eight from Lick[8], three from Bonn[60], one from the DAO[64], 14 from Ames[6], and two from OHP[2]). The Cambridge measures have been adjusted by –0·2 km s^{-1} from their 'as initially reduced' values, as in the case of 58 Psc which is of very similar colour index; all the others have been increased by 0·8 km s^{-1}, plus the offsets suggested in Table 3 of the *Radial Velocity Catalogue*[73] for the Bonn and DAO ones. The period given by a solution of the Cambridge velocities alone is 941·6 ± 0·5 days; by bringing in the rest of the data, with weights of 0·025, apart from the OHP ones which were attributed weight ¼, and two of the Ames measures that were accorded quality 'B' by their authors and whose weight is halved, the period is refined to 941·03 ± 0·12 days. The velocity curve is plotted in Fig. 3, and the orbital elements are as follows:

P	=	941·03 ± 0·12 days	$(T)_2$	=	MJD 53763·0 ± 0·9
γ	=	−13·72 ± 0·03 km s^{-1}	$a_1 \sin i$	=	28·7 ± 0·5 Gm
K	=	3·16 ± 0·04 km s^{-1}	$f(m)$	=	0·00107 ± 0·00005 $M_\odot$
e	=	0·713 ± 0·006			
ω	=	57·0 ± 1·2 degrees	R.m.s. residual (wt. 1)	=	0·15 km s^{-1}

The noteworthy feature of the orbit is of course its high eccentricity. The mass function is small and does not require the secondary star to have a mass larger than 0·4 $M_\odot$ if the primary's is supposed to be 2 $M_\odot$. The secondary is not likely to be a white dwarf, because if it had gone through its evolution as a giant it would scarcely have left the orbit with such a high eccentricity. It is therefore, rather certainly, a star on the lower main sequence; it could be as late as about M2.

Slight disquiet is created by the residuals of −0·9 and −0·6 km s^{-1} given by the two velocities[2] from the OHP *Coravel*. Those are quite unusual residuals for *Coravel* measures of stars whose radial-velocity traces show good 'dips', as witness the fact that the *largest* residuals among the 46 Cambridge measures are only 0·3 km s^{-1}. Whereas the weightings of the other sources have been chosen to bring their weighted variances into approximate equality, it has not been considered appropriate to down-weight the OHP measures to anything like that extent. Significant residuals from an epoch well removed from that of the majority of the observations often indicate a variation of the γ-velocity, with the implication that the system is of higher multiplicity. In this case, however, there is no other evidence to support such an hypothesis, which is altogether too far-reaching to be adopted on such a slender basis as two bad residuals.

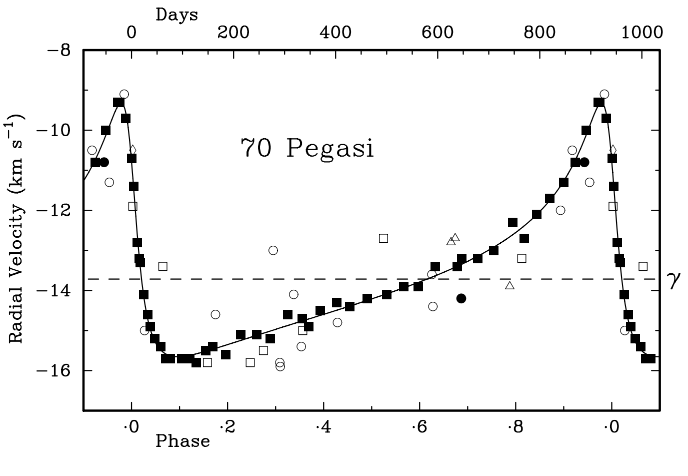

FIG. 3

As Fig. 2, but for 70 Peg. The plotting symbols are the same, but the weightings were 0·25 for OHP and 0·025 for all the other minor series; the single DAO point is plotted as an open diamond.

As a tailpiece to this account of the star, a minor mystery concerning one of its *names* may be mentioned. In Baily's *British Association Catalogue*[92] of 1845, the star is number 8182. Bailey made a most thorough and incisive critique of the identifications of stars in previous catalogues, and is believed by the present writer to be regarded as the arbiter of last resort in any discrepancies or differences of opinion that may arise, as they sometimes *do*[93,94]. The introductory text to the *Catalogue* includes a total of 28 pages of detailed comments, in sections entitled "Revision of the Constellations", "BAYER's mode of lettering the Stars", and "Errors in FLAMSTEED's Catalogue". In the body of the *Catalogue*[92], 70 Peg is also designated 'q Pegasi'. But in Baily's earlier (1835) volume, *An Account of the Rev.d John Flamsteed, to which is added his British Catalogue of Stars*[95], there is another considerable section concerning the identifications of stars; there then follows a reprinting — in effect a new, revised, and corrected edition — of Flamsteed[96], in which 70 Peg appears on p. 503 as no. 3220. An asterisk draws attention to a corresponding note, one of many after the end of the *Catalogue* proper, on p. 642; it reads, "70 *Pegasi*. This star is designated by the letter *q*, in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it." But ten years later it had somehow sprung back again!

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REVIEWS

People and the Sky: Our Ancestors and the Cosmos, by A. Aveni (Thames & Hudson, London), 2008. Pp. 252, 24 × 16·5 cm. Price £18·95 (hardbound; ISBN 978 0 500 05152 8).

Today our astronomy is based on God-free theories. We are shackled by a set of underlying 'truthful' principles that can only be changed after much thought and discussion. A rational and logical procedure lies at the foundation of our astronomical endeavours. But it wasn't like that in the distant past. Then, if you wanted the Sun to move back into the northern sky and winter to end, or if you just wanted the Sun to come back tomorrow, you had to give it something to make it worthwhile. Choosing what (or whom) to sacrifice was an important part of the ancient astronomer's job.

Today we are reductionists. The Principle of Simplicity (the solution that is easiest to understand is the correct one) and Ockham's Razor (*entia non sunt multiplicanda praeter necessitatem*) restrict our activities. But again it wasn't like that in the distant past. If you needed the odd dragon to help you out (say swallowing the Sun during a total eclipse!), so be it. You were not shackled by purposeful forces or abstract laws.

Professor Anthony Aveni (Colgate University) is one of the founders of archaeoastronomy. In *People and the Sky* he takes us way back to the times when astronomy had a social and religious purpose, to the times when our subject was technology-free, and animate. In those days astronomers indulged in a two-way dialogue with the supposedly super-natural forces that governed the skies. What happened above was thought to influence greatly what happened down here below. Astronomers searched for the intricate harmonies, not just so they could fathom out the underlying astrophysics, but more so they could 'peek round the corners of time' and foretell future events.

We are also taken back to a time when the sky mattered. Only a few per cent of the population lived in cities. The rest were out there under the dark starry sky. And they used it. The constellations were the back-drop to stories and myths. The Sun, Moon, and stars were the foundation of the calendar, clock, and compass. Hunters, farmers, sailors, and rulers used the sky and relied on its information. Full Moons, equinoxes, and solstices were not just minor footnotes in annual diaries, they were important events which mattered. Everyone knew the cardinal directions and these were used when travelling, building houses, planning towns and cities, and digging graves.

