THE OBSERVATORY

Vol. 129 | 2009 OCTOBER | No. 1212

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

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

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

The President. It's the second Friday the 13th in a row, so it's time for our meeting. Our first talk is by Dr. Stuart Clark, from the University of Hertfordshire, speaking to us on 'Richard Carrington: The Sun King.'

Dr. S. Clark. At 33, Richard Christopher Carrington was an accomplished astronomer. Educated at Trinity College, Cambridge, he had compiled a star catalogue and worked tirelessly for the Royal Astronomical Society. He hoped fate would provide a unique scientific discovery and, on the morning of Thursday, 1859 September 1, fortune favoured him with such a prize.

Working in his grandly appointed private observatory at Redhill, Surrey, he cranked-up the dome's shutter and prepared the beautiful two-metre-long brass telescope. He was six years into a long-term study of the Sun and its transitory sunspots. Manoeuvring a distempered board into position, he aligned the telescope so that it threw the Sun's image onto the screen. Poking the front-end of the telescope through a made-to-measure hole, he slotted a larger board into position around the telescope. This cast a shadow across the board, allowing him to see the Sun's 11-inch-wide image more clearly. Two gold wires, beaten into slivers and strung inside the telescope's eyepiece cast a diagonal crosswire on the image. Using them as guides, Carrington sketched the entire face of the Sun to produce a lasting detailed document.

Today was special because an enormous sunspot complex was visible. Opinions varied: openings in the Sun's clouds, revealing the surface, or mountaintops. From tip to tip today's was almost ten times the diameter of the Earth, yet it barely stretched a tenth of the way across the Sun. By 1118 GMT he had finished drawing and was listening to the chronometer's tick, recording the precise moments at which the various sunspots slipped beneath the crosswire, using them later to calculate the sunspots' exact positions.

Without warning, two beads of searing white light, bright as forked lightning but rounded and persistent, appeared over the monstrous group. Carrington assumed a ray of sunlight had found its way through the shadow-screen and jiggled the telescope, expecting the errant ray to zip wildly. Instead, it stayed fixed in its position; it was coming from the Sun itself. As he stared, the two spots of light intensified and became kidney-shaped. Carrington noted he became rather "flurried by the surprise" of being "an unprepared witness" to the event. Nevertheless, his scientific training made him note the time. Realizing the rarity of the situation — no one had described such an event — he hastened to find a witness.

Returning not 60 seconds later, his excitement faded: the lights were greatly enfeebled. He watched them drift across the giant spot, contract into mere points, and vanish. Noting the time again, 1123 GMT, he sketched the position of the lights' appearance and disappearance. He rooted himself to the telescope but his vigil was in vain; the Sun had returned to normal. He could see no indication that the strange phenomenon had ever happened. Carrington set to work on the mathematics. The lights had lasted five minutes, yet had traversed 35000 miles (nearly four-and-a-half times the diameter of the Earth). The disturbance must have moved at around 420 000 miles per hour. Judging by his sketch, the original fireballs had each been the size of the Earth.

The surviving accounts are unclear as to whether Carrington found a witness in his household but such a momentous observation demanded a wholly independent scientific corroboration. He went to Kew observatory, where Warren de la Rue was engaged in photographing the Sun every day. The bad news was that no one at Kew had seen the solar flare or even photographed the Sun. However, the magnetic instruments had captured the disturbance. The Earth's magnetic field had recoiled at exactly the time Carrington had seen the flare. The abrupt disturbance had lasted just three minutes but had taken the next seven to die back down to normal. It seemed the flare had somehow reached out across 93 million miles of void. Eighteen hours after the initial disturbance the Earth's atmosphere erupted with aurorae and the Kew needles again started moving, surpassing the strength of the initial recoil. Earth suffered a sustained assault unequalled in the decades that Kew had been collecting data.

As darkness fell on the evening of September 2, the aurorae were still raging. One of the lowest-latitude observations came from La Union, San Salvador, just 13 degrees 18 minutes north of the equator. As reports filtered around the globe, it became obvious that something extraordinary had happened to Earth. The aurorae had possessed a sinister side, too, disabling the telegraph system, the equivalent of today's Internet. Scientists had to solve the mystery of what caused the aurorae.

Carrington did have one asset: his reputation, his meticulous attention to detail. Then another astronomer turned up who had been observing the Sun on 1859 September 1: R. Hodgson, Esq., of Highgate, a Fellow of the RAS and a respected solar observer who had invented a special eyepiece with which to observe the Sun's fearsome light safely. Carrington insisted that they exchange no further information but would both present their accounts.

On 1859 November 11, the Fellows of the RAS gathered in anticipation at Somerset House, London. Carrington showed an enlarged copy of the drawing he had made. Hodgson described how the dazzling light illuminated the edges of the adjacent sunspot; his timings matched Carrington's and he had executed a sketch. No Fellow could be in serious doubt that something unprecedented had taken place on the Sun or, more likely, just above it. Carrington convincingly argued that, since the Sun's surface had displayed no difference before and after the event, the flare must have taken place high above the sunspot group. As for the putative link between the flare and the aurorae, there was considerable debate. Both men mentioned these two features; Carrington even showed photographs of the Kew charts, pointing out the magnetic jolt at the time of his flare and then drawing attention to the subsequent and powerful magnetic storm that coincided with the aurorae. A paragon of scientific scepticism, Carrington cautioned his audience that, whilst the contemporary occurrence deserved further consideration, "One swallow does not make a summer."

The sudden demonstration of the Sun's ability to disrupt life on Earth catapulted astronomers into a headlong race to understand the nature of the Sun. Gradually the emphasis of astronomy changed to understanding the physical nature of the stars and traditional charting astronomy began its transformation into present-day astrophysics.

The President. Thank you, Stuart, for a very entertaining talk. Any questions?

Mr. P. Sutherland. An event like this is bound to happen again, a storm of this magnitude. How can we prepare for it? The effects today would be incredible, wouldn't they, affecting satellites, *etc*.?

Dr. Clark. There is work now going on to try and answer exactly that question. If Mike Hapgood is still here, he is the best person to ask. *He* knows how to save the planet. [Laughter.]

The President. Any other questions?

Rev. G. Barber. Presumably this was a CME that happened to hit the Earth, whilst most of them miss — it wasn't a spectacularly large event, it was just that it happened to be pointing towards us?

Dr. Clark. Well, both, actually. I think it was spectacularly large and it was also pointing directly towards us. There were magnetometers all around the world by this time, 1859, and all of them, except one in India, went off their scales. So it was only fairly recently, when these ledger books were finally unearthed in India, that we've been able to put the top level on how big this event really was, and it was enormous.

Rev. Barber. So how frequent are CMEs of this magnitude?

Dr. Clark. You can get some sort of idea from the ice-core records, because the events create nitrates in the atmosphere. And I think you have to go back to the 16th Century before you get something as big.

Rev. Barber. So every 300 years then?

Dr. Clark. Something like that, yes.

Mr. M. F. Osmaston. Have you been able to dig at all into the pre-event observations of any kind? Or were there none, with hardly anybody bothering to look? Or can you get any precursor because that might relate to the previous question of how can we prepare for it, so there might be a little bit of warning?

Dr. Clark. Essentially, I think the best warning you get at the moment is when you see on the limb of the Sun a gigantic sunspot group coming round. And I think you can now see them from the quakes they cause around the back of the Sun.

Mr. Osmaston. That's with today's data, but is there anything in the 1859 story?

Dr. Clark. No, I don't really think so. There was another large flare or another large storm on August 28 as well — they came in a pair and that did cause some disruption too.

Mr. I. Ridpath. Were there any obvious extreme weather events associated with the flare?

Dr. Clark. Lumis devotes something like six or so pages in one of his papers about extreme thunderstorms during the period. When the papers were re-edited and republished quite recently those pages were edited out, for some unknown reason.

Mr. C. J. North. I seem to remember that there was an effect of a solar flare on the power supply in North America, which caused considerable disruption. How would that compare with the Carrington event?

Dr. Clark. My understanding is that that was somewhat smaller than the Carrington event and — I'm thanking Mike Hapgood for the information — it's actually the 20th anniversary of that particular flare today. So, yes it is one of the big ones, but I think it is still somewhat smaller than Carrington's.

The President. Let's thank Stuart again. [Applause.] The next talk is by David Smith from the US National Research Council, who is talking to us about 'The Planetary Science Decadal Survey.'

Dr. D. H. Smith. [The speaker said that he worked for the National Research Council, a part of the National Academy of Sciences (NAS), and he would describe a programme commissioned by NASA and the National Science Foundation to conduct a decadal survey for planetary sciences. The NAS was formed as a private foundation in 1863 when a group of distinguished scientists approached the government suggesting the formation of a national academy of science. The NAS was mandated, amongst other things, to "investigate, examine, experiment and report on any subject of science or art when asked to do so by any department of the federal government" and also "at no charge" [laughter]. Towards the end of the 19th Century, investigations such as an assessment of the metric system were undertaken, but they still haven't come to a conclusion on this [laughter].

In 1916 George Ellery Hale devised an early form of outsourcing when he set up the National Research Council to oversee the work done by the scientific community. Today the NRC has 1400 staff, a budget of 400 million dollars, and there are 500–800 projects in train. In the late 1950s, several agencies approached the Academy for scientific advice on what to do in space. On 1958 June 26, the Space Science Board was established by the Academy, later merging with the Space Applications Board to form the Space Studies Board.

One of the most important aspects of the Academy's work is the astronomy decadal surveys. These are now organized jointly by the Board of Physics and Astronomy and the Space Studies Board. The decadal surveys define the most important scientific questions to be addressed by the scientific community over a decade, and then specify and prioritize the missions that can address those questions. In 2000/2001 NASA commissioned a further two decadal surveys: one for planetary science and the other for solar and space physics. More recently there has been a decadal survey on Earth remote-sensing from space in 2007 and currently one on 'Life and microgravity sciences' is being organized.

The astronomers started their latest review last December, led by Roger Blandford at Stanford, and they are hoping to report by about mid-2010. Last year NASA came back to ask for another planetary-science decadal survey after the success of the previous one. New discoveries, new budget pressures, and new corporate activities between Europe and the US means that a fresh start is required in this area. Whilst the NASA annual budget for planetary exploration is about 1 billion dollars, that of the NSF is only 15–20 million dollars. An important task of the new survey is to decide how to divide up funding fairly between the various planets and between large, medium, and small missions. The new planetary decadal survey will consist of a steering committee and five topical panels, the subjects of which are Mars; the Moon, Venus, and Mercury; giant planets and exoplanets; satellites of giant planets; and finally comets, asteroids, Kuiper Belt objects, and Pluto. A chairman is expected to be announced soon and the panels will convene in the second half of this year with reports being submitted to the steering group a year later, followed by the final report in 2011.

The speaker concluded by saying that he was travelling around to talk to the community and is asking for input to the survey. Participation in the panels from the UK would be welcome and there is an application form on the website (http://www.nas.edu/ssb).]

The President. These decadal-survey reports are always very important and useful for the whole community worldwide.

Professor F. W. Taylor. David, I think the most important object in the Solar System that we should be studying, apart from the Earth and the Sun, is Venus. And I've always been rather disappointed that in the last 20 years NASA has put relatively little of its effort into Venus, compared to Mars, for example, and the outer Solar System. And I wonder why. The point I really wanted to make was that one of the answers to "why" might be in your slide, where you showed how the study was being divided up. And you hive Venus off with the Moon and Mercury on one side, Mars gets a whole box of its own with a whole team, and the outer Solar System gets two boxes, with the planets and the satellites with a team each, which of course you can satisfy with a single mission. So haven't you got a playing field that is almost vertical before you've even started this study?

Dr. Smith. No, I disagree with that. We gave the organization of the survey a great deal of thought: our committee on planetary and lunar exploration spent two and a half days last August arguing about whether you organize along scientific lines — instead of objects, you focus on planetary atmospheres, planetary interiors, *etc*. — or whether you organize around major scientific questions, rather than objects or disciplines. And they argued round and round in circles for two and a half days and basically ended up back here. When we identified our chair, we basically had the whole process all over again. He argued that this didn't make any sense, and argued himself right around in a circle, back to this. And the reason I think this makes sense is that we have to come up with a list of high-priority missions, and the only clean way to do that is to organize the study along the lines of destinations. You can't send a mission to Venus and Pluto. You can send one to Jupiter and Saturn, perhaps, or Jupiter and the Kuiper Belt objects, but there are limits.

The groupings also reflect, to some extent, the relative sizes of the research communities: there is a huge Mars community, there is a much smaller primitive-bodies community. I spoke to the Venus community two weeks ago in Houston, and there are twice as many people in this room today as there were at that meeting. So, yes, Venus may seem to be losing out because it is lumped in with the Moon and Mercury, but in some sense that reflects the relative sizes of the communities.

The other reason is that we want to have a clean interface with the assessment groups: these self-generated community groups, like the Mars Exploration Program Analysis Group (MEPAG) and the Outer Planets Assessment Group (OPAG), have a clean interface to this structure. For example, MEPAG can look at the Mars panel as its own, and OPAG and the Small Bodies Assessment Group can do likewise with other parts of the survey.

One final thing about Venus missions: I mentioned the issue of balance across the Solar System — the Mars community wants a Mars sample-return mission, a very, very expensive mission; the outer-planets community wants a flagship mission to Europa and Ganymede, maybe one to Titan in the future. The Venus community are catching up, and they have quite a detailed plan for a Venus flagship mission, but the big drawback is that the technology is still not as mature as it would be to justify spending many billions of dollars on a mission to the surface of Venus.

Mr. Osmaston. I would like to make a point that has been totally ignored, that the dynamics of the Solar System as it is today tell you an immense amount about how it was made. For example, the fact that all the planets have prograde orbits, and all the satellites are prograde, except one, tells you that actually they were all acquired while the central body had a tidal propensity so that the retrograde ones got wound in as accretion. And this probably tells you that the majority of the accretion of the central body was by tidal capture rather than by impact.

Dr. Smith. The formation of the Solar System isn't a major aspect of our survey, but I believe the astronomers are looking at that in their survey.

Mr. Sutherland. I know you need to excite the public, and when I write about space exploration I'm sick of getting the response that we are throwing our money away into space. When you do your outreach could you please stress and stress again that all this money is spent on Earth, on real jobs for people, and not landing on Mars. [Laughter.]

Dr. Smith. You're preaching to the converted.

Mr. Sutherland. I know, but I just hope this message is put out more widely, because a lot of people don't understand it.

Dr. Smith. One interesting thing — during the previous Decadal Survey, we put together a popular version of it. While we were waiting for the report to go through the review process and the editorial process, we had time on our hands; and so a colleague, an undergraduate student, and I compiled a popular version of the Decadal Survey. And it is probably the biggest seller of any document we've ever assembled. So when the statement of task was received this time, I was really pleased that NASA specified there had to be a popular version of this Decadal Survey — so we will indeed try and excite the public.

Professor J. Zarnecki. You referred in your presentation at various times to science and to mission, which are of course different. Which is the prime driver for the output of the survey?

Dr. Smith. Well, the survey will generate a list of high-priority scientific questions and then we will look at what missions can address those questions in approximately the next ten years. There are some big questions that are unlikely to be addressed in a decade and might require two or three decades or a century; and then there are smaller, bite-sized questions that can be addressed by a particular mission. One can put together a mission to a particular object to address that set of scientific goals, and then compare how those goals could be addressed by another mission to a different object. So the science is the key here, but we have to be cognizant of the reality that you have to build something — either a telescope on the ground or a spacecraft — to address those goals.

The President. Last question.

Mr. North. There is one planet missing from your survey, and that's the Earth. I was wondering whether global warming shouldn't have been mentioned in connection with that.

Dr. Smith. Global warming is really dealt with in the decadal survey concerning Earth observation from space. We were specifically told not to set scientific priorities for the Earth and not to prioritize missions that study the Earth, but the Earth is fair game in terms of comparative studies with the other planets. So studying the Earth to understand the planets, and *vice versa*, that's certainly within our purview; setting science goals for Earth observation or other Earth studies and prioritizing missions that address those goals is somebody else's responsibility. In fact you may have heard about the stimulus plan in the United States, the billions and billions of dollars: NASA is getting a small amount of that, and most of that is actually going to give a head start in implementing the missions identified in this Decadal Survey for our science.

The President. OK, thank you, David. [Applause.] Our next talk is by Ian Crawford from Birkbeck College and it's on '*C1XS*, *MoonLITE*, and a renaissance in UK lunar science.'

Dr. I. A. Crawford. There is currently renewed interest in the Moon among UK planetary scientists. In this talk I wish to summarize two lunar missions with strong UK involvement. One of these, the *Chandrayaan-1 X-Ray Spectrometer*, (*C1XS*) is already orbiting the Moon, while the other, *MoonLITE*, is a design study about to undergo a formal Phase-A assessment. Before describing these activities in more detail, on behalf of the respective science teams, I wish to summarize the overall scientific case for lunar exploration.

The primary scientific importance of the Moon arises from the fact that it has an extremely ancient surface (mostly older than three billion years, with some areas extending almost all the way back to the origin of the Moon 4·5 billion years ago). It therefore preserves a record of the early geological evolution of a terrestrial planet, which more complicated planets, such as Earth, Venus, and Mars, have long lost. In addition, the Moon's outer layers also preserve a record of the inner Solar System space environment over the last four billion years. This rich archive of Solar System history is relatively accessible to us — only three days away by spacecraft — and, as I will describe, in their different ways both *C1XS* and *MoonLITE* will contribute to its further elucidation.

C1XS was built at the Rutherford Appleton Laboratory in the UK and was successfully launched on India's first lunar mission, *Chandrayaan-1*, on 2008 October 22. The Principal Investigator is Professor Manuel Grande at Aberystwyth University. The instrument is designed to map the abundances of the major rock-forming elements (principally Mg, Al, Si, Ti, Ca, and Fe) in the lunar crust by detecting fluorescent X-rays from these atoms when they are excited by solar X-rays. Early data indicate that the instrument is functioning as intended, especially with regard to its sensitivity and spectral and spatial resolution, although it is critically dependent on solar X-rays and it is to be hoped that the Sun soon climbs out of the current protracted solar minimum. *C1XS* data will aid in determining whether regional compositional differences (especially in the Mg/Fe ratio) are consistent with models of lunar crustal evolution. *C1XS* will also permit geochemical studies of smaller scale features, such as the ejecta blankets and central peaks of large impact craters, and individual lava flows and pyroclastic deposits. These results will all bear on important, and currently unresolved, questions in lunar science, including the structure and evolution of any primordial magma ocean, as revealed by vertical and lateral geochemical variations in the crust, and the composition of the lunar mantle, which will further constrain theories of the Moon's origin, thermal history, and internal structure.

Whereas *C1XS* is a remote-sensing instrument, *MoonLITE* is intended to make *in-situ* surface geophysical and geochemical measurements by deploying instrumented penetrators at four widely separated locations on the lunar surface. A brief review of the *MoonLITE* concept and science case been given by Crawford & Smith (*Astronomy & Geophysics*, **49**, 3·11, 2008). As currently envisaged, the penetrators will be equipped with four scientific instruments: (*a*) seismometers, to constrain better the structure of the lunar core, mantle, and crust, especially at locations far from the Apollo seismic network (*e.g.*, the polar regions and the lunar farside); (*b*) heat-flow probes, to constrain better the thermal evolution of the lunar mantle, again at sites located distant from the *Apollo 15* and *17* heat-flow measurements; (*c*) X-ray spectrometers (or similar geochemical-sensing instruments), to determine the chemical composition of the regolith into which the penetrators are emplaced, which are likely to be far from any currently sampled location; and (*d*) polar-volatiles detectors for the detection and characterization of volatiles that may be trapped in permanently shadowed polar craters; currently the expectation is that two of the four penetrators would be targeted at such localities. In addition to these core instruments, others, including magnetometers and radiation monitors, have been suggested and will be studied during the *MoonLITE* Phase-A activity.