Aveni introduces us to a world that is far away from our modern understanding of astronomy, a distant world cloaked in many mysteries, and from which few useful clues have survived. He is a great advocate for archaeoastronomy and ethno-astronomy. He writes well and succinctly, and is consistently thought-provoking. The text is a first-class introduction to the astral understanding of our distant ancestors. It also well illustrated, well referenced, and has a useful bibliography. — DAVID W. HUGHES.

The Planet Gods, by Jacqueline Mitton & Christina Balit (Francis Lincoln, London), 2008. Pp. 32, 21 × 28 cm. Price £6·99 (paperback; ISBN 978 1 84507 923 9).

A typical two-page spread in this strikingly colourful book is dominated by a Christina Balit painting of a planet's surface being flown over by the appropriate Greek god or goddess, coupled with illustrations of the planetary glyph and sequences of rather psychedelic circles in the margins. The same treatment is given to the Sun, Moon, Ceres, Eris, and Pluto, but 'godless' Earth only creeps in on the spread indicating the relative planetary sizes.

Balit's arresting illustrations are accompanied by Jacqueline Mitton's short, appropriate, and whimsical commentaries on the relevant godly and planetary characteristics. Mitton and Balit have collaborated on four books. *The Planet Gods* is a revised and updated version of one of those, their 2001 publication *Kingdom of the Sun*.

Although aimed at children aged 7–11, the book will catch the eye of any age. Each gorgeous image is unfortunately sliced through by the book's spine. This is a great shame. One could easily imagine many of the purchasers of a differently designed book carefully taking it to pieces and framing the images. — CAROLE STOTT.

The Georgian Star, by M. D. Lemonick (W. W. Norton, London), 2009. Pp. 199, 21 × 14 cm. Price £17·99 (hardbound; ISBN 978 0 393 06574 9).

Michael Lemonick is a former science writer at *Time* magazine who today continues his work in the popularization of science through books and articles and through university courses. In this attractive little text he ventures into history with an account of the lives and work of William and Caroline Herschel, as part of Norton's 'Great Discoveries' series. It is now over half a century since J. B. Sidgwick published his popular biography of William Herschel, and that has remained the best introduction to one of the greatest astronomers of all time. In the interim the Royal Astronomical Society has made their treasure trove of Herschel manuscripts universally available, first on microfilm and more recently on CDs and DVDs. This has led to a large amount of primary research, so Sidgwick's biography is no longer satisfactory.

Lemonick has now stepped into the gap, fully exploiting the latest research and presenting it with the adroitness of an experienced popularizer. The result is a charming book that becomes the new standard introduction to William and Caroline Herschel.

However, I am not sure that the most fundamental component of the Herschel revolution in astronomy is stated with the clarity it deserves, and this must be my fault because my own books are generously cited throughout. William Herschel was intrigued by nebulae, and by the problem of whether all of them were distant star clusters, or only some. But either way, he was encountering numerous star clusters, and their very existence proved that an attractive force (gravity or similar) was at work among the stars. Furthermore, some clusters were scattered while others were concentrated, and clearly the attractive force would *in time* cause those that were scattered to become ever more concentrated: scattered clusters are young, concentrated clusters are old, and so clusters go through a life history. This sounded the death knell for the unchanging, clockwork universe of Newton and Leibniz and ushered in the evolutionary astronomy that we know today. — MICHAEL HOSKIN.

British University Observatories 1772–1939, by R. Hutchins (Ashgate, Aldershot), 2008. Pp. 557, 23·5 × 15·5 cm. Price £60 (hardbound; ISBN 978 0 7546 3250 4).

Don't make the mistake that I at first did, of supposing this to be a semi-popular account of how astronomy came to be taught in universities. Rather, this book is a densely written, densely documented work of record and reference — which is unsurprising once you learn that it's a development of the author's DPhil thesis. Even as someone with a significant involvement in my own university's (pre-1939) teaching observatory at Mill Hill, I therefore felt disadvantaged almost from the outset, as I quite quickly came to the conclusion that the book is written for the historian, and not for the astronomer.

However, having accepted the job of writing a review, I pressed on regardless. There were times when I felt I was losing sight of the wood for the trees, but nonetheless I did come away with some lasting insights into aspects of the development of pre-War astronomy in the British Isles. The perennial struggle for resources will come as no surprise to anyone, but I had expected teaching to be a much more important factor in the early history of *university* observatories, whereas much of the focus was, evidently, on research. Of the six university observatories identified as undertaking that research, the narrative is dominated by those of Oxford and Cambridge. The author shows these to be the most significant university observatories of the period, and the book gives the first full histories of these institutes, but the others (Dunsink, Durham, Glasgow, and London) weave in and out of the story to give a complete account of not only the scientific but also the social context.

I can't pretend that I found it easy to read this book — perhaps I was wrong to go through it cover to cover — but I did find it easy to admire its structure as comparative historiography, its thorough and exhaustive treatments, its rigorous referencing of primary sources, and the technical skill evident in the writing. The high standards of scholarship are matched by high production standards — this is a tome that feels good in the hand and looks good to the eye. I'd expect to find it on the shelves wherever the history of science is studied. — IAN D. HOWARTH.

Cosmic Imagery: Key Images in the History of Science, by John D. Barrow (The Bodley Head, London), 2008. Pp. 608, 24 × 18 cm. Price £25 (hardbound; ISBN 978 0 224 07523 7).

Describing astronomical and scientific discoveries without pictures is like trying to explain a spiral staircase with one's hands behind one's back. But nowadays we have no option. We live in the image age. Pictures are everywhere, and they are cheap to produce, colourful, information-rich, and the pixel size (thank goodness) is getting smaller. We have all been overtaken by a visual culture. We are wedded to the PC and the graphics package. Downloading images from the World Wide Web has become second nature. Gone are the days when you could stroll up to the conference podium with two pieces of chalk in your top pocket (one to use, plus a spare). Gone are the days when the production of diagrams for talks and research papers was a laborious and expensive business. And what a good thing this is. Retreating into history we realize that pictures came before writing. Pictures are memorable, pictures inspire. Every subject has its iconic images, and science is no exception.

John Barrow, the Cambridge University professor of mathematical science, clearly loves pictures and this leaps from every page of this delightful book. We are treated to an eclectic collection of iconic scientific images, each engagingly embellished by a page or so of text enlivened by apposite quotations and encouraging footnotes. The majority concern astronomy and mathematics. Barrow revels in the fact that we now live at a time when pictures are everywhere. He concentrates on what he terms the 'iconic image'. Let me mention a few examples. Imagine you are Copernicus and you have to search for your drawing pen while thinking — just how do I illustrate the heliocentric cosmos? Or you are in the shoes of either Hertzprung or Russell and you realize that what your new 'breakthrough' stellar-properties idea really needs is a good diagram. Or you are the young Hubble and the thought that galactic evolution has something to do with a tuning fork springs to mind.

I loved the photograph of the lecturing John Wheeler at the blackboard illustrating the effects of gravity. Wouldn't it have been wonderful if we could have balanced this image with one of Isaac Newton in Trinity College trying to do the same. Then there is the *Hubble Space Telescope*'s image of the towering multicoloured Eagle Nebula juxtaposed against the Earl of Rosse's delicate 1844 drawing of the Crab Nebula. And we are shown a young, dapper Einstein in 1905. Oh, if only the paparazzi had been around when Tycho Brahe went to Prague. Möbius strips compete with Babylonian Pythagorean triangles, Feynman diagrams with the earliest version of the periodic table, double helices with buckyballs.

This hugely enjoyable, colourful, and weighty book reveals a very human approach to science. Barrow accurately targets the general reader. Not only does he make you extremely proud to be a scientist, you will want to give copies of his superb book to your 'artistic' friends to show them just how much they are missing. — DAVID W. HUGHES.

La Carte du Ciel, edited by J. Lamy (EDP Sciences, Paris), 2008. Pp. 252, 24 × 16 cm. Price €29 (about £26) (paperback; ISBN 978 2 7598 0057 5).

Initiated in 1887 by Ernest Mouchez, Director of Paris Observatory, the *Carte du Ciel* was the first large-scale international astronomical project. It aimed to map and catalogue millions of stars as faint as the 11th or 12th apparent magnitude by taking advantage of the then-new photographic technologies. Thus, over several decades, more than 22 000 photographic plates (on glass) were exposed and measured by some twenty observatories over the globe. But the project was never completed. The *Astrographic Catalogue*, a by-product, has been largely ignored until the combined *Hipparcos* and *Tycho* space missions revived an interest in astrometry and in a vast range of related astrophysical and cosmological issues.

Because of the enormous resources, both human and material, it required, the *Carte du Ciel* has often been blamed for delaying the development of astrophysics in Europe. But other factors were also responsible for this, such as, in the first half of the 20th Century, the lack of interest in astrophysics by European astronomy 'barons', traditionally orientated towards positional, theoretical, or solar astronomy. Junior astronomers, attracted by more modern fields, had generally to look for a position outside Europe.

This volume, edited by Lamy, gathers together nine contributions dealing with several historical aspects of the *Carte du Ciel* project, as well as with its sociological, institutional, and organizational context. The emphasis is deliberately French, which can be understood given the origin of the project and the major contribution from France, but foreign readers would probably have preferred a more diversified approach. Only one chapter is centred on a foreign observatory, Potsdam (Algiers was French at the time). The *Hipparcos* space mission is introduced in a specific contribution and shows how estimates of costs and timeline must today be part of any viable scientific project.

The book is well illustrated in monochrome and completed with a name index and a basic bibliography. Readers will find many more references in the individual chapters, all in French, some of them with quotations in English. All in all, I found the volume quite informative and to be recommended to people interested in the history of astronomy (and with a command of French). — A. HECK.

Astronomical Applications of Astrometry — Ten Years of Exploitation of the Hipparcos Satellite Data, by M. Perryman (Cambridge University Press), 2009. Pp. 670, 25 × 19·5 cm. Price £70/$140 (hardbound; ISBN 978 0 521 51489 7).

While being a post-doc based at Liège (Belgium) Institute of Astrophysics, I spent the month of 1976 May at Strasbourg (France) Observatory lecturing on distance-determination methods. The local director, a gentleman moulded in the old-style, somewhat authoritative, managerial approach, so typical of the older French generation of the time, one day put on my desk a set of notes and requested my comments. Puzzled by what seemed to be a poisoned chalice, I started flipping through the pages and discovered a project for an astrometric satellite. The gentleman was Pierre Lacroute (1906–1993) whose directorship of Strasbourg Observatory was the longest on record (30 years) and who had considered carrying out astrometry from space as far back as 1965. With Pierre Bacchus (1923–2007), at Strasbourg Observatory from the end of WWII until the sixties, he had also tackled the challenge of data reduction.

That astrometric space project finally became the *Hipparcos* satellite, launched by the European Space Agency on 1989 August 8, with a broadened scope and an association with the *Tycho* experiment, both involving numerous scientific teams. The operational life of the satellite ended on 1993 August 15. Lacroute had already passed away and could not see the enormous catalogue (17 volumes!) resulting from his brainchild, nor the very numerous investigations carried out from the data as demonstrated, for instance, by the impressive scientific colloquium organized in Venice by ESA in 1997. *Hipparcos* had enabled the measurement of positions, distances, and proper motions, pinpointing more than 100 000 stars, typically 200 times more accurately than ever before.

A bibliographic review is not the place for presenting a scientific synthesis of such a milestone project that impacted so many subfields of astronomy. But do get this book by Michael Perryman for yourself, or make sure your library purchases a copy — possibly even several copies as it is certain the volume will be borrowed frequently. Perryman was ESA's Project Scientist for the *Hipparcos* mission from 1981 to 1997. He was therefore ideally positioned to compile (from some 5000 papers) this masterpiece of about 700 pages. The book is carefully presented and structured, very well documented with plenty of references, and complete with indices and appendices covering numerical quantities, acronyms, and a welcome author gallery. I also appreciated the reproduction of many illustrations and tables, as well as the use of inserts on specific themes. — A. HECK.

The Hunt for Planet X, by G. Schilling (Springer, Heidelberg), 2008. Pp. 317, 24 × 16 cm. Price £15/$27·50/€19·95 (hardbound; ISBN 978 0 387 77804 4).

Pity the poor orrery-maker Eise Eisinga of Franeker in Friesland, when in 1781, after spending years transforming the living room of his house into an accurate model of the then-known Solar System, he reads of the discovery of Herschel's new planet. It is with this story that Govert Schilling starts his book on the hunt for Planet X. But if Eise Eisinga thought he had problems, pity the poor orrery-maker today, with the many new discoveries of Solar System bodies and the IAU reclassification of planets; might we not wonder why orreries are not as popular as they once were?

The biographical-introduction style used with Eisinga is adopted for most of the book, so we learn a little about the personalities involved and then read about their astronomical work. Particularly in the later chapters we are offered descriptive insights into the everyday working lives of astronomers.

The book covers the history of discovery in the Solar System from Uranus up to very recent years. We also learn that disputes over classification are not new and that for nearly 40 years Vesta, Juno, Ceres, and Pallas were treated with the status of planets. Recent claims of conspiracy over the British hunt for Neptune are treated with an appropriate degree of scepticism. When we get onto the story of Pluto we are given some understanding of the reluctance of American astronomers to give it up as a planet.

One thing that comes over very strongly is the revolution in astronomy caused by CCD detectors: this and the greater efficiency of software search programs has led to the discovery of many new Solar System bodies in recent years. Our concepts of the structure of the Solar System are now challenged, with the previously largely theoretical Kuiper Belt starting to be populated.

The discovery of Eris, a body larger than Pluto, challenged astronomers to define what they meant by a 'planet'. A struggle over definitions followed (and continues) despite the decision at the Prague General Assembly of the IAU in 2006 to demote Pluto to 'Dwarf Planet' status.

The book is well illustrated in colour, with pictures located in the relevant sections of the text. At the end is a helpful table with data on large ice dwarfs. I found it particularly useful to correlate the official IAU names with some of the jocular names given by astronomers to their discoveries.

I found a few minor errors: Herbert Turner was Chief Assistant at Greenwich, not Astronomer Royal, and Ceres a patron goddess of Sicily rather than patron saint, but these do not detract much from the value of the book. There are no formal bibliographic references, but often enough information is given in the main text to track down references. In general this a good introduction to the history of Solar System discovery, particularly in recent years. — MARK HURN.