In 2008 July the *MoonLITE* science case was reviewed by an international peer review panel, chaired by Professor Carle Pieters of Brown University (http://www.bnsc.gov.uk/7304.aspx). This review found "the scientific potential of the *MoonLITE* penetrator-network concept to be exceptionally high in the context of the international exploration activities." Partly on the basis of this review, in 2008 December BNSC announced that it would conduct a formal Phase-A study of the *MoonLITE* concept. This will be coordinated by Professor Alan Smith of UCL's Mullard Space Science Laboratory, and is expected to begin in 2009 April. Any decision on implementation would follow a successful outcome to the Phase-A study.

It is instructive to see how the *C1XS* and *MoonLITE* science cases fit within the overall objectives of lunar exploration. The most recent and authoritative summary of lunar science priorities is given in a 2007 US National Research Council Report on *The Scientific Context for the Exploration of the Moon* (http://www.nap.edu/catalog.php?record_id=11954). This Report identifies and prioritizes eight top-level lunar-science 'themes', each of which breaks down into a number of specific objectives for investigation. The top four of these themes are: (*i*) the bombardment history of the inner Solar System is uniquely revealed on the Moon; (*ii*) the structure and composition of the lunar interior provide fundamental information on the evolution of a differentiated planetary body; (*iii*) key planetary processes are manifested in the diversity of lunar crustal rocks; and (*iv*) the lunar poles provide special environments that may bear witness to the volatile flux over the latter part of Solar System history.

Neither *C1XS* nor *MoonLITE* can address theme (*i*) as this will require sample return from multiple localities and, very likely, a renewed human presence on the Moon. However, *C1XS* will contribute to theme (*ii*) through its measurements of crustal stratigraphic and mare basalt composition, while *MoonLITE* would contribute through seismic and heat-flow measurements. Both missions would contribute to theme (*iii*) by remote sensing and *in-situ* geochemical measurements, respectively. *C1XS* cannot directly contribute to theme (*iv*), but those *MoonLITE* penetrators targeted at permanently shadowed polar craters would do so directly through *in-situ* measurements of the composition of any volatiles that may be present. Thus, between them, *C1XS* and *MoonLITE* have the potential to make significant contributions to three out of four of the highest-ranked lunar-science objectives identified by the 2007 NRC Report. In addition, by helping to build up the UK lunar-science community, involvement in these two missions will ensure that the

UK is well placed to participate fully in the next phase of international lunar exploration, including renewed human exploration, that is envisaged by the Global Exploration Strategy (http://www.scitech.ac.uk/Resources/PDF/gesframework.pdf).

The President. Any questions?

Mr. H. Regnart. Can I be just a little adventurous? In the very long run, with solar power and a Laithwaite electric gun, there is a possibility of the Moon being a source of natural resources for a planet running out of those that can't be replaced. Please comment.

Dr. Crawford. Well, possibly there is; it is a very-long-term view. I think the only responsible view to take on that is that we have not explored the Moon enough to know whether it has got resources that are of possible future benefit for human civilization, either on the Earth or for other space activities that we may undertake in the inner Solar System. We are still in the scientific-exploration phase; but we absolutely need to progress this exploration phase otherwise we will never know.

The President. Thanks very much, Ian. [Applause.] We now move on to the Eddington Lecture, and here to give it is Professor Andrea Ghez from the University of California at Los Angeles. She gave a talk yesterday in Cambridge and today we have it at the RAS; that is going to be the format for these Eddington Lectures. The title of the talk is 'Bringing our Galaxy's supermassive black hole and its environs into focus with laser-guide adaptive optics.'

Professor Andrea Ghez. [Professor Ghez described how the proximity of our Galaxy's centre presents a unique opportunity to study a massive black hole and its environs with much higher spatial resolution than can be brought to bear on any other galaxy. After more than a decade of astrometry from diffraction-limited speckle imaging on large, ground-based telescopes, the case for a supermassive black hole at the Galactic centre has dramatically improved, thanks to measurements of individual stellar orbits. The advent of adaptive-optics technology has further revolutionized our studies of the Galactic centre. The speaker presented the results of several new adaptive-optics studies on our current understanding of the Galaxy's central gravitational potential, the puzzling problem of how young stars form in the immediate vicinity of the central black hole, the characteristics of the under-luminous emission associated with the central black hole, and the rôle of future large, ground-based telescopes in these studies.]

The President. Thank you, Andrea, that was certainly a very exciting talk about a very interesting topic. Some questions?

Mr. Osmaston. What can you get from the metallicity of these things?

Professor Ghez. Well, it is interesting they are actually helium rich, but I'm not sure what that tells you. It is interesting to look at an analogy with M 31, another nearby galaxy where there is evidence of young stars in the centre. The best model that exists for the young stars in M 31 is that it also has this eccentric disc of old stars, so it is thought that the outflow mass loss from the old stars provides the reservoir of gas for the formation of stars at the centre. So there you would expect helium-enriched gas.

Mr. Osmaston. So these stars in Sagittarius are actually low metallicity?

Professor Ghez. We don't have a very good handle on that. I can't tell you the metallicity because we don't have enough spectra; we don't have enough spectral diagnostics to tell you the metallicity of the stars.

Dr. G. Q. G. Stanley. What is the price tag on the addition of adaptive optics to, say, a 10-metre telescope?

Professor Ghez. It's about 7 million dollars, and since it was 90 million for the telescope itself, that's about a tenth of the cost of the observatory.

Professor P. G. Murdin. Is the radio source Sgr A* coincident with the infrared variable source, and at the focus of the ellipses of the orbits?

Professor Ghez. That's an excellent question! Yes, for star SO−2 you get a dynamical centre and in fact you solve for it without constraining it to either the radio source or the infrared source. But you don't know it very well; you only know it to tens of milli-arcseconds, whereas the dynamical centre you know to a milli-arcsecond. Then you can ask about the infrared variable source, but the infrared variable source is actually very faint, so it suffers from the biasses of the underlying sources. In fact you can watch the apparent position of Sgr A* wander as background sources walk through it, so unfortunately you can't use that as a constraint for solving the orbit.

Professor Murdin. But it is not inconsistent.

Professor Ghez. It is not inconsistent, no.

Mr. Regnart. May I, without any criticism, suggest that a better way of presenting the cost of the adaptive optics might be to recast them as how much is saved by obtaining the same quality of seeing without, for example, using other means such as putting a telescope in space. And I suspect that instead of being, as it were, a debit of 7 million dollars it would be a credit of an unimaginable figure, let us say merely astronomical.

Professor Ghez. I like your way of phrasing it.

The President. Thank you very much, Andrea. I'm sure we are going to follow this story over the next five years, ten years, and further on as more and more stars are measured and as we refine all the statistics and find other interesting things. [Applause.]

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Thursday 2009 April 23rd at $14^{h}\ 00^{m}$
in the Weston Auditorium, de Havilland Campus, University of Hertfordshire, Hatfield

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

The President. Welcome to the RAS part of this very successful JENAM meeting. We proceed now to recognize outstanding members of our community in our awards ceremony. Let us begin with the Gold Medals. The Gold Medal for Astronomy is awarded to Professor David Williams, of University College London.

Miss Samantha Hickey. The Gold Medal is the Society's highest honour.

David Williams has made seminal contributions, particularly in the area of astrochemistry, which he has developed as a powerful tool for understanding the physics of the interstellar medium. He is the leading international authority on the chemistry of star-forming regions and in the utilization of specific molecules as diagnostics in progressive phases of star formation, from dense cores and prestellar objects to protostars, discs, and planets. He has probably done more to advocate the strengths and uses of astrochemistry than anyone else, having effectively introduced the subject as a modern discipline to the UK, and building a community that has enabled it to blossom into a major area of research activity. He is the 'Williams' of the Stecher–Williams mechanism for the photo-dissociation of molecular hydrogen by the absorption of ultraviolet radiation followed by spontaneous radiative emission into the vibrational continuum of the ground electronic state. Williams was also the first to point out the importance of radiative association as a process leading to the formation of complex molecules in the gas phase. He drew attention to the rôle of internal energy in driving chemical reactions in cold clouds. With Duley, he led the way in identifying the previously unidentified infrared bands, now detected also in external galaxies.

David Williams was President of the RAS between 1998 and 2000. He was a member of the three-man task force on potentially hazardous near-Earth objects commissioned by the Minister of Science in 2000. He has been an assessor in two Research Assessment Exercises, and served, respectively, as chair and co-chair of PPARC's Astronomy and Science Committees, doing much to establish a long-term roadmap and the priorities of UK astrophysics at that time. In recognition of his services to astronomy he was awarded the OBE.

He has led research groups in Manchester and London and has produced more than 300 publications in refereed journals, as well as authoring or editing numerous books. He is a natural motivator, possessing that rare ability to enthuse people, regardless of their age or experience.

As a distinguished scientist, teacher, and organizer David Williams is a worthy recipient of the Gold Medal of the RAS. [Applause.]

The President. The Gold Medal for Geophysics will be presented to Professor Eric Priest FRS, of St. Andrews University.

Dr. R. Chapman. Eric Priest is a giant in the fields of solar and solar–terrestrial physics. He has been a leading figure in the international solar-physics community for the past forty years and is widely recognized as the world's leading expert on the magnetohydrodynamic (MHD) theory of the Sun. He is best known for his work on magnetic reconnection: the process by which energy is released as a magnetic field changes its connectivity. He has made seminal contributions toward understanding the basic physics of this process and its application to the Sun and, due largely to his insights, we now recognize the critical importance of three-dimensional effects in reconnection. Priest's work on reconnection links slow processes, dominated by diffusion, to rapid processes involving MHD shocks. He developed an ingenious way of conceptualizing complex three-dimensional structures in terms of a 'magnetic skeleton', which is now widely used by the MHD-theory community.

Priest was elected FRSE in 1985, FRS in 2002, a Member of the Norwegian Academy of Sciences and Letters in 1994, and a member of the European Academy of Sciences in 2005. He has delivered many named lectures including the James Arthur Prize Lecture at Harvard and the Lindsay Memorial Lecture at the Goddard Space Flight Center. He was awarded the Hale Prize of the American Astronomical Society in 2002, only the second time it has been awarded to a British scientist. Priest created and led an extremely active and successful group at St. Andrews, served three times on Research Assessment Exercise panels and, as co-chairman of the PPARC Science Committee, played an important rôle when the UK joined the European Southern Observatory.

His impact on the discipline of solar and solar–terrestrial physics cannot be overstated, while he has set a fine example for the rest of the community to follow. This lifetime of excellent scientific achievement and devotion to the advocacy of solar physics make Eric Priest an outstanding person to receive the Gold Medal of the RAS. [Applause.]

The President. The Eddington Medal will be presented to Professor James Pringle of Cambridge University.

Miss Hickey. The Eddington Medal is awarded for investigations of outstanding merit in theoretical astrophysics.

James Pringle is one of the main developers of the modern theory of accretion processes, which has an all-pervasive influence on current work in astrophysics. He is probably best known for his work on disc accretion, where he cast the theory in a flexible and adaptable form that clearly separates assumption from well-established physics. Much of his early work is summarized in a review paper which has accrued more than 1000 citations. However, his major contributions go far wider. For example, he was one of the first to realize that there must be a special formation mechanism for neutron-star binaries in globular clusters, and proposed (with Fabian and Rees) the now-standard tidal-capture model. Again, he has had a considerable influence on star-formation theory, emphasizing the importance of the fact that stars form in clusters rather than singly.

Pringle continues to be an active researcher producing world-leading science, with interests now extending to planetary formation and dynamics. Throughout, his work is marked by a clear understanding of how the physics produces the effects under study. By distinguishing important physics from irrelevant detail he makes astrophysical problems soluble, and, importantly, makes their solutions easy to understand and apply more generally. James Pringle has had enormous influence in his field, which makes it highly appropriate that his achievements should be recognized by the award of the Eddington Medal. [Applause.]

The President. The Price Medal will be presented to Professor Malcolm Sambridge, of the Australian National University.

Dr. Chapman. The Price Medal is awarded for investigations of outstanding merit in solid-earth geophysics, oceanography, or planetary sciences.

Malcolm Sambridge has made particularly significant contributions to the study of non-linear inverse problems. He pioneered the use of genetic algorithms in geophysics, and developed the Neighbourhood Algorithm, an efficient method to optimize non-linear inverse problems with stochastic methods. This method has found wide application in the geosciences including seismic tomography, earthquake location, fluid flow in the subsurface, geodynamic modelling, mixing problems, and the reconstruction of thermal histories.

Sambridge has always stressed the importance of the estimation of uncertainty and in this connection has developed tools which are in use in institutions around the world. Many researchers have used his codes to solve problems which otherwise would have been almost intractable. Indeed, it is noteworthy that Sambridge is not just a scientist and teacher of wide-ranging interests and achievements; he has devoted much personal effort to developing and maintaining user-friendly software, which is made freely available from his well-documented web site.

Sambridge is simply the 'first name' in geophysical inversion and sampling theory, and a richly deserving recipient of the Price Medal. [Applause.]

The President. The Jackson–Gwilt Medal will be presented to Professor Peter Ade of Cardiff University.

Miss Hickey. The Jackson–Gwilt Medal is awarded for the invention, improvement, or development of astronomical instrumentation or techniques, for achievement in observational astronomy, or for achievement in research into the history of astronomy. Unfortunately, Professor Ade is unable to be with us today.

Peter Ade is the leading designer and supplier of infrared filters, polarizers, dichroics, and beam splitters. For over thirty years his components have been used in almost every major infrared facility constructed, and his laboratory has led the development of new designs and new manufacturing techniques.

Ade started his career at Queen Mary Westfield College before moving to Cardiff to be part of what has become one of the largest infrared and millimetre-wave instrumentation groups in the world. He pioneered the design of ^{3}He-cooled sub-millimetre bolometric instruments and led the development of metal-mesh filters. This technology has become critical to the design of low-pass, high-pass, and band-pass filters, as well as other spectrometric components. The instruments to which he has contributed critical sub-systems are too long to list here; suffice it to mention that currently Ade is involved in *SCUBA-2, Herschel–SPIRE, SIRTF, Clover*, and *Planck*.

Instruments designed by Ade have made possible important advances in atmospheric sciences, planetary sciences, astrophysics, and cosmology and make him a worthy recipient of the Jackson–Gwilt Medal. [Applause.]

The President. In recognition of his lifetime of contributions to the UK astronomical community, the RAS Service to Astronomy Award 2009 is presented to Professor Sir Arnold Wolfendale FRS, of Durham University.

Dr. Chapman. This award honours an individual who, through his outstanding work, has promoted, facilitated, or encouraged the science of astronomy and developed its rôle in the life of the nation.

From a first-class degree at Manchester in 1948, Arnold Wolfendale studied for a PhD in cosmic-ray physics under Blackett, beginning the scientific study with which he is most associated. Starting with the physics of cosmic-ray particles he took up the issue of their origin and what might be deduced about their sources from astronomical observations. This area of study led Wolfendale to inspire Durham University, where he remains Emeritus Professor, to focus on astronomy, astrophysics, and cosmology.

Wolfendale has carried out important tasks for SERC, and subsequently PPARC, shepherding through the funding system many of the telescopes and space missions that are now producing world-ranking science. He has been an active public advocate of fundamental-science research and has provided robust support for university research programmes and facilities. He was a frequent visitor in Whitehall where his dealings with Ministers of Science set a new, direct, and forceful tone for the on-going debate between scientists and government.

Wolfendale was elected to the Royal Society in 1977, became President of the Royal Astronomical Society in 1981, and was appointed the 14th Astronomer Royal in 1991. In 1994 he was elected to the Presidency of the Institute of Physics, and in 1999 became President of the European Physical Society. Notwithstanding his commitments, in recognition of which he was knighted in 1995, Wolfendale continued to be a frequent guest speaker at societies and clubs across the country, where he lectured not only about cosmic rays, but also about life in the Universe and the history of timekeeping. [Applause.]

The President. The Award for Services to Geophysics will be given to Dr. David Kerridge, of the British Geological Survey, Edinburgh.

Miss Hickey. This award honours an individual who, through his outstanding work, has promoted, facilitated, or encouraged geophysics or Solar System sciences and developed their rôle in the life of the nation.

David Kerridge is currently the Head of the Earth Hazards and Systems theme in the British Geological Survey (BGS), which he joined in 1983, and Head of Station at Murchison House, its main office in Scotland. He became leader of the Geomagnetism Group in 1991 and assumed managerial responsibility for the BGS's work in earthquake seismology in 1997.

Kerridge expanded the financial base and scientific remit of the Geomagnetism Group which is responsible for running the three UK permanent geomagnetic observatories, for example, by providing support for directional drilling for hydrocarbons in the North Sea and for support to the *SWARM* satellite constellation which is due for launch in 2010. Following the 2004 Andaman Islands earthquake and tsunami, he led a government-commissioned study to assess the tsunami threat to the UK and has represented the UK on the GEO Tsunami Working Group.

In addition, Kerridge has made important international contributions to geomagnetism. As President of the International Association for Geomagnetism and Aeronomy he laid the groundwork for the 50-year celebration of the International Geophysical Year and expanded opportunities for young scientists and scientists from less-developed countries. As chair of INTERMAGNET he helped establish a global network of digital magnetic observatories in places that previously had represented large gaps in the coverage.

For his many years' unselfish devotion to furthering geomagnetism and advancing the careers of his colleagues and others in the community, especially the next generation and those from poorer nations, the RAS Award for Service to Geophysics is presented to David Kerridge. [Applause.]

The President. The Group Achievement Award is given to the *SCUBA* team represented by Professor Walter Gear and Professor Colin Cunningham.

Dr. Chapman. This award recognizes outstanding achievement by large consortia in any branch of astronomy or geophysics where it is not appropriate to present, jointly, one of the other awards of the Society.

SCUBA, the *Submillimetre Common-User Bolometer Array*, was a camera built at the Royal Observatory Edinburgh and mounted on the *James Clerk Maxwell Telescope* on Mauna Kea. It saw first light in 1996 July, and operated until 2006. By enabling astronomers to map the sky at wavelengths of, principally, 450 and 850 μm with unprecedented speed, and hence to study thermal emission from cold dust, it ushered in a new era for several key areas of astronomy, in particular the formation of galaxies, stars, and planets. It is estimated that *SCUBA* has had the highest citation-impact factor of any astronomical instrument after the *Hubble Space Telescope*, and this at a cost orders of magnitude less than *HST*. In the field of galaxy formation, *SCUBA* has unearthed hitherto unseen galaxies with prodigious rates of star formation enshrouded in dust; these '*SCUBA* galaxies' are now presumed to be the predecessors of modern-day giant ellipticals. *SCUBA* also demonstrated the existence of large amounts of cold dust in nearby (Milky Way-type) galaxies. In the field of star formation, *SCUBA* has established physical conditions in prestellar cores and young protostars and determined that the Initial Mass Function for star formation is largely determined by the processes which form prestellar cores. In the field of planet formation, *SCUBA* obtained the first images that show rotating debris discs around Sun-like stars, with morphological evidence for planet formation.

SCUBA has been a truly remarkable instrument, in its initial concept, in its realization, and in the science which it has enabled. It is entirely fitting that the *SCUBA* team, too numerous to list here, who built it, should be recognized by receiving the Group Achievement Award. They are represented today by Colin Cunningham, Director of the UK *Extremely Large Telescope* programme and Honorary Professor at the University of Edinburgh. [Applause.]

The President. The Fowler Award for Astronomy will be presented to Dr. Sarah Bridle, of University College London.

Miss Hickey. The Fowler Award (A) is presented to an individual who has made a particularly noteworthy contribution to the astronomical sciences at an early stage of their research career, and is intended to recognize this contribution sufficiently early to give the career impetus.

Sarah Bridle has made important contributions in several different areas of cosmology, including the cosmic microwave background, gravitational lensing, and galaxy-redshift surveys. She is perhaps the most skilled and prolific of the new generation of theoretical cosmologists who combine observational results from more than one experiment in order to provide improved constraints on the various parameters which constrain the world model.

In 2001 she wrote an influential paper with Lewis on the application of Markov Chain Monte-Carlo methods to the estimation of cosmological parameters. This technique has since become the standard method for determining parameters from large cosmological data sets. Following on from this, she completed important work on extracting the maximum amount of information from the next generation of such large data sets, including the *Square Kilometre Array* and future photometric redshift surveys.

Sarah has contributed greatly to the mapping of dark matter on various scales by introducing a new and powerful maximum-entropy mass-reconstruction algorithm, notable for its versatility and applicability over a wide range in surface density. She also developed the machinery to combine cosmological constraints from distant supernovae, the microwave-background angular power spectrum, and various probes of large-scale structure. Sarah also has investigated non-parametric techniques for studying the evolution of dark energy.

As co-coordinator of the Dark Energy Survey Science Committee's Weak Lensing Working Group and a UK co-representative on the Dark Universe Explorer Steering Committee, Sarah occupies key positions in the fields of dark matter and dark energy,

As a young scientist of proven achievement and great promise, Sarah Bridle is a very worthy recipient of the Fowler Award. [Applause.]

The President. The Fowler Award for Geophysics will be presented to Dr. David Tsiklauri, of the University of Salford.

Dr. Chapman. The Fowler Award (G) is presented to an individual who has made a particularly noteworthy contribution to the geophysical sciences at an early stage of their research career, and is intended to recognize this contribution sufficiently early to give the career impetus.

David Tsiklauri is an extremely talented solar physicist with an impressive publication record and a strong background in theoretical plasma physics. He is one of the leading young solar-plasma physicists in the world focussing on the major unsolved problems of solar physics, particularly that of the heating of the solar corona.

Tsiklauri began work on solar physics around 2001, when he joined the research group at Warwick, before moving to Salford where he has established a distinctive solar-physics research group. Whereas most UK solar coronal physics is dominated by fluid theories, he has brought the importance of correctly treating the underlying kinetic processes into the mainstream and has established himself as one of the UK's leading experts in this field.

Tsiklauri was one of the first few people to apply particle-in-cell simulations to the coronal-heating problem, and to show how the wave energy of the Alfvén waves can be transformed to plasma particle energy in the conditions of the solar corona.

Tsiklauri uses a combination of advanced numerical and computational methods in his research and has identified a new electron-acceleration mechanism by Alfvén waves, one that had never been considered before and a potentially very important contribution to the problem of coronal heating. More recently he has extended this work to challenge the problem of magnetic reconnection using a collisionless kinetic approach.

David Tsiklauri's combination of original, distinctive research and commitment make him a very worthy recipient of the RAS Fowler prize. [Applause.]

The President. An Honorary Fellowship will be conferred upon Professor Matthew Colless of the Anglo–Australian Observatory.

Miss Hickey. The RAS may honour any person eminent in the fields of astronomy or geophysics by election as an 'Honorary Fellow' of the Society. This is typically in recognition of services to astronomical and geophysical sciences such as distinguished leadership of a school, observatory, or laboratory; outstanding services to national or international scientific organizations; exceptionally important work in editing scientific publications; influential work in education and public outreach in these sciences; or specially outstanding distinguished work in the history of these sciences.

Matthew Colless has been Director of the Anglo–Australian Observatory since 2004. Faced with the challenges of withdrawal of UK funding from the Observatory, he has pushed forward a vigorous programme of instrument development and scientific endeavour, spearheaded by contributions to the Australian Astronomy Decadal Plan 2006–2015, which envisions the AAO evolving into a National Optical Observatory, providing not only on-shore domestic optical/infrared facilities with significant new instrumentation capabilities (such as *HERMES*), but also co-ordinating Australian involvement in international projects like *Gemini* and *Magellan*, and future projects like the *Giant Magellan Telescope* and *PILOT*, as well as a thriving instrumentation technology programme. In addition to this, he continues an outstanding personal research programme, particularly in galaxy structure and evolution, and large-scale structure. In recognition of his leadership rôle in Australian astronomy, Honorary Fellowship of the Society is conferred on Matthew Colless. [Applause.]

The President. The second Honorary Fellowship will be conferred upon Professor Bernard Schutz of the Max Planck Institute for Gravitational Physics, Potsdam.

Dr. Chapman. Bernard Schutz is Managing Director of the Max Planck Institute for Gravitational Physics (the Albert Einstein Institute) in Potsdam and Director of its Astrophysical Relativity Department. He also holds a part-time professorship at Cardiff University. Schutz's early pioneering work on the stability of rotating stars and identification of their modes of oscillation led him into the field of predicting the gravitational-wave signals from pulsating and merging neutron stars and black holes, and into the effort to devise and implement new algorithms to detect the signals from these and other potential sources of gravitational waves, using the current generation of gravitational-wave telescopes. Schutz is the PI responsible for data analysis for the *GEO600* (the German–British gravitational-wave detector) collaboration, a member of the *LISA* (*Laser Interferometer Space Antenna*) International Science Team and the author of several influential books. In recognition of his leading rôle in the fields of stellar and gravitational-wave astronomy, Honorary Fellowship of the Society is conferred on Bernard Schutz. [Applause.]

The President. The last awards are for the post-graduate poster competition. Would the following please stand so we can show our appreciation? In the UKSP/MIST category, the overall winner is Fraser Watson, and second prize goes to Daniel Whiter. First prize in the astronomy category is Cristobal Espinoza, second prize goes to Roberto Raddi, and third prize to David Kipping. Please contact Jim Hough afterwards for what you are going to be given. [Laughter.]

My thanks also to Samantha and Robert who have read the citations so well and for their rôles yesterday. [Applause.]

We now move to the 2009 RAS Gerald Whitrow Lecture, which is by Professor George Ellis of the University of Cape Town, who is going to tell us about 'Evidence and theory, fact and fancy; the state of cosmology today'.

Professor G. F. Ellis. [It is expected that a summary of this talk will appear in a future issue of *Astronomy & Astrophysics.*]

The President. Thank you very much, George, for a very interesting and stimulating lecture. I'm sure there will be some questions. Donald?

Professor D. Lynden-Bell. George, I'm reminded of a great Presidential Lecture given by Herbert Dingle in which he called the Cosmological Principle the Cosmological Assumption, and he called the Perfect Cosmological Principle the Cosmological Presumption. This I thought was a very good corrective and I congratulate you on having given a very good corrective to what you regard as the majority of us.

Professor Ellis. Thank you, Donald. I should perhaps say what is very interesting is looking back. For people who want to pursue this, go and look back at Dingle and at Bondi's book where he discusses the Cosmological Principle, and then go and look at Stephen Weinberg's book where he discusses it. Weinberg's 1973 book was before inflation and at that stage he just took the Cosmological Principle; there wasn't inflation to justify it. Stephen Weinberg in that book says the following, and I quote, "It is inconceivable we are at the centre of the universe". It may be something you don't want to believe but to say it is inconceivable is simply wrong. It is conceivable that we are near the centre of the Universe. You may not like it but you can conceive it and you can test it.

Professor Sir A. Wolfendale. I'm sure Professor Whitrow would have been proud of you, as we all are. I want to make a comment about the forensic attitude, which I fully support. Some years ago we looked at the foreground in the *WMAP* data, because of course we look at the early Universe through the galactic halo. And my beloved cosmic rays got in on the act in that we found correlations between predicted cosmic-ray spectral changes and features in the map. So I would caution taking the details from the map, hook, line, and sinker: there may be subtleties and, for example, you mentioned this droop that we have at small *l*. That in turn could conceivably be due to cosmic-ray effects and indeed the positions of the peaks may not be quite what people think because of cosmic-ray effects. I think the detectives are still at work in that area.

Professor A. Gould. I think that your lecture was a very serious attack on the scientific process. So you said over and over again and louder and louder, at least eleven times, that the anthropic principle was untestable. But we don't know whether it is testable or not because there can be physical theories that can be verified in the laboratory that predict that the physical constants are settled by falling out of the vacuum. We don't know whether that is going to happen or not going to happen and if you repeat over and over again, louder and louder, that it is not going to happen you are just putting your hand up in front of scientific progress. There is no value at all to calling these things scientific theories, I agree with you there, but science progresses not just by theories but by speculations that lead to ideas and experiments in the future that are very important. You are walling that off and I think it is highly objectionable.

Professor Ellis. We must be very careful about two different things here. We may show in the laboratory that the vacuum has certain properties and that this depends on the state of something or other. That, of course, I would thoroughly support. The fact that you could show that would not prove that up there in the Universe beyond the visual horizon there are other domains where it has different values. Or at least I cannot see how you would prove that.

Professor Gould. It will have proven, though, that the thing that you said was unprovable, that the Universe was highly improbable, because it will show that these things happen by chance. This is a legitimate programme for scientific advance; I agree with you that it is not a theory, but it is something that pushes people's imagination about how to do experiments, and in that sense it is valuable.

Professor Ellis. Could you please tell me how you prove something happens by chance?

Professor Gould. Well, you show it the same way that you have false vacuum states that you see in iron magnets or whatever and you show that it is the fundamental physical process and that it is a random value. Anyway, these are experiments that could be done.

Professor Ellis. I think you are challenging a different part of my talk. I said that there were no lab tests possible of dark energy, and I think you are saying that there are lab tests possible for dark energy, and I think that is your real difference with me, and in so far as that is correct then I was wrong in what I said and you were right. If there are lab tests of dark energy, those are obviously incredibly important and should be pursued, but that still won't relate, won't prove, that a multiverse exists, which is a quite separate proposal.

Professor Gould. I *don't* aim to prove that multiverses exist, but the multiverse idea is something that leads you to look for these false vacuums. And it's not just the dark energy; personally I think the dark energy doesn't require any sort of anthropic argument. But the other things that you pointed to about electron mass, charge, and so forth: there's a legitimate case that that might actually be true, if we have a huge number of these that are actually in a physical theory that is demonstrated by much other evidence, and are shown to be random numbers and they all turn out to be favouring life. I think it is something that deserves serious consideration and I think that people should be forced to come up with experiments or pushed in the direction of experiments and not claim — as you say and I agree with you — that what they are doing are scientific theories, which they are not. But they *are* legitimate speculations that push science forward — they are not simply philosophical diversions for science.

The President. Thanks. I said that George's lecture was stimulating, provocative perhaps! Any other questions?

Mr. H. Regnart. Sometimes a scientific hypothesis may pop up just by intuition without any precursor at all and later be validated by experiment or observation. But apart from that, speculation is an absolutely essential precursor of any hypothesis that may or may not be validated by observation or experiment. And that is also perfectly alright if, but only if, it wears a sandwich board, both sides of which say "I am a speculation".

Professor Ellis. I'm 100% with you. [Laughter.]

The President. Let's thank George once again for a great lecture. [Applause.] Now, my final announcement before we break for tea and then the community forum, is just about the NAM next year, which is going to be held at the University of Glasgow from April 12–16. This one has been, I think, a tremendous success and Glasgow have got a challenge on there. Anyway let's all meet together in Glasgow next year. [Applause.]

[The A&G meeting was followed, after tea, by the STFC Community Forum.]

STFC COMMUNITY FORUM

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

Panel:
A. M. Cruise (*Chair of Astronomy Grants Panel, STFC*)
R. Gilmozzi (*PI of E-ELT, representing ESO*)
J. Krautter (*EAS President*)
K. O. Mason (*CEO STFC*)
D. Southwood (*ESA Director of Science*)

The President. We'll start with our distinguished panel by getting each of them to give a two-minute introduction, but I'll give Keith Mason five minutes as we'd like to know a little more about the budget. I have set up the Astronomy Forum, which represents the astronomy departments in the UK (astronomy being defined as being anything done above the clouds). The Astronomy Forum has already met twice and I think it's going very well. We are already engaging with STFC and other bodies, and there is more detail on the RAS website.

Professor D. Southwood. The ESA Science Programme and Robotic Exploration are of interest to those here. There is good news and bad news. The UK needs to think about how we will come through the selection process for Cosmic Vision and how this affects national priorities. We are already beginning to get involved in Mars exploration with our US colleagues and we can use this as a buffer, but they have to do what they want along with what we want, and I think that the UK is psychologically better at that than some of our European colleagues, so I look for leadership in this community. We have short-term major financial problems because we started both *Bepi–Colombo* and *Gaia* at the same time. *Bepi–Colombo* is over-budget and needs attention, but the good news is that the budget will increase annually by 3·5%. We will need to make some serious decisions quite soon about the next tranche of missions for 2017/2018.

Professor A. M. Cruise. I am currently Chair of the Astronomy Grants Panel. It's an agency that makes many people happy; unfortunately it makes twice as many people unhappy. I'm willing to take questions on the process involved but this is not an appropriate venue to discuss individual grant applications.

Dr. R. Gilmozzi. As Principal Investigator of the *ELT*, I wish to underline the importance of the UK's membership of ESO. It has brought a lot of know-how, instrumentation, and the *VISTA* telescope; and it is complementing the Paranal observatory, which starts operations soon, and it plays an important part in the *ELT*.

Dr. J. Krautter. I am President of the European Astronomical Society. The European approach has been shown to be a powerful way of producing new facilities and I would like to talk about the European perspective on UK astronomy. UK astronomy is on a very high level but the UK cannot exist on its own. The UK is not only a member of ESA and ESO but also of European networks like ASTRONET, OPTICON (Optical-Infrared Coordination Network for Astronomy), and RadioNet, and, in addition, the Royal Astronomical Society, the society of British astronomers, should be more integrated into Europe. We need more joint meetings and I'm happy that the RAS has given us the chance to present ourselves at this meeting.

Professor K. O. Mason. I'd like to summarize yesterday's budget — what it did and did not do for the science programme generally. The science ring-fence was protected, which was a huge win given the current economic turmoil. This underlines the Government's commitment and indicates that the Government perceives that science research is a key element on the road to recovery. We now have to deliver on our promises and future success will be judged on these expectations. Economic impact means making the most of what we do. This will be a challenge to everybody in the research base. I believe that there is a lot of expertise from people in this room and in this country which is vital to the wider health of the country's research base. The good news is that we have been putting together structures within STFC, which, whilst not universally popular, do make a gateway for you to make an impact in a very visible way and which justifies our existence. There are challenges ahead, one of which is the catastrophic devaluation of the currency over the last few months, which clearly impacts upon the affordability of the astronomy programme. This could have been very serious, but I can report that the Department for Innovation, Universities and Skills has made great efforts to cover the gap in the finances for next year to the tune of about £30 m. Even so, because of the fluctuations, we will have to find £10 m in this current year and we are doing that by slowing the programme and making other decisions at the end of this month. The other message is that no one knows how much money will be available in the future. It's vital to know now what our priorities are in case sudden decisions have to be taken, as they often are. We'll be considering these questions over the next few months.

The President. Thanks. With regard to prioritization, perhaps Michael Rowan-Robinson might like to make a comment.

Professor Mason. We're setting up the long-awaited Ground-Based Astronomy Review and Michael has agreed to Chair that.

Professor M. Rowan-Robinson. I have taken on what everybody has warned me is a poisoned chalice, and there will shortly be a public web page telling you about this review. It's a kind of mini-decadal review looking as far ahead as 2020 and we want to report by October. The main element will be to consult with you and we are already preparing a document which we want to publish by the end of May. You then have three months to input your views on what facilities you would like to see over the next decade and then we will pull it together for the report.

The President. Thanks, Michael. So let's throw this discussion open now for questions from anybody to any member of the panel.

Dr. M. Dominik. This is a question for STFC mainly, but everyone else is encouraged to comment on it. On Monday the Science Minister, Lord Drayson, said that the UK should be proud of the quality of its scientists and their productivity; they are amongst the leaders in the world, and future prosperity will depend increasingly on the country becoming a knowledge-based economy. It seems obvious to me that we need to make efficient use of the existing creative potential of people as a crucial priority to drive innovation. In particular, what are the current opportunities for young talent and what measures of improvement should be taken?

Professor Mason. I think we all agree that we need to make use of talent, particularly young talent, as we are looking to the future and this is a skills issue. Young people should go into science because it is fascinating and we love to find out new things, and also being smart is the only way to build a future for our descendants. We need young people for a sustainable future, to combat the challenges of climate change and an ageing population. The other point, as anyone involved in science will know, is that science is not a linear process. To have a resilient system in the future we cannot afford to specialize too much in one direction, such as the influx of particle physicists into banking, for instance, so we need a breadth of knowledge, expertise, and activity. I'm very keen on the interdisciplinary approach — everybody benefits.

Professor Southwood. I agree with Keith. Don't expect to spend a career in science repeating your PhD endlessly. Be open to moving, and think about what facilities you can exploit. The older scientist tends to get bogged down protecting the institution that he or she works for. Science evolves as society's needs evolve and astronomy evolves in parallel, as our society needs astronomy. In 40 years' time, astronomy will be different.

Professor T. Ponman. Keith, you said that we need to be clear about our priorities given the present and future uncertainties. I, for one, am a little confused as to whom the STFC strategy document is for. A document was produced which we thought was a draft strategy document when it fact it was a consultative document. That's not what the communities or the panels will need for setting priorities internally. What Michael is going to be doing will set genuine priorities within the programme. I wonder what the rest of the picture is. How do you see a strategy being developed which will actually form decisions within the community about the relative priorities of different projects and facilities?

Professor Mason. The target is one year from now. A glossy document is being produced for the end of the month but one of the problems is that the strategy never stops, it is evolving all the time. A document to which the panels can refer is about a year away but if we can do it by October that would be more comfortable, but of course this is a big job and consultation takes time to do properly.

Professor Ponman. Are there other elements in addition to the Ground-Based Review which will contribute to that process?

Professor Mason. There will be but I can't yet tell you exactly what will happen.

The President. Next question. [Silence.] So everybody is happy about what has happened. [Laughter.]

Professor K. Nandra. I have a question about Cosmic Vision. This is somewhat different from previous research endeavours in that ESA is asking member states to put in serious money for technology development in advance before the programmes are selected. As far as I am aware there is no evidence that the UK is doing that. My question to David Southwood is whether he is concerned that the UK does not appear to be pulling its weight in this exercise; and hence the question to Keith is, will this put the UK's scientists at a disadvantage when the selections for Cosmic Vision are actually made?