The Crowded Universe: the Search for Living Planets, by A. Boss (Perseus, London), 2009. Pp. 256, 24 × 16·5 cm. Price £15·99/$26 (hardbound; ISBN 978 0 465 00936 7).

As I watched the launch of the *Kepler* satellite the other night, I couldn't help feeling that we had indeed entered a new era in our search for habitable extrasolar earths. This book is meant to be a personal record of the history of exoplanet research and, in particular, space research specifically from an American viewpoint, and is timed to capitalize on, or rather celebrate, the start of this new era. Being quite ignorant of *Kepler*'s history, I really did enjoy large parts of this book — you have to take your hat off to Bill Borucki for his perseverance in seeing *Kepler* through to the launch pad in the face of budget cuts and unsupportive administrators; most people would have given up long before. Well done, Bill! Your vision promises to change the way we look at ourselves in the Universe.

The 'plot' of this book is basically a new slant on the old story of good *versus* evil (cowboys and indians, cops and robbers, *etc.*), but in this case it's America *versus* the rest of the World, and in particular Europe. As I don't care about Joe

Bloggs' nationality I didn't enjoy this aspect of the book and I found that, initially, it completely coloured my view of it (this story line being especially prevalent in the opening chapters). I don't think I really appreciated quite how much Mayor and Queloz's original discovery and, of course, *CoRoT*, has marked the American psyche (or at least the author's!); I hope it is just a ploy to help market the book. Personally, I think the history of exoplanet research is exciting enough that it doesn't need this angle. Once I got over that, I found Boss's discussion of the development of the subject extremely interesting and his interpretation and explanation of some of the key scientific papers revealing. He has developed one of the two leading ideas for planet formation (disc fragmentation) and it was rewarding to see him discuss developments in this context in a more casual way than in a 'real' science paper. For some political issues, clearly the wounds are too fresh, so they are only mentioned in passing (*e.g.*, the lobbying of the Senate by the proponents of the *Space Interferometry Mission*, bypassing the official channels). There are also a number of factual errors (*e.g.*, *Robonet* is not a French project), for which, while they are disappointing, maybe we can forgive Boss.

The structure of the book is interesting. Boss divided the material into a prologue, nine chapters, and an epilogue. The chapters detailed, almost in diary form, chronological developments in a particular area. The last chapter was unusual: it commenced by discussing recent events in the familiar format but then continued with future issues (*e.g.*, the launch of *Kepler*) but stayed in its diary format. The epilogue seemed almost an afterthought and discussed Boss's own recent research on the frequency of life in the Universe, the anthropic principle, *etc.* While obviously relevant to the book, it didn't really seem necessary.

Despite all the cowboys and indians and factual errors, I enjoyed this book. Maybe it's just the timing with the *Kepler* launch, but the world has changed in a quiet way and humans of all nationalities (including Americans) are getting close to finding out just how crowded our Universe really is. Boss is right to help us celebrate this. — DON POLLACCO.

Solar Sails: A Novel Approach to Interplanetary Travel, by G. Vulpetti, L. Johnson & G. L. Matloff (Springer, Heidelberg), 2008. Pp. 272, 24·5 × 16·5 cm. Price £15/$27·50/€19·95 (hardbound; ISBN 978 0 387 34404 1).

The three authors of this book are physicists deeply involved in the development and consideration of solar-sail technology, and they present a wide-ranging review of the subject and its current state of development as a working technology (currently, it seems, sadly undernourished; it is hoped this book may spark further interest in its growth).

Solar Sails is divided into four parts: an introduction to space propulsion, including the various approaches; a section on the concepts of solar sailing; and a section on the construction of sailcraft, all aimed at a lay readership. The fourth covers the basic mathematical and physical principles of solar sailing, and is pitched at a more technical level.

The text is lucid and clearly written, and the authors' command of their subject shines through. The discussion of terrestrial sailing in Chapter 2 is a good use of a familiar setting. The possibilities of solar sails are explored both in terms of timescale and of application, and the governing physics addressed. Essentials such as attitude control and deployment are covered in detail, and shown to be well within our reach. More speculative possibilities for enhancing sailcraft performance, such as lasers and particle beams, are also considered and judiciously assessed.

Diagrams are plentiful. For a lay readership, some explanations (*e.g.*, the basics of spherical geometry) could have benefitted from diagrams of their own. However, there is enough explanation to satisfy a technical or non-technical reader, and the segregation of the more thorough mathematical and physical treatment at the end, where it can be considered in detail, renders the rest of the book eminently approachable for a lay reader with a space-science interest.

For those willing to tackle the last section — which should present no problems to an undergraduate in a numerate discipline — the rewards are great. Terms are defined clearly at the beginning, and the book moves from consideration of the available radiation and the challenges of solar variability, through modelling of thrust on the sail, to the orbits, trajectories, and possible interplanetary transfers for targets including Mercury, Mars, and Halley's Comet. The progression is clear, the concepts well-explained, and the possibilities exciting.

There are some minor faults. The material in the first three sections could be better organized: topics are repeated (substantial parts of Chapter 2 are rehashed in Chapter 9) or discussed in separate small chunks (*e.g.*, the Planetary Society's *Cosmos 1*), though this is largely an editorial issue. The coverage can occasionally be somewhat erratic: the mathematics and physics are well-covered throughout, but the section on nanotechnology contains only a basic introduction to the subject, and its implications for solar sails are considered with extreme brevity. A brief treatment of the likely finances of solar-sail missions would also have been welcome. These, however, are minor gripes, and indicate more a piqued curiosity with solar sailing and its potential for opening up the Solar System than any fundamental problem with the book.

Solar Sails is a solid and broad introduction to the principles and inherent possibilities of solar sailing. The authors do an excellent job of explaining the principles, and devote a great deal of attention to making sure the reader understands their subject. It succeeds in leading the reader through the topic on both a conceptual and physical level, and its lucid exposition communicates the promise and advantages of a system with great potential for Solar System exploration. — ANSELM ASTON.

Around the World in 84 Days: The Authorized Biography of Skylab Astronaut Jerry Carr, by D. Shayler (Apogee, Burlington, Ontario), 2008. Pp. 272 + DVD, 25·5 × 17·5 cm. Price £16·95 (paperback; ISBN 978 1 894959 40 7).

Jerry Carr was one of the 'all-rookie' three-man crew which broke space-endurance records in 1973/4 by orbiting the Earth for 84 days in the space laboratory *Skylab*. How he got to that privileged position, the training it necessitated and the tribulations that accompanied it promise an engaging read. What distinguishes an "authorized" biography was unclear, since the subject of a biography is not necessarily living (or willing), but it seems to mean that the subject was agreeable to, and participated in, the venture through recorded interviews, meetings, diaries, and the like. Carr is quoted as saying that he had no desire to write the book himself, so Shayler has done both him and the reading public a service.

David Shayler, described as a "spaceflight historian", has an insatiable curiosity for facts and details about missions, equipment, and the people who operate them, and this book does more than shadow Carr through the pages of personal history; it is as much about the workings of *Skylab*, its payload, and its experiments. While Carr is central to the activities that are described, the principal chapter on the epic sojourn in space is so detailed that the other crew members get fairly full cover too. Around that centrepiece Shayler recounts Carr's early days, his introduction to and progress through the US Marines to a position at NASA, and afterwards the unwinding in various, mostly related, occupations. Accounts of his family of six are sometimes presented as excerpts of interviews. The book also includes a complementary DVD containing two documentaries and half a dozen narrations by Carr on specific training or support activities.