Professor Southwood. This is not a planted question! We've gone through various phases. There was, at one time, a fair bit of money for blue-skies research and, if you wanted funds for space research, you had to supply proof of principle, at least proof of technology, and that was best done in the university laboratory, at least in this country. The wheels fell off the wagon 10–15 years ago when ESA started to devolve work, and this started to corrupt the system. Certain member states say that if you seed this work then you've got to carry right through and build the entire instrument. We've done that for *JWST* and *NIRSPEC*. We've asked our advisory committees and even our Council and we are told "No, instrumentation should be built in the universities and funding should come from the national side". The problem is that when potential technologies are being tried out early in the project, if we seed too much we are seen as biassing the system and therefore become responsible for looking after the system. If we don't get national funding spent at this point we get the 'not invented here' problem when we hand over the project. It's been a long-standing problem and it goes in cycles. There is a solution and that is to shift the responsibility entirely to ESA. Personally, I think this kind of work is best done independently in the science community and that requires turning to Keith for the money for blue-skies instrumentation.

Professor Mason. For Cosmic Vision we will put money into appropriate studies. It's not as much as we would like to put in and more than some people would have us put in. The wider question is generally one of basic technology development in this country, which applies both in space and other areas. We have got to a stage where we are not putting enough into that kind of development in order to secure the future, particularly in the space area. Timescales are long and we only see the fruit of today's investment in about 20 years' time. My personal view is that we do not spend enough in this area and the only way to do a better job is to prioritize. Trying to spread the jam too thinly is a recipe for disaster and we should focus on doing a few things better and we need to debate what these things are.

Professor Cruise. I just want to make a demographic point. There is a noticeable decline in the number of people coming through with instrumentation skills. The universities have not done a very good job in providing a career structure for people with technical skills and the number of students wanting to do courses which include hands-on instrumentation has been dropping. There are lots of reasons for this. The Government, it seems, now want to get back into manufacturing industry again, and of course the university departments must react to that policy change, but with an inevitable delay built into the process. This is extraordinarily dangerous for the theoreticians and modellers because they then will not have front-rank instruments in the future unless the technical support is in place. I hope the various analyses and strategies will take account of the urgent need to replenish this resource of very important people.

The President. The basic answer I seem to be getting is that funding is in place for Cosmic Vision. Is that what you understand, Paul?

Professor Nandra. That's not really what I understand. What I was told was that the majority of funding for Cosmic Vision had been put on hold.

Professor Mason. I have already described the short-term budgetary issues and everything is going to get caught up in that, but the bottom line is that there is provision for Cosmic Vision. It could be more but it's sufficient.

Professor E. Brinks. I have a question for Mike Cruise. How is the grants line, particularly funding for PDRAs, going to fare in the near future?

Professor Cruise. Unless I get some new instructions, the level will be the same as that which I discussed with the office in the last month or so. If that is the case then the 25% cut which was made two years ago will still be in place but it will not be any worse than that. We are now in a situation where the panel can only fund world-class research. The UK has been able to build an astronomy base over the last few years by funding things which are more speculative, just below this level, and these have now developed into facilities of world-class quality. I have placed my report on the STFC website (http://www.stfc.ac.uk/resources/pdf/AGPState.pdf) and in it I worry for early-career scientists. There are structures in the evaluation of grants that inevitably tend to lean towards people at the peak of their career rather than those just starting out, and it's difficult to alter the grants system to favour young researchers in the way we would like. If the current level of funding carries on for many years into the future then it will be very serious. I think STFC will try to maintain the current level or even try to improve it in the future but we are all limited by the economic circumstances. To sum up, I've not been told of any change in the grants line, but it will be a very tough grants round.

Professor Mason. I echo much of what Mike says. It's a highly competitive line but it's also an expensive line and a significant part of the funding. It's all about choice and the only way to put more money in to improve grants would mean removing money from someone else, and this is far from easy. We could consider the way we handle grants — is there a better way of doing it? And as Mike has noted, there are in-built systemic tests in the awards system which fail particular people at particular stages of their careers. Can we do something different? I'm very happy to have such a debate and to discuss how to make best use of what is available but everything is now getting much more expensive much more rapidly than the amount of money is increasing. It's a fact of life and it means hard choices.

Dr. D. S. Brown. This is directed to anyone on the panel who wishes to answer it. We've talked about challenges for the future, financial or otherwise, and there are perhaps many people wondering what we can do to contribute to a solution. What is your advice for people like me, perhaps the younger, junior astronomers?

Professor Mason. When David and I talked about multidisciplinary research, I didn't mean hopping between disciplines; but one of the best things anyone can do is to spend time talking to people in other disciplines, about how we can help each other. You would be amazed how much leverage there is. This can also lead to easing of financial problems too because you can have a diversity of income, and you and your university will be more robust. Go out with open minds and use your range of skills, which are hugely in demand; this is a powerful thing that we should take more advantage of.

Professor Southwood. I encourage you to take an interest in contacts outside the university, particularly in the political world — an MP might one day become a minister, so if you believe what Keith is saying, get it out there in the minds of people who in the end vote for or influence budgets. This has to do with communicating to people in the political process, independent of their party affiliation: they will have an influence. This can be done not in an aggressive, political manner, but as a spokesman for science and technology. And remember, young people get more attention from MPs.

The President. Remember your fellow students may be in charge eventually; two of my fellow students are sitting on the panel here, and I would never have dreamt back then that they would be! [Laughter.]

Dr. Gilmozzi. Another activity that is important for the development of astronomy is to explain and bring our subject to the general public; after all, what we are doing is something that contains excitement, discovery, new knowledge — what we are paid, in a sense, to produce for people. We must make the effort to bring our subject to the public.

Professor Carole Jordan. May I ask Keith what portion of the total STFC budget goes on grants, and has that changed in the last five years?

Professor Mason. Roughly, 60 million pounds a year is spent on grants, out of a total budget of about 400 million pounds.

Professor Jordan. So why is it not possible to make cuts from the vast majority, since the grants line is what supports people who do the science? The ideas come from individuals in universities or government establishments that work with the universities. One thing for which I fought tooth and nail when I was on the Research Council is that the grants line was the most important line in the whole budget, and it's an easy target — if you tie up too much money in capital projects, and you don't leave enough money to support individual scientists on the research that can be done, particularly young scientists who have new ideas, you will run down the quality of the research that is done; and so I hope there is still someone on the Research Council who fights for the grants line. [Applause.]

Professor Mason. Well, there are two people on this panel who fight for the grants line too — but the majority of our money goes on international subscriptions. A lot of that goes to David [Southwood] here. [Laughter.] The amount that I turn over to David is out of my hands; it is fixed by the agreement by which the UK joined ESA, and I do not have control over whether that budget goes up or down, since it is done by majority voting on the various councils. Another large chunk of money, about the same as the grants line, goes on domestic facilities, which are long-term investments with relatively little flexibility. I hear what you say, Carole, and I agree with everything you say, but it comes back to hard choices — the only way to put more money into grants in the current climate is to stop doing something else: you have to tell me what to stop doing.

Professor Jordan. Every time you have a new project coming up, or you are looking at new projects within the time frames that are relevant, more care should be taken that when new things are taken on they do not imply a drop in the grants line.

Professor Mason. You never have that luxury because the timescale for projects in our field is typically a decade, and we don't even know what the budget will be in a year's time. We have to have a balance between the people with the bright ideas and the facilities with which to execute those bright ideas. Do you want to have an *ELT*? The astronomer will say 'yes', but you have to balance that with having people to use that telescope, and that balance is very difficult to get right; ultimately, the only way to put more money into grants is to stop doing something else.

Professor Cruise. Carole has hit the nail on the head. From the operational perspective of the grants panel, we see the result of the last decades of facility and instrument building — for example, if we look at *Herschel* and *Planck*, the UK has probably spent about 100 million pounds over the past 10 years contributing through ESA to those instruments, but we find ourselves able to spend only 3 million pounds supporting the science that comes from them. The ratio is that extreme. I think one of the biggest problems STFC has to face in formulating its strategy is working out what that ratio should be. If you really want to exploit some of these facilities, you will have to build less. But the current situation is very depressing: having spent 100 million pounds on these facilities, we are really limited in the number of post-docs that we can get to benefit from them. It is eye-wateringly sad to see the ratio being as large as this; however, this is not something that is an ill judgment on the part of the STFC — this is the result of the policy of the last 10–15 years. Some strategy for getting this correct needs to be found for STFC to maximize the science output from its programme.

Dr. Krautter. I can assure you that there are these problems in other European countries too. I realize, as Keith said, that one has to find a kind of balance; but from this discussion, I get the impression that a significant fraction of the UK community feels that the grants get decreased too much. I really warn that if you decrease the grants, you will have a lot of good and excellent instruments, but you will soon miss the people to exploit them fully. I really warn that the grants are the weakest link in this chain.

Dr. C. Owen. I wanted to follow up the question that Paul asked. The issue with Cosmic Vision and the new way that ESA operates is fine in principle, but STFC did not prioritize very strongly for Cosmic Vision, and as far as I am aware, has actually funded all the missions, but at a relatively low level. What concerns me is that there is a down-select coming up, and rumours are that that down-select will not down-select very much. Will we find ourselves moving into the next phase of Cosmic Vision with funding being spread thinly?

The President. Do you have too many things competing?

Professor Southwood. The point of a competition is to have winners and losers; in fact, with space missions, historically, not too many things really lose, but they get strung out in time. As an example, two predecessors of what became *INTEGRAL* lost twice before that mission finally emerged in the form that it flew. Probably the most serious issue, to be discussed by the SPC in June, is the fact that if you do two medium missions, one after the other, how much competition have you introduced? Are we considering too few missions?

Dr. Owen. If there are, say, four missions still in competition in January, will STFC still find enough money to fund the studies that are required to build those missions, even though some of them will fall by the wayside?

Professor Southwood. It's not quite like that, and STFC have not done too badly in communicating with me where they feel their community priorities are with some of those missions. In some cases, they have clearly indicated that the UK cannot provide appropriate long-term funding, and I am shifting the work to Spain, Netherlands, and so on, and this is a productive way for Europe to operate. In fact *INTEGRAL* was a good example of this, since it was not highly prioritized in the UK and the work was shifted to Italy, France, and Germany. If we do it early on, we can manage it so that Europe as a whole benefits. I wish I could get the same level of communication out of some of the other funding agencies in Europe.

Dr. Krautter. I fully support this attitude. The UK does not have to be involved in all ESA missions — that's the point of collaboration. There are many countries involved; it is better for one country to do fewer missions well than to try to cover all missions.

Professor Monica Grady. I'd like to change the subject and address a question to all the panel members. It's a comment that has been made at every NAM I have been to over several years. I am looking at a panel of the finest, venerable, European manhood [laughter]; this afternoon we had a medal session when all but one of the recipients was another fine example of global manhood. When are we as professional scientists going to grasp the nettle and realize that we have a vast resource of womanhood? And how can we make our science more accessible to non-whites and to women? We have to be looking at options for flexible working, breaks in fellowships, and so on; it's not an easy problem, but it is one to which we have been paying lip service in this community for years. We have no fellowships which specifically address the needs of women returning to work in STFC; the Royal Society does, but none of the research councils. We also need to make our field more attractive for non-whites. [Applause.]

Professor Cruise. I support very strongly what Monica said. I chaired the RAS awards committee this year, and I ask people when responding to the RAS for requests for nominations to think carefully about nominating people who are not pale, male, and stale [laughter]. The universities have a rôle here too. A year or so ago, I was advised in a certain matter of the salary level of a first-rate woman professor at a university, and I knew roughly what the male salaries were; and I was shocked at the disparity. The universities need to step up and deal firmly with the problem of equal pay. Everybody needs to try hard to break through this problem. Those people who have had the opportunity to work with women returning from having a family will know that there are benefits to the group as well as to the women themselves.

The President. Thank you, Monica, for raising this. Certainly in terms of the graduate-student intake, about half of the intake is now women, but at the professorial level, it is a few percent, as you have stressed. We need to do something about this, but you're right, we are starting too little too late, and we need to do much more.

Professor Mason. I don't think I have much to add; I agree with what Monica said. When I was head of a university department not so many years ago, we had a ratio of 2:3 women to men. My experience was that this high ratio of women staff made a huge positive difference to the dynamics of the department, how it worked; and I want to increase the ratio of women in the field. It's a very hard problem, as we all know. We have to fix it collectively, and it is all about attitudes, and recognition of the problems that women face.

Dr. Gilmozzi. I am here representing the Director General of ESO; of course, two years ago, the ESO Director General was a woman. The solution starts with accepting the fact that there is a problem. At ESO, this is beginning to be the case; we are far from an ideal solution, but there have been studies of, for example, the problems of motherhood during fellowships, *etc*. This is being actively studied, with discussions taking place with staff, leading to proposals to modify the rules. There is still a long way to go, but realizing that the problem is there is the first step.

The President. Carole, do you have a point on this one?

Professor Jordan. Yes I do! [Laughter.] When I was on a certain committee, there was an example of one woman on the committee who had children and she asked if child-care expenses could be claimed back; and most of the rest of the committee decided this would be too much. I complained that one could claim expenses to park one's car, but not one's child!

Dr. M. M. Bisi. I am a young scientist working in California — I am not in the UK since there is no funding for it; I was lucky to get a job in the USA, since there are few in Europe. What are the plans of STFC to rectify the problem of keeping young scientists in the UK?

Professor Mason. It's a good point, and firstly I would say that mobility is important: I spent part of my career in California, and in many respects, I wish I were still there. [Laughter.] We should take advantage of opportunities for mobility. We want to make the UK a place where bright people want to come and live and work. This takes time, but the current government has invested strongly in science over the last ten years, and it is showing: for example, the

Diamond Light Source Facility is a beacon, and it impresses and attracts people. We need to extend that ethos to everything we do, to aim to be world-leading and attract others.

Professor P. A. Crowther. A year ago there was criticism of STFC's communications, both with international partners and with the community. It's good to hear from David Southwood that communication with ESA by STFC is much improved. I do have a concern from a community perspective that although STFC's website is much prettier than it was six months ago, we are still not quite getting the full picture: examples are the cancellation of *Clover*, and the announcement of the Ground-Based Review today. We seem to be getting only a partial picture in the communications from STFC. I'd like to be done out of a job — I'd like to see more information made more transparently from STFC.

Professor Mason. You're doing a great job: whenever I want to find anything, I go to your blog [laughter]. There is a serious issue here, and it is about transparency and openness: but there is a tension between openness and the timing of announcements. Government departments deal with this by clamping down on everything to prevent leaks; we have a culture where we talk to people, so one of us might talk to a friend and it appears on your blog! Is that good or bad communication? Do we want to clamp down on everything? That would imply much less interaction with the community. Terry [STFC Director of Communications], would you like to comment?

Mr. T. O'Connor. I agree with Paul that the website needs to be improved. The goal is to ensure that the website, which is our primary means of external communication, is clear and supposed to make sure you can find the information that is relevant. We are working to improve it and provide you with more information about what's going on; last year we accepted the message that we could have done better with regards to consultation. We are trying to do better, and we hope you are noticing the difference.

The President. The information flow is considerably better than it was a year ago, and Paul's website helps.

Professor J. Hough. Just a comment on what Monica said about fellowships: in fact, we are very flexible in STFC with fellowships with regard to any form of part-time working, and we are encouraging women to come back. You'll find in our adverts that we welcome returners to apply, and this coming year we intend to offer 6-month grants for people to study up to prepare for fellowship applications.

Professor T. Shanks. I wanted to ask Keith and Roberto about ESO discussions concerning a penalty charge on *VISTA*. There are rumours going around that diplomacy has been a bit up and down, and I wondered if you could tell us if there is anything the community can do to help.

Professor Mason. We are in discussions with ESO about this; it is a non-trivial issue. What we are concentrating on is getting *VISTA* up and running, and that is looking good, with the usual teething troubles one expects to have with a complex instrument; we hope that by local summer time we will be starting survey work with it. ESO is our observatory, and we want to be a good partner within ESO, but we recognize the value of *VISTA* and we wish to find a mutually acceptable way forward that supports European astronomy, and not get hung up on things that are backward looking.

Dr. Gilmozzi. These discussions are well above my pay grade; I can say that the work towards reaching the start of real science operations has been going on very actively, regardless of any discussions there might be at other levels, and the cooperation between the teams of *VISTA* and Paranal has been really very productive.

The President. It's time to bring everything to a close. Let me thank the panel who have answered the questions that have been put forward. [Applause.]

On behalf of the RAS and of you all, I would like to thank the NAM meeting organizers: the Local Organizing Committee, Hugh Jones, Janet Drew, Elias Brinks, Jim Hough, John Atkinson, Bob Chapman, and Mark Sarzi; the Scientific Organizing Committee, Janet Drew and her 15 colleagues; and all the many others, including the students, who have contributed to the success of this meeting. It has been a great JENAM and we thank you very much. [Applause.]

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 208: HD 3065, HD 40602, HD 134738, AND HD 216525

By R. F. Griffin
Cambridge Observatories

The binary natures of three of the stars discussed in this paper (HD 40602 being the exception) came to light in the course of the 'Clube Selected Areas' programme or its unpublished extension; they are probably (certainly in the case of HD 216525, which was observed by *Hipparcos*) giants. HD 3065, which is less than 5° from the North Celestial Pole, has an orbital eccentricity of 0·66 and period of 1285 days. HD 134738 has an eccentricity that is even higher, 0·78, whereas its declination is more than 90° lower, and as its period (341 days) is close to one year the sudden periastron passage occurs when the system is unobservable in several successive seasons, a circumstance that has created delay in the determination of its orbit. HD 216525 has a circular orbit with a period of only 16 days; it has a projected rotational velocity of 29 km s^{-1}, and could well be expected to exhibit RS CVn activity, although none has been reported. Finally, HD 40602 is a different sort of system, being an Am-type binary which was taken onto the observing programme in what was initially intended to be a 'service-observing' operation. It has proved to be double-lined, with a period of 61 days and an orbital eccentricity of almost 0·8, which is believed to be the highest yet known for a binary with a period less than 100 days.

Introduction

The only one of the four stars for which the writer is on record as having exhibited a previous interest is HD 134738. It is the very last one, among the 30 spectroscopic binaries identified in the first paper[1] on the 'Clube Selected Areas', to have its orbit published; the high eccentricity of nearly 0·8 and the orbital period of almost one year conspire to have delayed satisfactory phase coverage of the orbit. HD 40602 is an Am system that was 'bequeathed' to the author by Dr. J.-M. Carquillat, who had discovered its binary nature, upon his retirement. The duplicity of the other two stars treated in this paper came to light in the course of observations of an unpublished extension of the Clube Selected Areas programme.

HD 3065

At a declination above 85°, HD 3065 is nearer the Pole than any other star yet treated in this series, having a declination more than one degree above that of the previous highest, HD 83065, the subject of Paper 65[2]. (The difference of exactly 80 000 in the HD numbers is a remarkable coincidence!) In its direction from Polaris, it is nearly opposite the Pointers in the Plough.

The star is in an extension to Clube Area 3, which is[1] nominally centred at Galactic coördinates $l = 135°$, $b = +35°$, corresponding approximately to RA $9^{h}\ 30^{m}$, declination +80°. The extreme declination causes the Area to overlap the celestial pole, and results in its including HD 3065, which is nearly nine hours of RA away from the centre of the Area but is at a declination of 85°, where an hour of RA is little more than degree in angular terms. The star is almost at the maximum declination that can be observed with the Cambridge 36-inch reflector, in which the light travels northwards to reach the coudé focus. At declinations above 86° the light beam, after reflection successively from the primary and secondary mirrors and the coudé flat, is obstructed by running into the secondary mirror a second time.

It was not until the author came to write this paper and looked up the (very small) literature on HD 3065 that he realized that the star is in the area of sky covered by the well-known old Galactic star cluster NGC 188. It was in the context of an investigation of that cluster that photometry of the star was obtained by Sandage[3], who gave it the letter designation O within the cluster and found $V = 7^{m}{\cdot}95$, $(B-V) = 0^{m}{\cdot}96$, $(U-B) = 0^{m}{\cdot}70$. HD 3065 is in the outskirts of the NGC 188 field and is far too bright to be accepted as an actual member of the cluster, a conclusion that has been confirmed in astrometric investigations[4,5]. Its *HD* type is K0; the *Hipparcos* parallax is $0''{\cdot}00386 \pm 0''{\cdot}00071$, indicating a distance modulus of $7^{m}{\cdot}07 \pm 0^{m}{\cdot}4$ and thereby an absolute magnitude of about $+0^{m}{\cdot}9$. There does not appear to be any MK classification, but from the colour indices and the luminosity one might infer the type to be about G8/K0 IIIb; as seems to happen very generally with the stars that feature in this series of papers, the luminosity is decidedly smaller than corresponds to the 'ridge-line' of class III stars in the informative post-*Hipparcos* diagram of Keenan & Barnbaum[6], and lies in the area occupied by the supposedly helium-burning 'clump' stars.

The writer first measured the radial velocity of HD 3065 as a star in the enlarged Clube Area 3 in 1995; a second observation in 1997 July was discordant, and the new velocity was immediately confirmed by another measurement. When it was next measured only six weeks later, a further large change (17 km s^{-1}) had taken place; it transpired that a periastron passage in a highly eccentric orbit had occurred, and that that phase of the orbit merited much more frequent observations — which were duly accorded to it on subsequent occasions. The 12 initial observations, in 1995–98, were made with the Haute-Provence (OHP) *Coravel*[7]; with the commissioning of the analogous instrument in Cambridge in 1999 the observing programme was transferred to the home site, where a further 68 measurements have been accumulated. There was an 18-month interval in 1998/9 when neither of the *Coravel*s was available; the Dominion Astrophysical Observatory (DAO) in Victoria, B. C., kindly offered observing time on the spectrometer[8] at the coudé focus of the 48-inch reflector there, and five out of the total of seven DAO measurements of HD 3065 were made with it then.

Only when this paper was being drafted did its author become alerted to the fact that, in the course of observations made of NGC 188 with the collaboration of Dr. J. E. Gunn about 30 years ago, with the radial-velocity spectrometer[9] that we constructed for the 200-inch Palomar telescope*, HD 3065 had twice been observed under the alias 'NGC 188 O' and its spectroscopic-binary nature had thereby already been discovered. The two observations have been included at the head of Table I, where all the 89 available radial velocities are listed. In the solution of the orbit, which is illustrated in Fig. 1, they have all been given the same weight; the resulting elements are:

P	=	1285·31 ± 0·31 days	$(T)_2$	=	MJD 51980·8 ± 0·7
γ	=	−12·81 ± 0·05 km s^{-1}	$a_1 \sin i$	=	193·7 ± 1·1 Gm
K	=	14·57 ± 0·07 km s^{-1}	$f(m)$	=	0·1758 ± 0·0031 $M_\odot$
e	=	0·6588 ± 0·0025			
ω	=	301·9 ± 0·6 degrees	R.m.s. residual	=	0·39 km s^{-1}

The noteworthy features of the orbit are its high eccentricity and the large mass function; the latter demands a minimum mass of about 1·2 $M_\odot$ for the secondary if the primary is supposed to have a mass of 2 $M_\odot$. The secondary is hardly likely to be a white dwarf, with such a mass and with the orbit left so eccentric after the evolution of the present secondary as a giant, so it seems very likely that the companion is an F-type main-sequence star. In fact, it hardly takes the eye of faith to notice in Fig. 1 a distinct appearance of 'dragging' of the velocities towards the γ-velocity when the primary is within 5–6 km s^{-1} of that value; it is possible that the radial-velocity traces would show explicitly a weak secondary feature around the time of nodal passage, but regrettably no specific effort was made to verify that at the appropriate time.

HD 3065 appears in Famaey *et al.*'s tabulation[11] of K and M giants, and is there recorded as being a spectroscopic binary with a γ-velocity of –8·19 ± 0·30 km s^{-1}. The radial-velocity information in that tabulation was derived from the data base of OHP *Coravel* observations on file in Geneva, which includes those that were made with that instrument by the present writer. The γ-velocity given by Famaey *et al.* presumably represents the interpretation by those authors of the fragmentary material at their disposal. If the orbit given in the present paper is to be believed, the Famaey *et al.* γ-velocity is off by about 16 times its

*The radial-velocity observations made of stars in NGC 188 were not considered by the observers to be sufficiently numerous to support a discussion of the cluster, although they did, naturally, provide interesting information concerning membership and identifed a number of spectroscopic binaries. The Palomar data have, however, been subscribed to a major investigation, published[10] very recently, of radial velocities and spectroscopic binaries in NGC 188. Although that study does include orbits of a number of field stars, HD 3065 is not among them.

TABLE I

Radial-velocity observations of HD 3065

Except as noted, the sources of the observations (all equally weighted) are as follows: 1995–1998 — OHP Coravel; 1999–2008 — Cambridge Coravel

Date (UT)	*MJD*	*Velocity* km s⁻¹	*Phase*	*(O–C)* km s⁻¹
1979 Nov. 3·13*	44180·13	−21·2	$\bar{5}$·931	−0·1
1980 Oct. 24·17*	44536·17	−7·0	$\bar{4}$·208	−0·1
1995 Jan. 9·84	49726·84	−9·4	0·246	−0·9
1997 July 26·96	50655·96	−14·8	0·969	0·0
28·08	657·08	−14·3	·970	+0·2
Sept. 11·02	702·02	+2·8	1·005	+0·5
11·99	702·99	+2·2	·006	−0·4
13·07	704·07	+3·3	·007	+0·3
13·83	704·83	+3·3	·007	+0·1
Dec. 23·81	805·81	+1·5	·086	+0·1
24·75	806·75	+1·4	·087	+0·1
1998 Apr. 28·91	50931·91	−5·5	1·184	+0·3
July 9·07	51003·07	−8·5	·239	−0·2
24·08	018·08	−8·7	·251	0·0
1999 Apr. 6·21†	51274·21	−14·3	1·450	0·0
17·17†	285·17	−15·1	·459	−0·6
July 8·47†	367·47	−15·1	·523	+0·8
12·46†	371·46	−15·6	·526	+0·4
Nov. 3·22†	485·22	−17·0	·614	+0·8
Dec. 28·86	540·86	−18·4	·658	+0·2
2000 Jan. 8·79	51551·79	−17·9	1·666	+0·9
27·09†	570·09	−19·2	·680	−0·2
29·17†	572·17	−19·3	·682	−0·2
Feb. 19·79	593·79	−19·5	·699	−0·1
June 18·08	713·08	−21·4	·792	−0·3
July 20·10	745·10	−21·8	·817	−0·2
Aug. 2·11	758·11	−21·9	·827	−0·2
Sept. 4·09	791·09	−22·3	·852	−0·2
Oct. 6·06	823·06	−22·9	·877	−0·6
Nov. 1·96	849·96	−21·8	·898	+0·4
Dec. 1·93	879·93	−21·7	·921	0·0
15·89	893·89	−21·2	·932	−0·1
29·75	907·75	−20·5	·943	−0·4
2001 Jan. 6·85	51915·85	−19·8	1·949	−0·5
13·78	922·78	−17·9	·955	+0·5
25·80	934·80	−16·0	·964	+0·3
Feb. 9·78	949·78	−11·8	·976	+0·6
15·77	955·77	−10·4	·980	−0·1
23·80	963·80	−7·8	·987	−0·6
Mar. 3·78	971·78	−4·8	·993	−1·0
July 4·08	52094·08	+1·3	2·088	+0·1
Aug. 2·10	123·10	−1·3	·111	−0·4
11·11	132·11	−1·3	·118	+0·2
Sept. 30·02	182·02	−3·8	·157	+0·4
Oct. 19·02	201·02	−6·1	·171	−1·0
Nov. 9·93	222·93	−6·3	·188	−0·3
Dec. 1·89	244·89	−6·8	·205	0·0

TABLE I (*concluded*)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O–C) km s⁻¹*
2002 Jan. 1·83	52275·83	–8·9	2·230	–1·0
Feb. 5·79	310·79	–8·5	·257	+0·4
Mar. 1·79	334·79	–9·2	·275	+0·4
July 15·09	470·09	–12·4	·381	+0·3
Aug. 13·11	499·11	–13·2	·403	0·0
Sept. 5·04	522·04	–13·8	·421	–0·1
Oct. 4·08	551·08	–14·3	·444	–0·1
Nov. 4·02	582·02	–14·6	·468	+0·1
2003 Jan. 4·77	52643·77	–15·2	2·516	+0·6
Feb. 14·78	684·78	–16·1	·548	+0·3
July 14·08	834·08	–18·8	·664	–0·1
Aug. 9·10	860·10	–19·3	·684	–0·2
Sept. 11·03	893·03	–19·6	·710	0·0
Oct. 18·02	930·02	–20·7	·738	–0·6
Nov. 13·01	956·01	–20·9	·759	–0·4
Dec. 17·86	990·86	–20·9	·786	+0·1
2004 Aug. 8·13	53225·13	–15·1	2·968	+0·1
17·15	234·15	–12·6	·975	+0·1
31·06	248·06	–7·6	·986	0·0
Sept. 4·04	252·04	–5·8	·989	+0·2
10·06	258·06	–3·1	·994	+0·3
16·05	264·05	–0·7	·998	+0·2
21·06	269·06	+1·1	3·002	0·0
29·12	277·12	+3·8	·009	+0·1
Oct. 6·08	284·08	+5·1	·014	–0·2
19·01	297·01	+6·4	·024	–0·3
26·00	304·00	+7·0	·029	+0·2
Nov. 5·00	314·00	+6·5	·037	0·0
12·85	321·85	+5·7	·043	–0·3
26·85	335·85	+4·9	·054	0·0
Dec. 5·77	344·77	+4·1	·061	0·0
16·85	355·85	+3·5	·070	+0·4
26·83	365·83	+3·1	·078	+0·8
2005 Sept. 29·05	53642·05	–10·3	3·292	–0·1
Oct. 25·99	668·99	–11·3	·313	–0·5
Nov. 19·01	693·01	–11·7	·332	–0·3
Dec. 10·84	714·84	–11·2	·349	+0·6
2006 Jan. 4·74	53739·74	–12·7	3·368	–0·3
Sept. 20·09	998·09	–16·7	·569	+0·2
Oct. 21·94	54029·94	–17·1	·594	+0·3
Nov. 27·99	066·99	–17·5	·623	+0·4
2008 Feb. 15·81	54511·81	–14·8	3·969	0·0

*Observed with Palomar 200-inch telescope.
†Observed with DAO 48-inch telescope.

listed standard error (18 times when account is taken of the fact that the OHP measurements have here been adjusted by +0·8 km s^{-1}).

HD 40602

HD 40602 is an eighth-magnitude metallic-lined A star, to be found in Orion 2° north-following Betelgeuse. It came suddenly to the writer's attention on

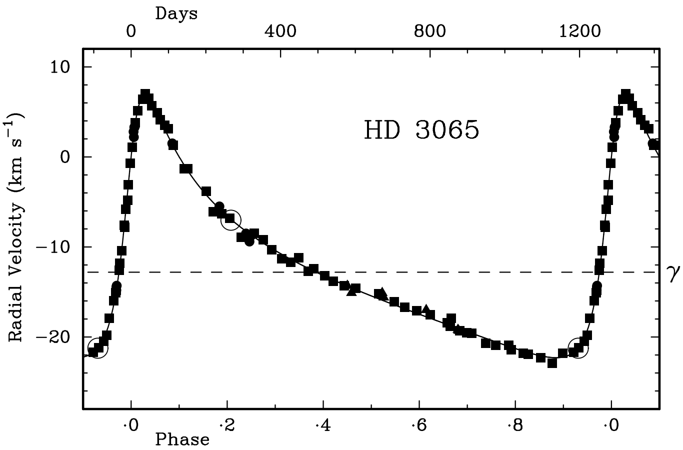

FIG. 1

The observed radial velocities of HD 3065 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. Measurements plotted as filled circles were made with the OHP *Coravel*, while the filled squares are Cambridge *Coravel* observations and the filled triangles represent DAO velocities. Two observations that were found to have been made of HD 3065 under the alias 'NGC 188 O' with the 200-inch Palomar telescope long ago are plotted as large open circles and are seen to support the orbit solution 'perfectly'.

2007 March 19, when Dr. J.-M. Carquillat requested him to observe it, as will be described more particularly below. The star is *listed* in a photometric catalogue by Mendoza[12] — but no photometry is given for it there! Actual photometry was later given by Feinstein[13], as $V = 7^{m}{\cdot}90$, $(B-V) = 0^{m}{\cdot}37$, $(U-B) = 0^{m}{\cdot}12$. The *HD* type is A2. Slettebak & Nassau[14] were the first to recognize the metallic-lined nature of HD 40602. They identified it provisionally in an initial objective-prism survey made at 280 Å mm^{-1} at Hγ with a 4° prism on the *Burrell Schmidt*[15] of the Warner & Swasey Observatory, and followed it up with a slit spectrogram (104 Å mm^{-1} at Hγ) obtained with the Perkins reflector, which at the time was the original 69-inch instrument in Delaware*. They gave the *K*-line type as A5 and the metallic-line type as F2.

Grenier *et al.*[16] obtained, mainly for radial-velocity purposes, five spectra of HD 40602 with the *Marly* spectrograph[17] on the 1·2-m reflector at OHP. They noted it as having a variable velocity, and gave a mean of $+36{\cdot}4 \pm 9{\cdot}2$ km s^{-1}, but they did not provide the individual results. They gave the spectral type as "A4mA7F4". It is regrettable that there does not seem to be any universally received way of specifying metallic-line types. It is the writer's understanding that 'A4m', by itself, would designate a metallic-lined A star whose *hydrogen* type — which seems to be the nearest to characterizing an Am star physically — is A4. But then the *K*-line type should be earlier and the metallic-line type later than that. Perhaps the Grenier *et al.* type is intended to mean what would be represented more explicitly by the notation kA4hA7mF4.

*The 69-inch reflector, which was largely funded by Prof. Hiram Perkins (1833–1923) of Ohio Wesleyan University, was the third-largest telescope in the world, after the Mount Wilson 100-inch and the DAO 72-inch, when it came into operation in 1925. Owing to the poor climate and deteriorating sky conditions in Delaware, the telescope was moved to the Lowell Observatory site in Arizona in 1961. In 1964 the original 69-inch mirror was replaced by a 72-inch one; the old mirror is on public display at the Perkins Observatory, whose functions are now largely educational. The ownership of the 72-inch telescope passed to the Lowell Observatory in 1998.

The star then featured in the comprehensive investigation[18], undertaken by Carquillat and his collaborators with the OHP *Coravel*, of the radial velocities of Am stars. They obtained eight measurements of it in 1992/3, showing variation in the range +10 to +30 km s^{-1}, and three more on consecutive nights in 2004 when the velocity was near +35. When they wished, on account of Dr. Carquillat's impending retirement, to draw a line under the programme, and they had had no further opportunity to make progress on HD 40602, they appealed to the present writer for fresh observations, as described in the second sentence of this section. The appeal was made very late in the observing season, but in principle there was still time to confirm the very short orbital period that the then-existing observations might have suggested. The star gives a very shallow and broad dip in radial-velocity traces, so was not easy to measure, but with sufficient integration time a result could be obtained. Unusually fine spring weather did indeed allow a good series of observations to be made, but what they showed was a monotonically increasing radial velocity, rising far beyond anything that had been seen previously. They were pursued as long as possible into the evening twilight, and were even extended by a further two days by a spectrum kindly obtained at the DAO by Dr. R. E. M. Griffin. They created at the time a great puzzle, because they showed an acceleration away from what certainly appeared to have been the normal velocity of the star, and thereby led to the expectation of an imminent periastron passage in a highly eccentric orbit, heralding an abrupt descent of the velocity. The rise in velocity did indeed slow down, but then instead of a dramatic reversal it seemed to accelerate anew! — and at that juncture the observing season came to an enforced end, with the Cambridge telescope reaching the westerly hour-angle limit represented by a permanent physical obstruction (the floor!) just at the time that it became dark enough to start observing.

Before the ensuing season started, the origin of the star's apparent misbehaviour dawned upon the observer — it must be double-lined. For much of the short time that the object had been under observation the trace must have been a blend of the two components, with the measured velocity favouring the stronger primary dip. A time of seemingly enhanced change occurred as the two dips drew apart, and the measurement began to refer to the primary alone instead of to the blend, and then the rate of change fell again when it was just the primary that was being measured. There was keen anticipation of the new observing season, when the period would become apparent and the evidently extreme orbital eccentricity would be defined. Initial observations proved to be in the long phase of mutual blending, but then after a month a dramatic periastron passage was witnessed; the expected secondary dip was indeed seen and measured as a separate entity, separated in fact by more than 200 km s^{-1} from the primary on the night of nodal passage.

There is now a total of 65 radial-velocity observations, of which 31 have yielded measurements of both components (sometimes in separate integrations); four others (including the one obtained at the DAO) have measured the primary alone. Fig. 2 shows a radial-velocity trace obtained with the Cambridge *Coravel* at a double-lined phase; it is about as good a trace as can be obtained of the star with that instrument. The profiles of the two dips being known from such traces, they could be specified in the reductions of traces in which the dips are blended together; in that way, although the apportioning of the dip became unreliable where the blending was very close, twin velocities have been measured from blends over about half of the interval (which in fact lasts for most of the orbital period) when the dips are unresolved. The traces obtained

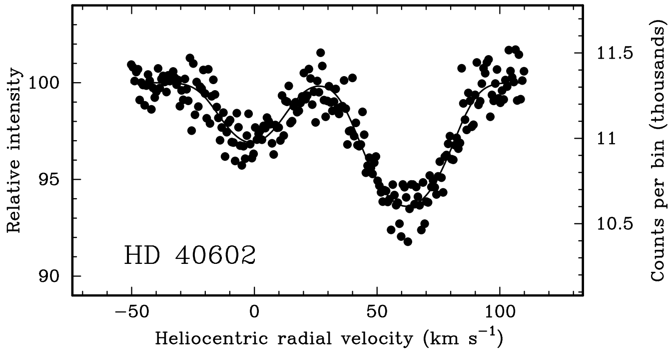

FIG. 2

Radial-velocity trace of HD 40602, obtained with the Cambridge *Coravel* on 2009 February 10 and illustrating the unequal double lines.

before the system was recognized as double-lined could not be treated in that way, lacking adequacy in both S/N ratio and scan width. All the available velocities are set out in Table II, in which the usual adjustment of +0·8 km s^{-1} has been made to the OHP data and an estimated one of −0·8 km s^{-1} to the Cambridge observations. Where the velocities change very rapidly just after the nodal passage, timings are given in Table II to an extra decimal place; in such cases where, in addition, observations of the two dips were made consecutively (instead of simultaneously in a scan wide enough to include them both at once) they are necessarily recorded on separate lines. To equalize the variances of the components' velocities, those of the secondary have been weighted ¹⁄₁₂. The question of the weighting of the OHP velocities does not arise, because all are blended and so cannot be utilized in the solution; the single DAO measure has been somewhat arbitrarily assigned half-weight. The orbital solution is shown in Fig. 3, and its elements are:

P	=	60·6378 ± 0·0018 days*	$(T)_6$	=	MJD 54515·210 ± 0·012
γ	=	+34·55 ± 0·21 km s^{-1}	$a_1 \sin i$	=	29·95 ± 0·19 Gm
K_1	=	59·60 ± 0·31 km s^{-1}	$a_2 \sin i$	=	36·4 ± 0·4 Gm
K_2	=	72·5 ± 0·8 km s^{-1}	$f(m_1)$	=	0·292 ± 0·006 $M_\odot$
q	=	1·217 ± 0·015 = (m_1/m_2)	$f(m_2)$	=	0·526 ± 0·019 $M_\odot$
e	=	0·7980 ± 0·0017	$m_1 \sin^3 i$	=	1·75 ± 0·05 $M_\odot$
ω	=	37·6 ± 0·5 degrees	$m_2 \sin^3 i$	=	1·434 ± 0·029 $M_\odot$

R.m.s. residual (unit weight) = 0·93 km s^{-1}

*The 'true' period (in the rest-frame of the system) is 60·6308 ± 0·0018 days. It differs from the observed value by 3·8 standard deviations.