Now there are two ways of painting a tree: by copying it in every detail as in a photograph, or by reproducing its characteristic shape, the way it bends before the wind, almost its 'mood'. Similarly, a biography may be solely a factual account, or it may try to fathom what made the subject tick. There are clearly merits in recording historical facts for posterity, but by choosing that option here the author forewent what would have been the more interesting study: those very human questions as to why Carr followed the career paths that he did, what impelled, drove, fascinated, and repulsed him, how he balanced service and authority, and how he viewed the colleagues with whom he grew up, or trained, or (for 3 months) lived in space. To be fair, Shayler does include a postlude which is somewhat more analytical than what had gone before, and adds a touching appreciation of the man by one of the *Skylab* crew, but by that time it is rather too late. Had the book been Carr's autobiography he would very likely have offered reasons, impressions, and fears (however irrational), and would have cut back on the many awards, the schedules, the happenings, the family trees, the daily doings, and the minutiae which tend to overfill Shayler's version. I suspect we are the losers; the reported snippets from Carr are pithy, human, and often tinged with enough wit or sarcasm to make entertaining, as well as more thought-provoking, reading.

The style of writing is somewhat terse since it is largely reporting facts, and only occasionally shows that it can flow more prettily. The spelling is American, which is puzzling coming from a British author based in the UK and a Canadian publisher, and (a common complaint of mine) is overloaded with acronyms, some of which are nowhere spelled out. But it has to be said that Shayler is an exhaustive researcher, even if he does leave nothing out. His account of Carr's astronautical career is also a 'biography' of the *Skylab* module, and anyone with a passion for spaceflight will find plenty in this book to satisfy. — ELIZABETH GRIFFIN.

Quantum: Einstein, Bohr and the Great Debate about the Nature of Reality, by M. Kumar (Icon Books, Cambridge), 2008. Pp. 448, 24 × 16 cm. Price £20 (hardbound; ISBN 978 184831 029 2).

Isaac Asimov, in the preface to his *Biographical Encyclopedia of Science and Technology*, makes the interesting point that on average only one man in every six million has made any significant contribution to the advance of science. *Quantum* covers in some detail the lives and contributions of seven of those exceptional men, as well as touching more briefly on the lives and achievements of at least twenty others.

The thread of the narrative runs from Planck in 1900 introducing the concept of a quantum of energy, *via* Einstein, Bohr, de Broglie, Pauli, Heisenberg, and Schrödinger, to the presentation at the Volta Conference at Lake Como in 1927 of what later became known as the Copenhagen Interpretation of quantum mechanics. From then on, the book is concerned, as the subtitle suggests, with the subsequent continuing debate about the completeness of the Copenhagen Interpretation; and throughout the book the unhappiness of many of the quantum physicists at the difference between what they were discovering and the established principles of classical mechanics is an underlying *leitmotiv*.

Quantum is not a textbook of quantum theory. There is a lot of information about the developing ideas of the protagonists, but there is more about their lives and times, and I am left with the feeling that the author is more concerned to put across the human element in this amazing build-up of knowledge than the scientific discoveries. This is perhaps a praiseworthy objective, and I learnt much of interest about many of the personalities involved, but it brings with it a drawback. The rate at which the average reader can comprehend esoteric matters such as Planck's oscillators, wave/particle duality, matrix mechanics, and the like is significantly slower than the rate at which details of personal history can be absorbed. Biographical details surround and are mingled with scientific exposition in the author's thorough and otherwise forward-flowing account of the thought and research leading up to the Copenhagen Interpretation, and I found it frustrating to have to keep changing the pace of my reading to accommodate this.

Quantum theory is notoriously difficult to understand — Paul Dirac famously believed that it could not be described in words but only *via* mathematics — but the quantum information in this book is well expressed and, subject to the criticism above, not difficult to follow. The author only deals with details of the atom that are relevant to uncertainty and the reality debate — he does not, for example, mention Pauli's idea of the neutrino, notwithstanding the fact that Pauli is one of his principal characters — but his account of those details, and of the thought experiments put forward by Einstein to counter Bohr's belief that observation is essential to an electron's existence, is admirably lucid and interesting.

Because the argument about reality is still on-going, the book cannot close with the clear verdict as to who was right — Bohr or Einstein — which from a dramatic point of view the reader would like to find. In fact the book concludes by first describing Bohm's and Bell's work on hidden variables and Alain Aspect's subsequent experimental demonstration that, paradoxically, local reality is not real, all of which can be argued to support Bohr, but then goes on to cite the poll at the Cambridge quantum-physics conference in 1999 in which only four of the ninety participating physicists still accepted the Copenhagen Interpretation, thirty followed Everett's many-worlds concept, and fifty ticked the box labelled "none of the above or undecided". Common sense certainly supports Einstein's belief in a submicroscopic reality, and it is that, rather than his reluctance to accept the possibility of spooky action at a distance (which Aspect has now shown to exist), which was the cornerstone of his philosophy.

The book's broad picture of the early development of atomic theory and the lives and times of its creators encourages reflection, and it is strange that Planck and others were so taken aback by the discovery of quanta and discontinuity. The idea of discontinuity is inherent in the idea of the atom, and that dates back to Democritus and the 5th Century BC. The book mentions that Planck knew of but did not accept the theory of atoms, but does not tell us his reason for disbelief, and it is ironical that as an atomic iconoclast he was the first to hit upon the reality of the quantum. Schrödinger's entanglement concept, mentioned late in the book in connection with the thought experiment that ultimately led to Aspect's demolition of local reality, is another fascinating topic for reflection. If particles, once entangled, can thereafter react instantaneously and without any velocity restraint, it is perhaps not surprising that the Big Bang inflation episode also disregarded speed limits, coming as it did immediately after the total entanglement represented by the primordial singularity. A third and rather pleasing reflection is that both Einstein and Bohr are now commemorated by their own chemical elements — Einsteinium and Bohrium.

The last ninety pages of the book contain a timeline of events from 1858 (Planck's birth) to 2007, a glossary, notes of the many sources quoted, a bibliography, and an index, and there are 16 pages of pertinent illustrations, principally of the key personalities in early atomic research. A great deal of work has gone into its writing, and much information about atomic theory can be acquired from it; but its principal merit lies in the overview which it presents of the circumstances in which quantum mechanics, now so important to all of us in terms of lasers, computers, and so on, first came to be developed, and of the pantheon of great physicists whose exceptional insight, tenacity, and mathematical flair created it. — COLIN COOKE.

Modern Quantum Field Theory: A Concise Introduction, by T. Banks (Cambridge University Press), 2008. Pp. 271, 25·5 × 19 cm. Price £35/$65 (hardbound; ISBN 978 0 521 85082 7).

Modern Quantum Field Theory: A Concise Introduction is a very ambitious and in many respects successful book. Professor Banks has played a leading rôle in developing many of the advances in the more theoretical branches of particle physics over the past two decades, and his *Introduction* takes the reader rather a long way into the subject. The author describes his style as "terse rather than discursive" in the introduction to the book, and that is certainly the case. The book covers as much ground in little more than 250 pages as many textbooks in the same subject manage in 500, or even over a number of volumes. In order to facilitate this there are a large number of problems associated with each chapter, and for someone learning the subject it is necessary to attempt some of these in order to progress through the book continuously. They are certainly not optional extras, as can be the case in many textbooks. This format means that anyone attempting to learn quantum field theory from scratch will have to work through the book slowly and very thoroughly, but they will be guaranteed a very detailed understanding if they do so.