The most notable fact about the orbit is, of course, its high eccentricity, particularly in association with the rather short period. Abt[19] quite recently remarked that, among binaries with logarithmic periods (days) in the range 1·5–2, *i.e.*, periods of 32–100 days, there was none with $e > 0·8$. In fact the record-holder appeared[20] to be HD 111306, whose orbit was determined in

TABLE II

Radial-velocity observations of HD 40602

Except as noted, the sources of the observations are as follows:
1992–2004 — Haute-Provence Coravel; 2007–2009 — Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *Prim.* km s⁻¹	*Sec.* km s⁻¹	*Phase*	*(O–C)* *Prim.* km s⁻¹	*Sec.* km s⁻¹
1992 Dec. 14·06	48970·05	+29·6		$\overline{86}$·553	—	—
1993 Jan. 13·03	49000·03	+27·9		$\overline{85}$·047	—	—
Nov. 26·01	317·01	+11·2		$\overline{80}$·275	—	—
27·07	318·07	+19·6		·292	—	—
28·05	319·05	+15·2		·308	—	—
29·00	320·00	+20·6		·324	—	—
Dec. 2·02	323·01	+22·8		·374	—	—
3·06	324·05	+28·2		·391	—	—
2004 Dec. 4·00	53343·00	+33·2		$\overline{14}$·669	—	—
5·03	344·03	+35·8		·686	—	—
6·02	345·02	+35·0		·702	—	—
2007 Mar. 21·85	54180·85	+27·8		0·486	—	—
25·82	184·82	+31·5		·551	—	—
26·86	185·86	+31·0		·569	—	—
27·85	186·85	+31·9		·585	—	—
31·85	190·85	+33·7		·651	—	—
Apr. 3·86	193·86	+34·1		·700	—	—
4·83	194·83	+35·2		·716	—	—
5·85	195·85	+35·6		·733	—	—
6·83	196·83	+36·8		·749	—	—
7·84	197·84	+36·5		·766	—	—
8·82	198·82	+38·2		·782	—	—
9·82	199·82	+39·8		·799	—	—
10·83	200·83	+41·5		·815	—	—
11·83	201·83	+44·0		·832	—	—
12·83	202·83	+46·9		·848	—	—
14·83	204·83	+57·7	—	·881	+0·3	—
15·83	205·83	+61·0	—	·898	−0·6	—
17·18*	207·18	+67·7	—	·920	−1·3	—
Sept. 15·17	358·17	+23·9		3·410	—	—
30·16	373·16	+33·5		·657	—	—
Oct. 5·19	378·19	+36·1		·740	—	—
18·21	391·21	+89·2	—	·955	+0·4	—
19·04	392·04	+102·9	−46·4	·969	+0·5	+1·6
20·06	393·06	+125·5	−79·3	·986	−0·6	−2·5
21·052	394·052	+111·8	—	4·002	−0·5	—
21·072	394·072	—	−58·8	·002	—	−0·4
21·214	394·214	+102·2	—	·005	+1·5	—
22·07	395·07	+47·9	+16·1	·019	−1·7	−0·1
Nov. 1·17	405·17	+15·1	+62·0	·185	+1·4	+2·1
3·20	407·20	+14·2	+54·9	·219	−0·5	−3·8
8·10	412·10	+19·1	+53·7	·300	+1·7	−1·7
9·13	413·13	+17·4	+50·1	·317	−0·6	−4·6
12·13	416·13	+18·5	+46·6	·366	−1·3	−6·0
16·11	420·11	+23·1	+44·5	·432	+0·9	−5·0
17·07	421·07	+23·9	+44·7	·447	+1·0	−4·1
Dec. 11·16	445·16	+49·9	+17·6	·845	−0·6	+2·4
13·04	447·04	+57·5	+16·8	·876	+1·3	+8·6
17·03	451·03	+78·9	−20·0	·942	−0·4	−0·1

TABLE II (*concluded*)

Date (UT)		MJD	Velocity Prim. km s⁻¹	Velocity Sec. km s⁻¹	Phase	(O–C) Prim. km s⁻¹	(O–C) Sec. km s⁻¹
2008 Feb.	11·00	54507·00	+53·4	+10·4	5·865	−0·5	−0·6
	11·93	507·93	+55·2	+5·1	·880	−1·9	−2·0
	15·93	511·93	+82·6	−27·4	·946	+0·6	−4·2
	16·93	512·93	+95·6	−38·8	·962	+0·2	+0·7
	17·91	513·91	+116·2	−62·7	·979	+0·5	+1·5
	18·79	514·79	+130·0	−82·3	·993	−1·8	+1·5
	26·90	522·90	+12·8	+64·7	6·127	+0·1	+3·5
Oct.	17·20	756·20	+110·2	−59·4	9·974	+0·7	−2·8
	19·080	758·080	+97·4	—	10·005	−0·4	—
	19·100	758·100	—	−40·3	·006	—	+0·3
	19·215	758·215	+88·1	−31·3	·007	+0·3	−1·1
	22·19	761·19	+17·4	+51·4	·057	+0·1	−4·2
	25·14	764·14	+13·6	+57·3	·105	+0·8	−3·8
	28·19	767·19	+12·7	+58·0	·155	−0·3	−2·7
2009 Feb.	10·96	54872·96	+62·8	−0·4	11·900	+0·6	−1·4
	11·94	873·94	+67·5	−2·6	·916	+0·1	+2·8
	13·96	875·96	+85·5	−30·6	·949	+1·2	−4·6
	16·93	878·93	+125·6	−77·2	·998	+0·9	−2·0

*Observed by Dr. R. E. M. Griffin with a CCD at the DAO 48-inch telescope.

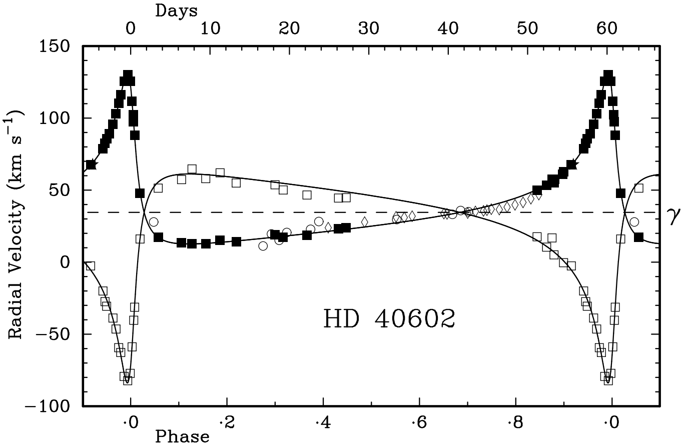

FIG. 3

The observed radial velocities of HD 40602 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. The orbit is computed from the Cambridge observations plotted as squares, filled for the primary and open for the secondary, plus one DAO CCD measurement of the primary, plotted as a five-pointed star (partly hidden). Open circles and diamonds represent measurements made at OHP by Carquillat and collaborators, and at Cambridge by the author, respectively, of blended traces that were reduced as single-lined and not taken into account in the solution of the orbit. The manner in which the blending rather suddenly becomes 'unstuck' at a phase about days 52–54 — a very confusing event before the character and orbit of HD 40602 were understood — is well illustrated.

Paper 173[21] of this series, with $e = 0{\cdot}779$ at $P = 61{\cdot}5$ days. That has now been supplanted by the orbit determined here for HD 40602.

Further discussion of HD 40602 is somewhat compromised by the difficulty of quantifying the effects of the metallic-line peculiarity, particularly in a binary system. The colour indices are likely to be redder than those associated with normal A/F stars on account of the extra absorption by metallic lines, and perhaps also because of actual reddening of interstellar origin, the system being at low Galactic latitude (+7°) and sufficiently distant for appreciable interstellar absorption to be possible. The parallax determined by *Hipparcos* is $0''{\cdot}00566 \pm 0''{\cdot}00120$, corresponding to a distance in the neighbourhood of 150–200 pc and a distance modulus of about $6^{m}{\cdot}25 \pm 0^{m}{\cdot}5$; that modulus leads in turn to an absolute magnitude of about $+1^{m}{\cdot}7 \pm 0^{m}{\cdot}5$, before allowance for any interstellar dimming.

A normal A star with the hydrogen type of A7 that we think Grenier *et al.*[16] intended for HD 40602 has[22] a tabular absolute magnitude of $+2^{m}{\cdot}2$, which may be expected to be enhanced by about $0^{m}{\cdot}3$ by the supplementation afforded by the considerably fainter secondary. That is seen to agree, as nearly as the accuracy of the information permits, with the absolute magnitude found from the parallax. The ratio of dip areas in radial-velocity traces such as that shown in Fig. 2 is 1 to 0·36 and corresponds arithmetically to a Δm of $1^{m}{\cdot}1$; the fainter star, however, can be expected to have substantially stronger spectral lines — unless indeed it is not a metallic-line star like the primary — and therefore to give a dip that is disproportionately strong in relation to the luminosity of the star producing it. The actual Δm (in the B band, which approximates to the wavelength region utilized by the *Coravel*) is therefore likely to be as much as $1^{m}{\cdot}4$ or even $1^{m}{\cdot}5$, although the difference in the V band will be somewhat less owing to the difference to be expected in the colour indices of the two stars.

The mass ratio of 1·217 is quite accurately determined, thanks to the enormous velocity amplitudes in the very eccentric orbit. The logarithm of the ratio is 0·085; armed with that value we can use Andersen's logarithmic graph[23] of the relationship between well-determined stellar masses and $(B-V)$ colour indices to estimate the differences in colour or spectral type between the components. As it happens, colour indices run rather linearly with spectral type in the relevant range of A5 to G5, and the general drift of masses down that part of the main sequence shows a change of –0·013 in $\log(m)$ per spectral sub-type or –0·05 per $0^{m}{\cdot}1$ in $(B-V)$. Thus we might deduce a difference in types of about 6 sub-types and a difference of about $0^{m}{\cdot}17$ in colour index. Inserting that last value into the discussion of the previous paragraph leads finally to an estimated ΔV of about $1^{m}{\cdot}3$ between the components of HD 40602.

In discussing the mass *ratio* we have not referred to the absolute values of the masses. They are, of course, not determinable here — what we have obtained from the orbit are the minimum values, which are the true masses multiplied by the unknown factor $\sin^3 i$. Those values appear to be slightly on the low side of the absolute masses for stars of the putative hydrogen types, which to form a combined impression of an A7 star would need to be A6 + F2 or possibly A5 + F1. They hardly allow, however, the $\sin^3 i$ factor to be as low as 0·9, or, therefore, $\sin i$ to be as low as 0·97, and so appear to set a lower limit of about 75° to the orbital inclination. The mean separation, projected onto the line of sight, of the component stars ($(a_1 + a_2)\sin i$ in the informal table above) is about 66 Gm, but at the time of conjunction shortly (0·68 days) after periastron it is only about 20 Gm. The stellar radii, according to tabular values for the spectral types, must be about 1·7 and 1·4 $R_\odot$, giving a sum of about 3·1 $R_\odot$ or say

2·2 Gm, so eclipses would occur if $\cot i \lesssim 2{\cdot}2/20$, *i.e.*, $i \gtrsim 84°$. It is not apparent from the *Hipparcos* 'epoch photometry' that any eclipses were observed, but there would be some interest in photometric monitoring of the system now that the exact times of conjunction can be predicted.

Although the dips seen in radial-velocity traces of HD 40602 are very shallow, they do allow estimates to be made of the projected rotational velocities of the components; the mean values are 23 km s^{-1} for the primary star and 20 for the secondary. Since $\sin i$ is so nearly unity, the same values can be taken as the actual equatorial velocities; then, if the stellar radii suggested in the last paragraph above are accepted, they lead to rotational periods of about 3·7 and 3·5 days, respectively. For pseudo-synchronous rotation[24] at the high eccentricity of the HD 40602 orbit, the rotational period is shorter than the orbital one by a factor of 12·4, making it 4·9 days. The similarity to the proposed actual periods, derived without any effort (even concealed!) being made to bring the numbers into coincidence, is suggestively close, and one cannot help considering how they could be brought together. It is, of course, not axiomatic that the system *should* be pseudo-synchronized, but it seems worthwhile at least to entertain such an hypothesis: the periastron separation of the components is analogous to that in a *circular* orbit with a period of only five days, in which captured rotations would be the norm. The writer is not willing to accept that the 'observed' $v \sin i$ values could be over-estimated by about one-third, and would prefer to believe instead that the estimates of the stellar radii should be increased by about that amount. That would have the effect of increasing the putative luminosity of the system by about $0^{m}{\cdot}6$, to about $+1^{m}{\cdot}3$ — still well within the range deduced from the parallax even without allowance for interstellar absorption. The larger radii would need to be explained either in terms of the stars being constituted like 'normal' A stars of earlier types than those with which we have credited them, or else in terms of incipient evolution that has lifted them somewhat above the main-sequence luminosity. (In view of the disparity in masses, the latter explanation could apply only to the primary, and so is less attractive.) An additional small consequence of increased radii would be that the limiting orbital inclination above which there would have to be eclipses would be reduced from 84° to 82°.

HD 134738

The 'Clube Selected Areas' programme was begun as long ago as 1967 with the then entirely novel photoelectric radial-velocity spectrometer at the coudé focus of the Cambridge 36-inch reflector, with the intention of providing systematic radial-velocity material for an investigation of Galactic structure. Sets of stars in fields spaced at every 45° in Galactic longitude and all at ±35° in Galactic latitude were selected by Dr. S. V. M. Clube for the writer to observe; the selection criteria were that all the stars should be classified as having spectral type K0 and be within half a magnitude of $9^{m}{\cdot}0$ photovisual in the *Henry Draper Catalogue*[25]. The first set of results of the observations, embracing the ten (out of the total of 16) Areas accessible to the Cambridge telescope, was published[1] in 1986. It gave the radial velocities of 406 stars, of which 30 were identified as being spectroscopic binaries; 13 of the 30 had already by that time been the subject of published orbits. Since then, orbits have been presented for all the rest of the 30, with the sole exception of HD 134738. In fact, literally half of the papers listed for HD 134738 in the *Simbad* bibliography (there are only four in total!) are ones in the present series: in 2000, when the 28th orbit was given[26], HD 134738 was identified as one of the two that were still outstanding, and in 2004 there was expressed the "hope to present the orbit of HD 134738 before long"[27]. That hope is redeemed here, provided a somewhat charitable elasticity is ascribed to the expression "before long"! It may also be noted, for completeness, that in 1986 the binary natures of four other stars, identified in the paper[1], remained in doubt; orbits have since been published for three of them, but the fourth, HD 218716, having been kept under quasi-annual observation ever since, is no longer regarded as a binary.

What has delayed the presentation of this orbit is the combination of a period close to one year and a high eccentricity — making it desirable to obtain relatively intensive observations over a small interval of phase — with an inconveniently low declination (–6°) which greatly curtails the observational accessibility of the star and thus the length of the observing season.

Very little is known about HD 134738, a star to be found about 4° north-preceding β Librae, apart from the radial-velocity data provided here. There is no ground-based photometry nor any MK classification, but we are indebted to *Tycho* for the photometry, $V = 8^{m}{\cdot}86$, $(B-V) = 1^{m}{\cdot}11$. The negative (though none too reliable) *Tycho* parallax and the modest proper motion suggest that the star is a giant, in which case it could be supposed from its colour to be of type K0 or K1.

The first two radial-velocity observations, in 1975 and 1977, yielded results of +63 and +78 km s^{-1}, marking the star out as an obvious binary. Subsequent observations, however, were not as high as +78, and in the 1980s years and years went by when, despite reasonable assiduity on the part of the observer, only values in the +50s seemed to be obtainable. As time went on, it was recognized that the period must be close to one year and that, throughout the observing season, one was seeing relatively uninteresting phases of the orbit; eventually, however, from 1989 onwards, signs of the sudden annual maximum were observed. The period is actually 341 days, so the date of the maximum retrogrades around the calendar by 24 days each year, in a cycle that takes about 15 years. In retrospect it can be seen how, in 1977 when the velocity of +78 km s^{-1} — which has proved to be right on the peak of the velocity curve — was observed, the maximum was within the quite restricted observing season, but then during the 1980s it was not. Just when it became accessible again, in the early '90s, the observer was entirely dependent upon observing runs at OHP, which happened not to coincide sufficiently well with the critical phases. Thus, for a really satisfactory coverage of the orbit it was necessary to have patience for a further 15-year cycle; it is only during the last few years that the velocity maximum has again come at a time of year when the star can be observed, and the peak value has at last been seen again in four of the last five seasons.