For purposes of teaching, the book is best suited to a graduate-level course, and ideally the student should have some knowledge of quantum field theory before starting, as the basics are covered very quickly. It is likely to be best suited to those with a more complete mathematical background. Browsing of the book is most apt for those who already have a reasonable background in quantum field theory, and they will find much that is insightful and illuminating. Particularly appreciated was the modern treatment of the theory of renormalization, presenting the subject in a far more physical manner than many textbooks, which often leave the reader thinking it is nothing more than a trick to remove divergences. Here the understanding of renormalization in terms of physics at different energy or length scales is presented explicitly from the beginning, providing the reader with the explanation of the techniques in terms of the underlying physics from the outset.

To summarize, *Modern Quantum Field Theory: A Concise Introduction* is not the most user-friendly textbook on quantum field theory, and requires effort. However, the reader who takes time to make a full study of it will end up better and more deeply informed than they will from a study of most other introductory texts on the subject. — ROBERT THORNE.

Panoramic Views of Galaxy Formation and Evolution (ASP Conference Series Vol. 399), edited by T. Kodama, T. Yamada & K. Aoki (Astronomical Society of the Pacific, San Francisco), 2008. Pp. 513, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 668 4).

Like the 'low' in 'low redshift', 'panoramic' is clearly one of those words whose meaning, to observational astronomers, evolves with time. While some of the contributions to this conference present work from surveys of large areas of sky, others are based on surveys of size much less than a square degree. However, as many of these use *Suprime-Cam* images and this was the first *Subaru* International Conference, we probably shouldn't quibble!

The book is one of the ever-expanding *Astronomical Society of the Pacific Conference Series* — Volume 399, in fact, the 19th of the year. The organizers and editors clearly did a fine job of gathering the contributions for the printed volume, as the conference was held in 2007 December and the proceedings have a publication date of 2008. The conference itself was largely a Japanese/US affair, with over 70% of participants from one of those two countries. One hazard of the rapid publication (or perhaps an excess of honesty!) is that the printed papers don't always coincide with the meeting programme included in the book. Some talks (if they were actually given) do not appear in the proceedings, while other authors' changes of mind about what to talk about are also evident.

The topics considered are largely as expected for an up-to-date look at research in galaxy formation and evolution, with the emphasis on the observational side — surveys for Lyman-break galaxies and Lyman-α emitters and the cosmic star-formation rate, lensing and dark-matter maps (which according to Richard Ellis in the 'Conference summary' were ranked one of the top-ten science stories of 2007, along with the discovery of proteins in Tyrannosaurus Rex!), mass assembly, cosmic down-sizing, galaxy clusters, co-evolution of galaxies and black holes, *etc*. With numerous good reviews and pointers to most of the work in the field, all in all this presents a valuable resource for this area and it is well worth getting your library to buy one. — STEVE PHILLIPPS.

Tools of Radio Astronomy, 5th Edition, by T. L. Wilson, K. Rohlfs, & S. Hüttemeister (Springer, Heidelberg), 2009. Pp. 515, 24·5 × 15·5 cm. Price £72/$109/€79·95 (hardbound; ISBN 978 3 540 85121 9).

Here is an old friend, said (admittedly by the publisher) to be the most-used introductory text in radio astronomy. A very significant change from earlier versions is the addition of problem sets at the ends of each of the 16 chapters, anything from five to 22 per chapter (total 207, more than enough for the instructor to be able to recommend only the ones she knows how to do, though there must surely be a solution set lurking somewhere). Other significant changes include (*a*) addition of a third author, (*b*) a good deal of restructuring to distinguish basic theory (which changes only slowly) from practical aspects of instrumentation and results (which change more rapidly), (*c*) the discussion of instrumentation and data collection and processing which looks forward to *ALMA* and the *SKA*, making the volume useful, the authors hope, for radio engineers (a grand old name, which they apply to Jansky and Reber) as well as radio astronomers, and (*d*) updating of the references, though with no attempt at completeness and a continuing focus on paper sources owing to the frequent ephemerality of web addresses.

There is a solid discussion of Bremsstrahlung and synchrotron emission processes, though inverse Compton appears only as a loss mechanism and in connection with the Sunyaev–Zeldovich effect. Other items you might look for and not find include solar radio bursts and metre-wave techniques and results.

I have a mild complaint about the index: a few days after my copy arrived, I really wanted a table of known interstellar molecules. Actually there is one (on p. 450 in case you need it), but none of the 36 index entries under 'molecules' or 'molecular' takes you there, nor is there a list of tables anywhere. The total is 155 molecules, excluding isotopomers, and I wish some of the names had been given. All right, CH_4 is methanol; C_4H may not deserve a name, let alone C_8H; but what is NH_2CH_2CN, not to be confused with CH_3CH_2OH, which is ethyl alcohol?

Our astronomy/cosmology programme does not currently teach radio astronomy, but if yours does, I'm sure you will find this iteration at least as useful as the previous four. — VIRGINIA TRIMBLE.

Ultraviolet and X-ray Spectroscopy of the Solar Atmosphere, by K. J. H. Phillips, U. Feldman & E. Landi (Cambridge University Press), 2008. Pp. 349, 25·5 × 18 cm. Price £85/$170 (hardbound; ISBN 978 0 521 84160 3).

The physics of complex ions that underlies solar ultraviolet (UV) and X-ray spectroscopy is not usually included in undergraduate courses unless there are local experts. Thus there is a need for a textbook for use by graduate students and others new to the field. I read this book with high expectations, but found that it is, like the proverbial curate's egg, excellent in parts.

The best parts are those that are most closely related to the main aim of the book — to explain how UV and X-ray spectra can be used to determine physical conditions in the solar atmosphere. In these I include the chapters on the basic theory required (2, 3, and 4) and that on plasma diagnostic techniques (5). The former give a full account of the topics needed beyond the level of a typical undergraduate course, with sufficient links to material that should be familiar. The latter gives a comprehensive account of diagnostic techniques used in solar physics, with useful discussions of their relative merits. Postgraduate students should find these chapters helpful. There are a few lapses from the overall high standard, *e.g.*, the plasma is not technically in 'collisional ionization equilibrium', since collisional ionization is not balanced by its inverse process (collisional, 3-body recombination). In (electric dipole) intercombination transitions, the breakdown of LS-coupling is due to spin-orbit interactions, not to mixing between different electron configurations.

There is a description of the solar spectrum (Chapter 6), which is good, so far as what *is* included is concerned. However, no criteria are given for the selection of the sources of the line data given in Appendix 2, which are generally not those for the original line identifications. There is some bias: the list of coronal lines observed above the limb includes only lines observable with the *SUMER* (*Solar Ultraviolet Measurements of Emitted Radiation*) instrument on *SOHO* (*Solar and Heliospheric Observatory*), thus excluding important lines above 1610 Ångstroms. Similarly, not all density-sensitive lines are included in the tables within the chapter; this reduces the value of the book for stellar applications.