The effort that has been expended to obtain properly distributed observations of the HD 134738 orbit has resulted in the unusually large number of 139 radial velocities being accumulated. They are given in Table III. There are 44 obtained with the original spectrometer at Cambridge, 35 with the OHP *Coravel*, and 51 with the Cambridge *Coravel*. In addition, five observations were made with the DAO spectrometer[8], three at ESO, and one at Palomar. The OHP and ESO velocities have received the usual adjustment of +0·8 km s^{-1}, and those made at Cambridge — both with the original spectrometer and with the *Coravel* — have been adjusted by –0·5 km s^{-1} on an empirical basis to bring them into systematic agreement with the OHP ones. Previous experience of the distinct colour dependence of the *Coravel* zero-point would not lead to an expectation that such a large change would be needed to the Cambridge observations, but

TABLE III

Radial-velocity observations of HD 134738

Except as noted, the sources of the observations are as follows:
1975–1991 — original Cambridge spectrometer (weighted ⅛ in orbital solution);
1992–1999 — OHP Coravel (weight 1); 2000–2009 — Cambridge Coravel (weight 1)

Date (UT)	MJD	Velocity km s⁻¹	Phase	(O–C) km s⁻¹
1975 June 25·93	42588·93	+63·4	0·921	+2·5
1977 June 1·96	43295·96	78·2	2·993	0·0
1979 Mar. 12·12	43944·12	57·8	4·893	−1·2
May 18·97	44011·97	55·3	5·092	+1·2
1980 Jan. 2·28	44240·28	55·8	5·761	+0·7
Feb. 23·20	292·20	59·6	·913	−0·7
May 9·01	368·01	52·0	6·136	−1·0
1981 Mar. 1·17	44664·17	75·6	7·004	−0·7
Apr. 18·05	712·05	53·1	·144	+0·2
May 5·04	729·04	53·5	·194	+1·1
19·35*	743·35	52·9	·236	+0·6
June 1·00	756·00	50·6	·273	−1·6
1982 Jan. 21·23	44990·23	66·5	7·960	0·0
Mar. 4·16	45032·16	52·4	8·082	−2·1
May 4·04	093·04	51·2	·261	−1·0
23·98	112·98	52·2	·319	0·0
1983 Feb. 3·59†	45368·59	56·0	9·069	+0·6
23·17	388·17	52·2	·126	−0·9
Mar. 15·16	408·16	51·3	·185	−1·2
Apr. 24·06	448·06	52·0	·302	−0·2
May 15·98	469·98	50·9	·366	−1·4
June 18·94	503·94	52·7	·465	+0·1
1984 Jan. 2·28	45701·28	58·2	10·044	−0·1
Apr. 14·06	804·06	53·1	·345	+0·8
24·05	814·05	54·0	·374	+1·7
May 14·01	834·01	54·9	·433	+2·4
1985 Feb. 17·52†	46113·52	53·2	11·252	+1·0
May 31·99	216·99	51·5	·555	−1·6
1986 Apr. 10·13	46530·13	52·7	12·473	0·0
May 6·03	556·03	52·6	·549	−0·5
19·03	569·03	52·6	·587	−0·7
1987 Mar. 4·19	46858·19	52·5	13·435	0·0
May 8·01	923·01	54·0	·625	+0·4
June 3·97	949·97	53·6	·704	−0·7
July 17·99‡	993·99	56·7	·833	+0·1
1988 Feb. 1·57†	47192·57	52·5	14·415	+0·1
Mar. 12·14	232·14	53·0	·531	0·0
Apr. 13·07	264·07	54·0	·625	+0·4
June 2·99	314·99	54·6	·774	−0·7
1989 Feb. 23·40‡	47580·40	53·1	15·552	0·0
Mar. 27·16	612·16	+53·6	·645	−0·1

TABLE III (*continued*)

Date (*UT*)			*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O–C)* *km s*$^{-1}$
1989	Apr.	30·09	646·09	+55·3	15·744	+0·5
	May	26·99	672·99	57·4	·823	+1·1
	June	1·99	678·99	57·0	·841	+0·2
		18·94	695·94	59·3	·891	+0·5
	July	4·91	711·91	63·9	·937	+1·2
		11·91	718·91	66·2	·958	+0·1
1990	Jan.	27·21	47918·21	53·7	16·542	+0·7
	Feb.	13·37‡	935·37	52·9	·592	−0·4
	Apr.	5·06	986·06	53·1	·741	−1·7
	May	26·99	48037·99	62·1	·893	+3·1
	July	5·92	077·92	72·1	17·010	+0·2
1991	Jan.	27·24	48283·24	53·6	17·612	+0·1
	May	9·05	385·05	60·2	·911	+0·1
		23·03	399·03	66·7	·952	+1·9
		25·01	401·01	64·1	·957	−1·9
	June	10·99	417·99	72·7	18·007	−1·4
		12·98	419·98	71·9	·013	+1·9
1992	Jan.	16·24	48637·24	54·3	18·650	+0·5
	Feb.	27·54†	679·54	54·7	·774	−0·6
	Apr.	22·13	734·13	61·5	·934	−0·7
		25·12	737·12	63·5	·943	+0·1
		30·09	742·09	65·7	·957	−0·2
	June	20·98	793·98	53·4	19·109	−0·1
		27·00	800·00	52·8	·127	−0·3
	Aug.	13·87	847·87	52·4	·267	+0·2
	Dec.	18·24	974·24	53·6	·638	−0·1
1993	Feb.	14·20	49032·20	55·3	19·807	−0·7
	Mar.	18·18	064·18	59·4	·901	0·0
	July	6·94	174·94	52·2	20·226	−0·1
		11·97	179·97	52·6	·241	+0·4
	Dec.	27·22	348·22	55·4	·734	+0·7
1994	Jan.	8·22	49360·22	55·1	20·769	−0·1
	Feb.	18·18	401·18	58·4	·889	−0·3
	May	1·08	473·08	53·6	21·100	−0·2
	July	29·86	562·86	52·0	·363	−0·3
1995	Jan.	7·24	49724·24	56·1	21·836	−0·6
	June	2·01	870·01	52·3	22·263	+0·1
1996	Jan.	1·24	50083·24	58·6	22·888	−0·1
	Mar.	30·15	172·15	53·2	23·149	+0·4
	Dec.	25·22	442·22	62·0	·941	−1·1
1997	Jan.	26·21	50474·21	59·9	24·034	−0·4
	Apr.	1·13§	539·13	51·5	·225	−0·8
	Dec.	22·22	804·22	76·6	25·002	−0·7
		24·24	806·24	73·8	·008	0·0
		25·23	807·23	71·9	·010	+0·2
1998	May	3·10	50936·10	52·9	25·388	+0·5
	July	9·97	51003·97	53·9	·587	+0·6
1999	July	13·27†	51372·27	53·5	26·667	−0·4
2000	Mar.	4·20	51607·20	52·1	27·355	−0·2
	May	1·03	665·03	+53·5	·525	+0·6

TABLE III (*concluded*)

Date (UT)			MJD	Velocity km s⁻¹	Phase	(O–C) km s⁻¹
2001	May	12·05	52041·05	+53·2	28·627	−0·4
2002	Apr.	7·11	52371·11	52·8	29·595	−0·5
	June	18·93	443·93	55·9	·808	−0·1
2003	Feb.	18·20	52688·20	53·8	30·524	+0·9
	Apr.	19·08	748·08	53·7	·700	−0·6
	June	12·93	802·93	58·0	·860	+0·5
2004	May	19·02	53144·02	57·2	31·860	−0·3
	June	16·93	172·93	63·9	·945	+0·2
		25·92	181·92	69·7	·971	0·0
		27·93	183·93	72·0	·977	+0·1
		28·91	184·91	73·3	·980	+0·2
2005	May	23·00	53513·00	63·3	32·942	+0·1
		27·99	517·99	65·8	·956	0.0
		30·99	520·99	67·9	·965	0·0
		31·98	521·98	68·5	·968	−0·2
	June	6·97	527·97	75·6	·986	+0·1
		8·95	529·95	77·9	·991	+0·1
		9·93	530·93	78·3	·994	−0·1
		10·92	531·92	78·9	·997	+0·4
		13·92	534·92	75·2	33·006	+0·3
		21·94	542·94	62·3	·029	+0·6
		22·93	543·93	60·7	·032	−0·2
		26·94	547·94	57·8	·044	−0·4
		27·94	548·94	58·2	·047	+0·5
2006	May	6·08	53861·08	67·3	33·962	+0·2
		9·99	864·99	71·2	·973	+0·7
		11·03	866·03	71·5	·977	−0·2
		16·03	871·03	77·5	·991	−0·2
		17·02	872·02	78·3	·994	−0·1
		21·97	876·97	73·2	34·009	+0·1
		26·97	881·97	64·5	·023	+0·3
	June	2·98	888·98	57·6	·044	−0·7
2007	Apr.	4·13	54194·13	62·3	34·938	−0·5
		10·09	200·09	66·3	·956	+0·7
		16·09	206·09	70·6	·973	+0·1
		19·07	209·07	73·8	·982	−0·2
		30·06	220·06	68·8	35·014	−0·3
	May	1·06	221·06	67·2	·017	−0·1
		2·04	222·04	66·0	·020	+0·3
		8·03	228·03	60·0	·038	+0·5
		15·00	235·00	56·2	·058	−0·1
		19·00	239·00	54·9	·070	−0·4
		23·04	243·04	54·4	·082	−0·2
	June	20·95	271·95	52·8	·166	+0·2
2008	Mar.	31·15	54556·15	78·2	35·999	+0·1
2009	Mar.	6·21	54896·21	78·4	36·996	−0·2
		9·15	899·15	75·8	37·005	+0·2
		21·16	911·16	+58·5	·040	−0·5

*Observed with Palomar 200-inch telescope (wt. ½).
†Observed with DAO 48-inch telescope (wt. ½).
‡Observed with ESO *Coravel*.
§Observed with Cambridge *Coravel*.

there seems to be a declination dependence too at the lowest declinations. It is difficult to investigate, and probably impossible to correct, its sinister implications, but it may be mentioned that with the *f*/30 coudé system used by the Cambridge *Coravel* the beam from the telescope is vignetted below a limiting declination of −5°.

In solving the orbit, the observations with the three *Coravel* instruments have all been accorded unit weight; those made with the original spectrometer warrant a weighting of only ⅛ and those made at the DAO and Palomar have been weighted ½. Fig. 4 illustrates the solution, whose elements are as follows:

P	=	341·154 ± 0·011 days*	$(T)_{26}$	=	MJD 51144·81 ± 0·11
γ	=	+55·58 ± 0·05 km s^{-1}	$a_1 \sin i$	=	38·78 ± 0·26 Gm
K	=	13·17 ± 0·07 km s^{-1}	$f(m)$	=	0·0200 ± 0·0004 $M_\odot$
e	=	0·7785 ± 0·0020			
ω	=	17·1 ± 0·5 degrees	R.m.s. residual (wt. 1)	=	0·41 km s^{-1}

*The 'true' period (in the rest-frame of the system) is 341·091 ± 0·011 days. It differs from the observed value by 5·9 standard deviations.

If the primary star is indeed a giant, and may be attributed a mass of 2 $M_\odot$, then the minimum mass of the secondary is 0·5 $M_\odot$, corresponding to the mass of a main-sequence star with a type of about M0. The secondary certainly cannot be expected to be a white dwarf, since a system with a period of only one year would assuredly not be left with such a high eccentricity after the completion of the giant-branch evolution of one of its components. No evidence of the companion star has been seen in the radial-velocity traces.

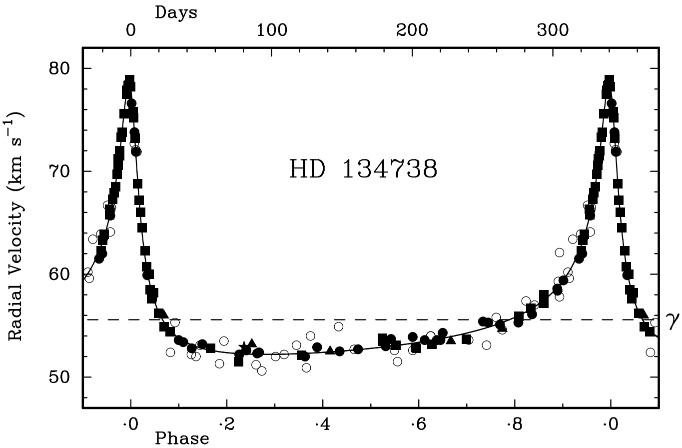

FIG. 4

The observed radial velocities of HD 134738 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. Observations made with the original radial-velocity spectrometer at Cambridge are shown as open circles and were given a weight of only ⅛ in the solution of the orbit. Those made with the OHP and Cambridge *Coravels* (both given unit weight) appear as filled circles and squares, respectively, while DAO and Palomar measurements (weight ½) are plotted as filled triangles and as a single filled five-point star, respectively. Three velocities obtained with the ESO *Coravel* are treated as if they were made at OHP.

HD 216525

HD 216525 is a star, fainter than most of those treated in this series of papers, in the southern part of Pegasus about ½° south-preceding the fifth-magnitude ρ Pegasi. It was placed on the Cambridge observing list as being eligible for the Clube Selected Areas programme in an extended version of Area 10, centred at RA 23^h, declination +21°. We are indebted to *Hipparcos*/*Tycho* for what little is already known about it: $V = 9^m{\cdot}53$, $(B-V) = 1^m{\cdot}14$, $\pi = 0''{\cdot}00041 \pm 0''{\cdot}00145$. With a parallax indistinguishable from zero, the star is evidently at least several hundred parsecs away and must have a distance modulus of at least eight or nine magnitudes, demonstrating that it is a giant. That makes it the more interesting that its orbital period, as will be demonstrated below, is only about 16 days.

Radial-velocity observations had an uncharacteristically sudden start. The first observation, in 2002 October, showed a very wide and shallow dip, requiring several minutes' integration to obtain a reasonably reliable velocity on such a faint star. The obvious interpretation of the high implied rotational velocity was that the star was a member of a short-period binary system, so it was re-observed on the following night, when it was duly found to have undergone a considerable change of velocity. Nine additional observations, still in the same calendar month, provided more than enough material for the initial derivation of its orbit. Now, after some six years, there is a total of 40 radial velocities, which are set out in Table IV. Inasmuch as they have all been made with the same instrument, the Cambridge *Coravel*, they have not been weighted or tampered with in any way, but simply solved for the orbit, which is illustrated in Fig. 5 and has the following elements:

P	=	$15{\cdot}9054 \pm 0{\cdot}0005$ days*	$(T_0)_{31}$	=	MJD $53040{\cdot}437 \pm 0{\cdot}016$
γ	=	$+30{\cdot}14 \pm 0{\cdot}17$ km s^{-1}	$a_1 \sin i$	=	$7{\cdot}29 \pm 0{\cdot}06$ Gm
K	=	$33{\cdot}34 \pm 0{\cdot}27$ km s^{-1}	$f(m)$	=	$0{\cdot}0612 \pm 0{\cdot}0015\ M_\odot$
e	≡	0 (fixed)			
ω	is undefined in a circular orbit		R.m.s. residual = $1{\cdot}0$ km s^{-1}		

*The 'true' period (in the rest-frame of the system) is $15{\cdot}9038 \pm 0{\cdot}0005$ days. It differs from the observed value by $3{\cdot}5$ standard deviations.

The statistical test explained by Bassett[28] has been used to check that the assumption of an exactly circular orbit is warranted. The sum of the squares of the residuals of the 40 observations from the circular solution is $38{\cdot}35$ (km s^{-1})^2; it falls to $33{\cdot}39$ when the eccentricity is left free. The difference of $4{\cdot}96$ (km s^{-1})^2 is to be ascribed to the two extra degrees of freedom represented by e and ω, while the $33{\cdot}39$ is the cost of the remaining 34 degrees. The ratio of those variances, per degree of freedom, gives $F_{2,34} = 2{\cdot}52$, which is just about at the 10%-significance point (2·48; 5% is 3·28, 1% 5·29) and thus 'not significant' in any statistical sense.

Although the dip seen in radial-velocity traces is very shallow, its actual area is not particularly small, being similar to that normally given by late-G giants; the observational problem is that the dip is greatly smeared out, no doubt by the rapid rotation of the star. The mean projected rotational velocity, $v \sin i$, is 29 km s^{-1}, and being the mean of 40 individual values it has only a small formal uncertainty. The r.m.s. deviation of the individual values from the mean is $2{\cdot}0$ km s^{-1}, but by itself that quantity conceals the fact that the deviations do not appear to be random; in particular, there was a time in the summer of 2003 when the values were persistently above 30 km s^{-1}, whereas in 2006 they averaged 27. No instrumental reason for such variation is identifiable, but it

TABLE IV

Cambridge radial-velocity observations of HD 216525

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)* *km s⁻¹*
2002 Oct. 3·97	52550·97	+35·2	0·226	+0·1
4·94	551·94	+21·7	·287	−0·7
6·92	553·92	+1·0	·412	−0·8
9·96	556·96	+3·8	·603	+0·3
12·99	559·99	+38·2	·793	−0·9
18·02	565·02	+56·8	1·110	+0·9
18·98	565·98	+47·0	·170	+0·8
19·94	566·94	+35·2	·230	+1·0
21·96	568·96	+8·2	·357	−1·1
23·94	570·94	−4·1	·482	−1·1
27·96	574·96	+27·3	·735	+0·4
Nov. 3·96	581·96	+45·1	2·175	−0·2
4·90	582·90	+34·4	·234	+0·9
6·93	584·93	+8·7	·361	0·0
12·90	590·90	+28·0	·737	+0·6
Dec. 9·84	617·84	+0·6	4·431	+0·7
2003 Aug. 3·10	52854·10	+23·8	19·285	+0·9
4·09	855·09	+10·5	·347	−0·6
15·10	866·10	+61·1	20·039	−1·4
30·09	881·09	+62·8	·982	−0·5
Sept. 14·03	896·03	+58·5	21·921	−0·9
24·00	906·00	+1·0	22·548	+2·7
28·98	910·98	+51·8	·861	+0·3
Oct. 11·94	923·94	+15·5	23·676	+0·4
Dec. 7·79	980·79	+30·7	27·250	+0·5
15·78	988·78	+32·2	·752	+1·6
2004 Aug. 20·09	53237·09	+8·1	43·364	−0·2
Sept. 1·10	249·10	+56·3	44·119	+1·7
Oct. 6·01	284·01	+16·2	46·314	−0·9
Nov. 12·91	321·91	+19·3	48·697	+0·1
14·90	323·90	+44·2	·822	−0·5
26·82	335·82	−0·5	49·571	−0·6
Dec. 19·82	358·82	+62·6	51·017	−0·7
2005 Sept. 24·02	53637·02	−2·0	68·508	+1·2
2006 Sept. 11·05	53989·05	+7·9	90·641	−1·2
20·07	998·07	+37·6	91·208	−1·2
21·01	999·01	+27·6	·267	+1·1
23·02	54001·02	+1·9	·394	−2·1
Dec. 16·83	085·83	+24·3	96·726	−0·8
2007 Sept. 15·04	54358·04	+48·2	113·840	+0·2

would be unwise to rule out its possibility entirely in the absence of stronger evidence of real variation. *Hipparcos* did see distinct evidence of photometric variability, and listed the star as an 'unsolved variable', though without arranging for it to receive any variable-star designation. Koen & Eyer[29], taking up where *Hipparcos* left off, considered that they had discovered a periodicity in the *Hipparcos* 'epoch photometry', but (as in so many other cases) the period that they thought they found was very close to the rotational period of the satellite — and the small discrepancy from it does not correspond to an alias of the orbital period.

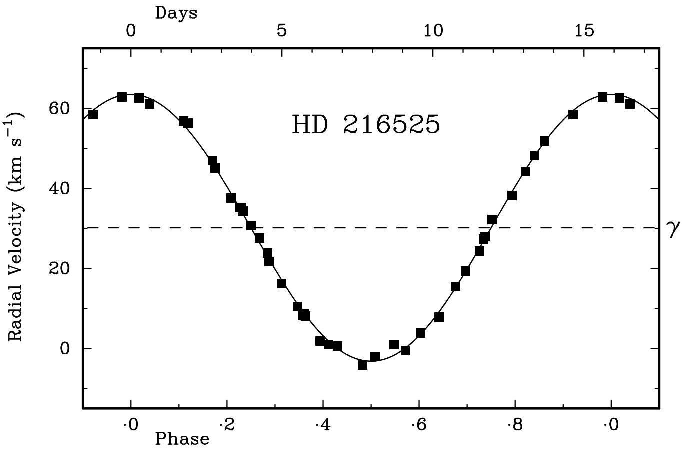

FIG. 5

The observed radial velocities of HD 216525 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. All the observations were made with the Cambridge *Coravel*.

Late-type stars in such rapid rotation nearly all exhibit active chromospheres, with strong and variable emission in *H* and *K* and usually in Hα too, but no such activity has been reported for HD 216525. It may occur but simply have been overlooked, because the star is rather faint and up till now there has been no reason to take a special interest in it — although that does not explain how it came to be included in the *Hipparcos Input Catalogue*[30]. In any case it would seem well worthwhile now for observers with appropriate instrumentation to look for photometric and spectroscopic manifestations of chromospheric (RS CVn-type) activity.