Chapter 7 gives a valuable account of instrumentation used since 1973, but one statement in the concluding summary cannot pass without comment. This says that "The earliest observations gave us only crude information about the spectra ... ." Yet it was from those early spectra that the majority of the most important emission lines, on the disc and at, and above, the limb, were identified and from which diagnostics for the electron density and temperature were developed. Regarding accuracy, the first flight of the Naval Research

Laboratory's *High Resolution Telescope and Spectrograph* (*HRTS*) (a normal-incidence, stigmatic spectrograph) took place on 1975 July 21 (not, as in Table 7·2, on 1979 March 1), and led to the first observation of molecular hydrogen in the solar atmosphere and the discovery of molecular fluorescence by strong transition-region lines.

The next set of chapters (8, 9, and 10) give accounts of the phenomena observed in the quiet and active solar atmosphere, but the interpretation could be stronger and the links to the underlying physics are weak. Also, some material is biassed to the authors' views and some matters described as recent discoveries were understood twenty to thirty years ago — for example, that the transition region occurs at different heights around transient features such as spicules, rather than in a single static layer. Similarly, it is not 'surprising' that the range of the solar coronal temperature is small. Stellar studies have shown that the surface magnetic flux and coronal parameters depend on empirical, convection-zone, dynamo numbers. Early solar observations established how the coronal temperature varies over the sunspot cycle.

The introductory chapter contains too many inconsistencies and inaccuracies to list here. Graduate students would be better served by reading Athay's early classic (*The Solar Chromosphere and Corona: Quiet Sun*, Reidel, 1976), or the books by Foukal and Mariska, listed under 'Further reading'. As examples of problems, Table 1·7 combines three models of the atmosphere, with disregard for hydrostatic equilibrium, and the gas pressures given at and above 126 000 K do not agree with those from equation 1·2. Errors include the statement that the interiors of M *and K* stars are fully convective.

The last chapter on element abundances gives a good description of the results of direct measurements in the solar wind. These indicate that elements with low first-ionization potentials (FIP) are overabundant, with respect to the photosphere, by around a factor of 4, except in the fast wind. However, the results for other regions are presented without sufficient detail concerning the methods used and atomic data adopted. No physical reasons are given as to why the quiet transition region has a FIP effect of only around a factor of 1·5, far smaller than in the wind. Similarly, a claim that line intensities, observed at heights up to 0·45 solar radii above the limb, show evidence of gravitational settling is not supported by any discussion of the physics, or of the various possible line-excitation processes (clearly described in Chapter 4). That said, the well-established observations of changes in the ratios of Ne VI and Mg VI line intensities in active-region loops, as they evolve and expand, do deserve further modelling, including density-dependent treatments of the line formation and ion populations.

An overall weakness of the book is that there is no discussion of how line optical depths that are greater than around unity can affect the fraction of the photons created in the transition that escape from the atmosphere in a particular line of sight (which also depends on the actual geometry). In addition to lines formed in the chromosphere and low transition region, strong coronal resonance lines can also become optically thick at the limb. Finally, it is a pity that the three authors could not agree on the definition of the transition region, as given in the useful glossary. Throughout the book, the resonance lines of He I and He II are incorrectly described as chromospheric lines; in Chapter 6, even the lines of C IV, formed in the mid-transition region around 100 000 K, are incorrectly described as being formed in the 'upper chromosphere'.

In conclusion, I recommend the material on atomic physics and diagnostic methods, and the descriptions of the solar spectrum and recent instrumentation, but overall there are too many flaws for this to become a standard graduate textbook. — CAROLE JORDAN.

Astronomical Optics and Elasticity Theory, by Gérard René Lemaître (Springer, Heidelberg), 2009. Pp. 575, 23·5 × 15·5 cm. Price £72/$109/€79·95 (hardbound; ISBN 978 3 540 68904 1).

This is an utterly fascinating book, to which my initial reaction was, "What do the two halves of the title have to do with each other?" The answer is that the author is interested in active optics, especially production of non-spherical mirrors under stress, an idea that goes back to Bernhard Schmidt. I do not know the answer to your next question, whether he is related to Georges Lemaître (but am willing to guess he is not a lineal descendant). There is some real history at the beginning (including Persian mathematicians) and a good deal of realistic future at the end, addressing how to change the shapes of surfaces in a desirable fashion with applied forces and how to inhibit changes in shape of other surfaces due to unavoidable forces. There is a section on the Chinese Schmidt, *LAMOST*, which has a segmented bisymmetric elliptical primary, in whose design the author was involved. The list of acronyms and the glossary are both extensive and useful for the non-expert reader. This one had not quite grasped that the existence of the word 'astigmatism' must imply stigmatism (and, of course, anastigmatism). And it is satisfying to learn that Roderick Willstrop and Donald Lynden-Bell have shown that all two-mirror telescopes of practical interest have already been discovered (leaving out one due to a colleague in another discipline that required the observer to get his head into the tube between two mirrors). I do not mean to imply that the book is easy reading (nor was it meant to be) or that I expect you to dash out and acquire your own copy, unless you are seriously involved in telescope design. But just about every page has an image or an equation or a definition that invites thought. — VIRGINIA TRIMBLE.

Introduction to Astronomy and Cosmology, by I. Morison (Wiley, Chichester), 2008. Pp. 341, 24·5 × 17 cm. Price £32·50 (paperback; ISBN 978 0 470 03334 0).

This is an up-to-date, well-written, first-year-undergraduate textbook. The field covered is extremely wide, so some lack of depth is unavoidable.

It begins by explaining some definitions, coordinate systems, and time. The reader is then taken through the evidence for the Copernican system and the deduction of the laws of planetary motion. Newtonian gravity immediately follows, and then the effect of General Relativity.

Our Sun is the best-observed star, and energy generation, the solar atmosphere, and solar–terrestrial phenomena are clearly explained. The planets are treated, first as a general group with a comparison of their masses, radii, and atmospheres. This is followed by more detailed accounts of the separate planets, their rings and satellites, and results from recent space probes. The search for exoplanets is now a well-established field of research, allowing a comparison between our own Solar System and others.

The tools needed to observe the Universe are particularly well covered, including physical optics and the human eye. The designs of both optical and radio telescopes are fully reviewed. Some representative large telescopes are described together with the improvement in image sharpness provided by adaptive optics. All regions of the electromagnetic spectrum are included together with cosmic rays and gravitational waves.

To study a star, the astronomer needs to know its distance, luminosity, mass, temperature, and radius. The methods of measuring those quantities are described, leading first to the Hertzsprung–Russell diagram and then on to stellar evolution. Supernovae are a particularly interesting stage of stellar evolution because they lead to the production of heavy elements and neutron stars or black holes.

The Milky Way, together with its constituent globular and open clusters, has been intensively studied for many years but the most interesting current topic is the mounting evidence for a black hole at its centre. The cosmic distance scale must be established in order to study external galaxies. The Cepheid distance scale extends to the Virgo cluster, and further distances, critical to cosmology, can be measured from supernovae of type Ia. The evidence that decides between the Steady State and Big Bang theories of the Universe is reviewed, together with the evidence for the existence of dark matter, dark energy, and intelligent life.

This book is illustrated throughout with clear, informative diagrams. It can be recommended, not only to first-year university students but also to sixth-formers preparing for university entrance. It would also be a good text for an adult course. Many people will enjoy reading it, and may even attempt the questions at the end of each chapter. — DEREK JONES.

The Cambridge Double Star Atlas, edited by J. Mullaney & W. Tirion (Cambridge University Press), 2009. Pp. 148, 30 × 24 cm. Price £27·50/$35 (paperback; ISBN 978 0 512 49343 7).

With the 18th edition of *Norton's Star Atlas*, the editor and publishers made the mysterious decision to stop labelling the double stars on the maps. With this new competitor, that misjudgement has been sidestepped, and for the growing band of observers who are looking at double stars it will be a welcome arrival.

James Mullaney is a well-known observer and writer on deep-sky matters and this book follows closely on the heels of his previous volume, *Double and Multiple Stars and How to Observe Them*. In collaboration with the well-known uranographer Wil Tirion, he has produced a splendid tome for the double-star *aficionado*, and indeed, the observer in general, since the usual galaxies, nebulae, clusters, and variable stars are also plotted. As might be expected, the maps are clear and unambiguous and drawn with a generous scale on good-quality paper, there being 60 A4 pages covering the whole sky. The magnitude limit is 7·5 but a number of fainter double stars of exceptional interest, such as Krüger 60, are included. The whole is presented in a ring binding which is also fitted with a spine so that the pages do not quite fold back upon themselves.

There are several accompanying tables or appendices, the first of which is the author's selection of 133 of the finest double and multiple stars for telescopes of 2 to 14 inches in aperture, but I wonder if they should have been ordered in ascending RA rather than alphabetically in constellation. However, there can be no argument about the contents of this list, which will be of great use to the public-observatory demonstrator looking for celestial gems to show his or her visitors. The main table is the 2400-entry double-star target list, which lists positions, magnitudes, and relative coordinates for the vast majority, but not all, of the pairs and multiples plotted on the charts. For each pair the latest separation (presumably) is given from the *Washington Double Star* (*WDS*) catalogue but no position angle or epoch is quoted, even though there seems to be enough space there to include both. The author notes that a few pairs have no catalogue numbers, such as α Sco, α Cen, and ψ Vel, but, in fact, the current *WDS* applies numbers to all systems so that these three binaries are, respectively, GNT 1, RHD 1, and COP 1.

For the double-star enthusiast this is a must-have. — R. W. ARGYLE.

Cataclysmic Cosmic Events and How to Observe Them, by Martin Mobberley (Springer, Heidelberg), 2008. Pp. 256, 23·5 × 17·5 cm. Price £19/$34·95/€24·95 (paperback; ISBN 978 0 387 79945 2).

This book in the Springer *How to Observe Them* series follows the usual format with a short theoretical section on the object types covered and then detailed vignettes on interesting members of the class. In this particular case I think the title is a bit of a misnomer as it covers primarily cataclysmic variable stars with chapters on dwarf novae, recurrent novae, novae, supernovae, and only short chapters on AGN and gamma-ray bursters, which is what I thought the book was going to be about. A better title might have been 'Cataclysmic variable stars and how to observe them'. The book also includes a chapter on flare stars. It therefore seems a bit of a mishmash and, with the same author's book — in the same series — on supernovae, material is necessarily duplicated, particularly, of course, in the section on supernovae. There is also a chapter on variable-star-observing techniques using both visual and CCD methods, which is reasonably comprehensive. The book gives me the feeling of throwing together loosely-related subjects, and although I cannot find fault with the material *per se* I feel that perhaps had the author concentrated on a tighter range of subjects and dealt with them in a little more detail then perhaps this would have been a better book; an example here is having a section on observing solar prominences, which perhaps should be covered in a book on observing the Sun — it feels to me like padding.

The resources, which I assume are thumbnail images from the DSS, are, I feel, reproduced far too small to be of any real use to act as finder charts, which is what I suspect they are there for. I am also not sure why Martin references the *Caldwell Catalogue* as one used by supernova patrollers as I suspect anyone who reported a supernova in a galaxy and referenced it by its Caldwell number would be laughed out of the Central Bureau for Astronomical Telegrams. There are places for such lists but this is not one of them. I think if you have a primary interest in variable stars you might find something in this book, but personally I felt it was a disappointment. Martin has written some good books but this is not one of his better ones. — OWEN BRAZELL.

Building a Roll-Off Roof Observatory, by J. Hicks (Springer, Heidelberg), 2008. Pp. 160 + CD ROM, 25·5 × 17·5 cm. Price £24·50/$59·95/€46·95 (paperback; ISBN 978 0 387 76603 4).

The author of this book has been selling plans and advising people on how to build roll-off-roof observatories for many years. He compares the advantages and disadvantages of dome-type observatories and roll-off-roof designs. He then proceeds to give detailed advice as to how to deal with everything from planning applications to the construction itself. Every aspect of the construction is dealt with and naturally the book concentrates on the author's own designs. There is little that could be faulted with either the design or the advice, but if one is to criticize then it would be that there is a major concentration on what the author and his customers have used and no serious discussion of alternatives. There are many photographs of other people's designs but alternative construction techniques and materials are rarely mentioned. Examples would be that neither Scandinavian log cabins as starter units or the use of metal-clad, foam-cored composite panels, as used in many industrial estates, are mentioned. The former can be very acceptable in areas where the neighbours might complain, while the latter is so robust and thermally insulated that there is a reduced need for structural framing. The only major area where this reviewer would fault the advice given is in the area of mechanisms for opening and closing the roof. The author recommends wire ropes and electric winches while this reviewer has found that half-tonne-weight roofs can be opened and closed with one hand given pre-stretched polyester ropes and correct design. The book comes with a CD on which all the drawings in the book are presented in an accessible format for printing or inspecting in greater detail.

The author has given extra details about suppliers and/or specification and is meticulous in giving credit to other sources of information that he has incorporated into the book. This is necessary as, although the front cover of this series of books carries the name of Patrick Moore, about as English a person as one could find, the use of Canadian and North American terminology permeates the book and readers in Europe will be left wondering on occasions as to what is being used.

There is a more serious problem with this book which is not the fault of the author but of his editors. In a previous review of another book in this series (*My Heavens* by Gordon Rogers; see **128**, 516) I criticized the fact that all the photographs had been reproduced in monochrome despite the fact that the author of that book is known for the excellence of his colour astrophotography. In this book there are sixty-nine photographs in monochrome of which twenty-four are reproduced again in colour. There seems no good reason to produce the same photographs at the same size and with the same captions twice. Additionally, ten of the photographs have caption frames the same size as the photographs, with the text generally only taking up three or so lines. One is left with the impression that someone was trying to fill up pages in the book. There is some careless confusion on pages 100 and 101 between the symbols for feet (′) and inches (″) while bizarrely on page 132 two English words are produced in Greek text. Table (b) at the bottom of page 123 seems to confuse the dimensions of sawn and planed timber.

Would I recommend the book? Yes. Should you seek more information before starting? Yes. — NORMAN WALKER.

Here and There

MORE OF A BLUE RINSE THAN DUST

The bright naked eye cluster of blue stars [the Pleiades] get their colour from an interstellar cloud that passes between us and them. — *The Daily Telegraph*, October Night Sky.

A LUNAR LAPSE

… Venus, the brightest object in the solar system barring the Sun … — *The Daily Telegraph*, October Night Sky.