It is of some interest to speculate on the nature of the spectroscopic companion to HD 216525. If we make the assumption (which has become conventional in this series of papers, without thereby being made any more probable) that the observed giant has a mass of 2 $M_{\odot}$, then the mass function derived from the orbital elements shows in this case that the companion has a minimum mass of about 0·8 $M_{\odot}$. There is no means of telling how far above that minimum it may be unless its signature can be seen in the spectrum, or equivalently in radial-velocity traces. Although giant stars in short-period orbits are rare, several have previously been discovered by the writer. Two which are quite analogous to HD 216525 are HD 115781[31], with a period of 19 days and a $v \sin i$ of nearly 40 km s^{-1}, and HD 33978[32], with a period of 11 days and $v \sin i$ 39 km s^{-1}. In each of those cases, radial-velocity traces with high *S*/*N* ratios permitted the discovery of very small secondary dips, smaller in terms of area by a factor of 19 in the case of HD 115781 and 14 in that of HD 33978. Those stars are brighter than HD 216525, near eighth magnitude, and the high-*S*/*N* traces were obtained in observations made near the zenith at good sites with larger telescopes (the Palomar 200-inch and the Danish 61-inch at ESO, respectively) than that with which the writer has observed HD 216525 low in the sky. There is little hope of observing similarly weak secondary features (if they were to exist) with the presently available instrumentation.

The minimum size of the star is immediately calculated from the observed rotational velocity multiplied by the orbital period; the radius has to be at least 9 $R_\odot$. Direct comparison of the $v \sin i$ value of 29 km s^{-1} with the (semi)-amplitude of the radial-velocity variation, $K \sim 33$ km s^{-1}, demonstrates that the centre of gravity of the binary system lies just outside the primary star, at a radial distance of $33/29\, R_\star$ from its centre, no matter what the orbital inclination may be. Since the companion is not likely to be more massive than the star that has been observed, and is almost certainly a lot smaller, we can be sure that the system is well detached.

References

(1) R. F. Griffin, *MNRAS*, **219**, 95, 1986.
(2) R. F. Griffin, *The Observatory*, **105**, 226, 1985 (Paper 65).
(3) A. R. Sandage, *ApJ*, **135**, 333, 1962.
(4) A. R. Upgren, W. S. Mesrobian & S. J. Kerridge, *AJ*, **77**, 74, 1972.
(5) I. Platais *et al.*, *AJ*, **126**, 2922, 2003.
(6) P. C. Keenan & C. Barnbaum, *ApJ*, **518**, 859, 1999.
(7) A. Baranne, M. Mayor & J.-L. Poncet, *Vistas Astr.*, **23**, 279, 1979.
(8) J. M. Fletcher *et al.*, *PASP*, **94**, 1017, 1982.
(9) R. F. Griffin & J. E. Gunn, *ApJ*, **191**, 545, 1974.
(10) A. M. Geller *et al.*, *AJ*, **135**, 2264, 2008; **137**, 3743, 2009.
(11) [Announced by] B. Famaey *et al.*, *A&A*, **430**, 165, 2005.
(12) E. E. Mendoza, *Rev. Mex. A&A*, **1**, 175, 1974.
(13) A. Feinstein, *AJ*, **79**, 1290, 1974.
(14) A. Slettebak & J. J. Nassau, *ApJ*, **129**, 88, 1959.
(15) R. F. Griffin, *The Observatory*, **122**, 329, 2002 (Paper 167; see footnote on pp. 330/1.)
(16) [Announced by] S. Grenier *et al.*, *A&AS*, **137**, 451, 1999.
(17) G. Lemaitre *et al.*, *A&A*, **228**, 546, 1990.
(18) J.-M. Carquillat & J.-L. Prieur, *MNRAS*, **380**, 1064, 2007.
(19) H. A. Abt, in W. Hartkopf, E. F. Guinan & P. Harmanec (eds.), *Binary Stars as Critical Tools & Tests in Contemporary Astrophysics* (*IAU Symposium*, no. 240) (CUP, Cambridge), 2007, p. 414.
(20) R. F. Griffin, *The Observatory*, **128**, 448, 2008.
(21) R. F. Griffin, *The Observatory*, **123**, 344, 2003 (Paper 173).
(22) T. Schmidt-Kaler, in K. Schaifers & H. H. Voigt (eds.), *Landolt-Börnstein* (Springer-Verlag, Berlin), **2b**, p. 18, 1982.
(23) J. Andersen, *A&A Review*, **3**, 91, 1991 (see Fig. 2).
(24) P. Hut, *A&A*, **99**, 126, 1981.
(25) A. J. Cannon & E. C. Pickering, *HA*, **91**–**99**, 1918–1924.
(26) R. F. Griffin, *The Observatory*, **120**, 137, 2000 (Paper 151).
(27) R. F. Griffin, *The Observatory*, **124**, 190, 2004 (Paper 176).
(28) E. E. Bassett, *The Observatory*, **98**, 122, 1978.
(29) [Announced by] C. Koen & L. Eyer, *MNRAS*, **331**, 45, 2002.
(30) C. Turon *et al.*, *The Hipparcos Input Catalogue* (ESA SP–1136) (ESA, Noordwijk), **5**, 2270, 1992.
(31) R. F. Griffin & F. C. Fekel, *JA&A*, **9**, 213, 1988.
(32) R. F. Griffin, *MNRAS*, **371**, 1159, 2006.

REVIEWS

Hidden Harmony, by J. R. Leibowitz (Johns Hopkins University Press, Baltimore), 2008. Pp. 160, 19 × 23·5 cm. Price $24·95 (about £16) (hardbound; ISBN 978 0 8018 8866 3)

It is no bad thing to be reminded that science and particularly physics is not totally solid logic; at the core, where the fundamentals are teased out of the fabric of the Universe, physics is an exercise of the imagination — not in the sense of making things up but in the sense of finding order and creating simplified models through reducing complex data, sifting out and selecting what is relevant, and discarding noise. While acknowledging the importance of the physics imagination, there may also be a common psychological condition among physicists, that in order to compensate for their professional lives in a tough science they take on the mantle of 'Renaissance Man' and acquire a deep interest in the arts. Anyway, there is a strong tradition of scientists writing about the arts, and even in textbooks there is a tendency to ape art world *mores*. Think of all those poetic and literary chapter headings in almost every learned monograph. And indeed I am showing the same symptoms in writing this review. There is, however, a deeper strain of this condition in which the actual tools of physics are used to explain some aspect of the visual arts, or as in the case of this book, to show some underlying equivalence in aim between the artist and scientist. Or as it says on the dust jacket, "... that physics and art share guiding aesthetics ...."

The title *Hidden Harmony* sounds as if it might come from Alexander Pope's essays on man, in which in rhyming couplets he declares a ringing endorsement of experimental physics:

All nature is but art unknown to thee
All chance direction which thou canst not see
All dischord harmony not understood
All partial evil, universal good.
In spite of pride
And erring reasons spite
One truth is clear
Whatever is, is right.

Leibowitz is an emeritus professor of physics at the Catholic University of America (the national university of the Catholic Church in the United States) and also a one-time chairman of its art department. Pope would approve of his attempt to show the connectedness between concepts in physics and art. At first glance it sounds an unlikely project. To be sure, we understand that the physics of musical sounds and the physics of perception and colour vision have artistic relevance. But to attempt to reconcile the airy-fairy-seeming nonsense of art-speak with the hardest of hard sciences sounds like a step too far. Leibowitz makes a worthy attempt, if occasionally drifting over to being a little condescending with a sort of adult-education feel to the erudition — the great man speaking to the massed ranks of the unaware. But nonetheless, to some extent he succeeds.

He begins with the conservation laws — energy, momentum, *etc.* — and introduces the Noether theorem which states that "all continuous transformations correspond to conservation laws". Our well-known conservation laws fall out of the most basic axiom of physics — the invariance to certain transformations, *i.e.*, that the laws of physics are the same no matter where and when you test them. Once you have said that physics is the same at time t_0 as it is at time t_1 then energy conservation results. Likewise momentum conservation results from the invariance to position. I was surprised by the Noether theorem as I had not met this powerful and highly relevant piece of fundamental physics before, which places the conservation laws in context — or at the very least the Noether theorem is a restating of them in terms of invariance. So far, so good, but how does this relate to art? Leibowitz introduces us to balance and weight, symmetry and broken symmetry, tension and interest, and pigment-colour theory, with example paintings and sculptures from the 15th to 20th Centuries. We are guided through the idea that just as information in the physical world is a consequence of broken symmetry so in a similar way the interesting elements in works of art come from breaking the symmetry as well as resolving tension and using the emotional contents of colour: the warmth of reds, the cool of blues, and so on. We are told that just as the success of physical theory depends on its passing various tests (falsifiability, *etc.*) for art the equivalent test seems to be "does it work?". Apart from begging many more questions than it answers — does it work for whom, when does it work, *etc.* — it just quite frankly seems a bit too easy.

I suppose this highlights one of the things that most bothered me about this book: the assumption that it is okay for a professor of physics to explain the science and then go on to explain the relation to structure and form in art. I don't think I would be as content for a professor of fine art to tell me about art and then show me how that relates to physics. There would be a problem of credibility. There seems to be an inherent arrogance in physics that assumes that its success in explaining and rationalizing the physical Universe makes it equivalently valid as a starting point to explain other features of the human landscape. There may be similarities and some of the same tools are used, but to claim an underlying equivalence seems to me to be false.

The best bits of this book are in giving a real feel for, and yet dealing non-mathematically with, physics concepts. I've already mentioned the conservation laws; the section on the underlying concepts of Maxwell's equations and the resulting description of electromagnetic radiation is wonderful. The vector symbols are treated as elegant hieroglyphs which do not need to be understood in order to grasp the relationship between the terms.

In science our models of the physical world must necessarily be abstractions — certainly in cosmology and astronomy the physical realities are well beyond our reach. Just emphasizing that point is to reflect on the nature of physics. Leibowitz compares this truth to the position of an artist such as Cézanne, who we are told, in his paintings from nature, attempts to realize the human sensations experienced when viewing nature, *i.e.*, he abstracts from experience.

Perhaps the essence of what this book is all about comes in the final, summary chapter. Abstraction from direct experience has provided a way forward for physicists, with ideas being developed on the basis of simple imagined models: light travelling in the ether, which although later found to incorrect, provided a foundation model for the wave-like nature of light; or imagined passengers in falling lifts giving an insight into the curvature of space time. Leibowitz claims the same use of simple ideas and abstractions is true in the visual arts. I can see that we may claim a forward direction in our understanding of the physical Universe — our descriptions become more complete and applicable to a wider range of situations. I am less sure that the same can be said for the arts. What would be the equivalent of the useful but failed ether model?

Nevertheless, this is a thoroughly thought-provoking, accessible read, which not only provides some interesting views on art but gives some very nice insights into the concepts underlying physics. — BARRY KENT.

Foundations of New World Cultural Astronomy, edited by Anthony Aveni (University Press of Colorado, Boulder), 2008. Pp. 826, 23 × 15 cm. Price £26·50 (paperback; ISBN 978 0 87081 900 1).

The major problem with archaeoastronomy is that the people you would like to question are dead — long dead; and not only are their lab notes and research papers missing, they were never written in the first place. So you stumble over such marvels as the Intihuatana stone at Machu Picchu; the geometric earthworks at Newark, Ohio; the E-Group complex at Uaxactun, Guatemala; the Navaho Canyon petroglyphs in Arizona; the thirteen towers in Chankillo; and the desert Nazca lines in Peru, and you are completely mystified as to their purpose. What were our ancestors doing? Were they trying to regulate a solar calendar by taking solar-horizon observations at solstices, equinoxes, and quarter days? Were they trying to integrate solar and lunar calendars? Or, in the case of the petroglyphs, are we looking at drawings of their favourite constellations, or amazing, rare celestial sights such as supernovae or great comets? Our answers are inevitably tainted with our modern pre-conceptions and so archaeoastronomy abounds with controversies, challenges, and scepticism.

At the heart of the subject is a clash between the astronomers who can easily map out what the sky was doing, and the archaeologists and anthropologists who try to reveal what ancient cultures found significant. It is this confluence of a precise science and a social science that has fascinated many new students. To overcome the fact that most of the original research papers were published in rare specialist journals and rather obscure conference proceedings, Anthony Aveni, one of the leading scholars in the field, has collected together in the volume under review a host of the more significant recent contributions to the subject. This introductory 'reader' is well indexed and the individual papers are also enlivened by appended discussion sections and commentaries. Refreshingly for the European reader is the fact that what happened this side of the Atlantic is mainly ignored. The book concentrates on the Americas, North, South, and Central. It is a fascinating read, and even if it solves few of the arguments at least it gives the protagonists more ammunition. — DAVID W. HUGHES.

Eyes on the Skies: 400 Years of Telescopic Discovery, by G. Schilling & L. Lindberg Christensen (Wiley, Chichester), 2009. Pp. 133, 30 × 25 cm. Price £14·99 (hardbound; ISBN 978 3 527 40865 8).

Stunning, colourful, large images grace this impressive book. Portraits of Galileo, Lipperhey, and Hale compete with full-page pictures of telescopes such as Herschel's 1·2-m, Rosse's *Leviathan*, the *Yerkes* refractor, the *Hooker*, ESO's *Very Large Telescope*, the *Large Binocular Telescope*, and *Hubble*. Moving from the visual, we are shown a pre-launch view of the *Spitzer Telescope*, the Arecibo dish, lines of *ALMA* antennae, and one of the sixteen-hundred squat water tanks of the *Pierre Auger Observatory*. Celestial images of NGC 1672, Cassiopeia A, and the Sombrero Galaxy are juxtaposed with the likes of the *Hubble* Deep Field and the *WMAP* image of the microwave sky, but the telescopes remain the stars of this book.

The accompanying text is informative and paints an accessible picture of what astronomers are up to and how their basic tool — the telescope — has changed during the 400 years since it was turned skyward. This book is an ideal gift for the young astronomer, and gives an accurate impression of what most professional astronomers are capable of imaging. The advantages of the charge-coupled device over the photographic plate and the pencil drawing are stressed. And the suitability of mountain-top locations such as Kitt Peak and Mauna Kea is discussed. But for me there is rather too much about the 'what' and too little about the 'why'. The opportunity to explain why astronomers are so keen to double the size of the biggest available telescope every 50 years or so has been missed. — CAROLE STOTT.

Lettres à Madame du Pierry et au Juge Honoré Flaugergues — Lalandiana I, by Jerôme Lalande; texts edited, annotated, and commented by Simone Dumont and Jean-Claude Pecker (Ed. Vrin, Paris), 2007. Pp. 272, 20·5 × 13·5 cm. Price €25 (about £23) (paperback; ISBN 978 2 7116 1939 9).

This book gathers together letters exchanged between Jérôme Lalande and, on one hand, Louise Dupiéry (or du Pierry), who had been more than a friend for Lalande and, on the other, Honoré Flaugergues, a conciliation magistrate in Viviers and an 'amateur' astronomer. Dupiéry was also occasionally involved in astronomical activities.

Lalande left a voluminous correspondence, today spread over many libraries in Europe and the US. The letters offered in this compilation have been selected by two experts on Lalande's life and activities (see the review in **129**, 35). The texts have been edited whenever necessary: punctuation and accentuation often neglected by Lalande, use of upper-case characters according to modern standards, rectification of spelling mistakes, and explanation of abbreviations sometimes abundantly used by Lalande. The letters are extensively commented upon and annotated. Their chronology has been restored whenever possible, some of them being undated.

A substantial index (56 pages) of names mentioned in the correspondence provides the readers with a context for the characters involved. Flaugergues himself received a dedicated biography (14 pages). An astronomical section (17 pages) gathers together a number of definitions and concepts for non-specialists.

Technically the book is well presented, with a few illustrations (all b/w). The price is a bargain, especially at the cost of reproduction of archive documents from official institutions. The editors and the publisher have to be commended for making available such a resource to present and future generations of historians of astronomy. We ought to see more often such contributions. — A. HECK.

Le Méridien de Paris — **Une randonnée à travers l'Histoire**, by Philip Freriks (Ed. EDP Sciences & Obs. Paris, Paris), 2009. Pp. 132, 24 × 13·5 cm. Price €19 (about £17) (paperback, ISBN 978 2 7598 0078 0 & 978 2 901057 62 8).

This book is the French adaptation of a Dutch edition published in 2007 under the title *Het spoor van de monumentale meridiaan — Een 'petite histoire' van Parijs* (The Trail of the Monumental Meridian — A Short Story of Paris), a very first version of which was made available in 1995.

The volume is organized along the lines of the 'imaginary monument' created by the Dutch artist Jan Dibbets between 1989 and 1994 and resulting from a public tribute to French astronomer François Arago (1786–1853): an open trail through the city created with 135 medallions (each a dozen centimetres in diameter) sealed mainly on the ground along the meridian of Paris within the city *Boulevard Périphérique* (ring freeway). Those small plaques are marked with the name of Arago and the letters N and S indicating North and South. They can be found in significant locations from the *Cité Universitaire* (university residences) in the South up to the vicinity of the *Place Pigalle* in Paris' northern red quarter, *via* the Montsouris park, the Luxembourg gardens, the Louvre museum, and the Royal Palace, just to mention a few of the best-known spots. Some of the medallions have already disappeared (in the hands of collectors, covered by road-works, restructuring of sidewalks, *etc*.) or become inaccessible (now in properties with restricted access).

Thus the book can be seen as a South-to-North walk along the meridian of the French capital, describing interesting monuments and places, as well as related events and characters. Much can be learned along such a route, even if one remains sometimes unsatisfied. For example, the reader curious of Arago's life should rather acquire the excellent 2008 book by James Lequeux (same publishers) reviewed in this magazine (see **128**, 501, 2008). Freriks' approach is definitely that of an outsider, in the sense that a French or a Parisian author would have certainly presented things differently, without that Dutch touch found here and there (Juliana Foundation, Mata-Hari, ...), sometimes vindictively when the author roundly qualifies as plagiarism the *Méridienne Verte* (Green Meridian), a line made of trees imagined by Paul Chemetov to mark Year 2000 and crossing France along the Meridian of Paris from Dunkirk to Prats-de-Mollo-la-Preste on the Spanish border.

Eight rudimentary maps help with spotting the medallions in some quarters. There is a one-page bibliography (but it should have been updated to include, for instance, Lequeux's book mentioned above). A name index for people and places would have been most welcome. But the most surprising feature of this original city guide is that all illustrations are in black and white. It is risky of the publishers to dare offering 132 pages in black and white for €19 while very glossy tourist guides, entirely in colour, are nowadays available for similar or even lower prices. And one could have hoped for such an option here, particularly since the book has been published with the help of the Foundation for the Production and Translation of Dutch Literature. — A. HECK.

The Day We Found the Universe, by Marcia Bartusiak (Pantheon, New York), 2009. Pp. 368, 23 × 15 cm. Price $27·95 (about £14) (hardbound; ISBN 978 0 375 42429 8).

The day of the title is 1925 January 1, when a paper (by Edwin Hubble), announcing his discovery of Cepheids in M31 and M33 was read (by H. N. Russell) before a meeting of the American Association for the Advancement of Science in Washington, D.C. But in fact the book is a spritely and careful voyage through the period 1900–1930 that saw the final resolution of the issue of existence of other galaxies and the first generally-accepted data for a linear relationship between galaxy redshifts (from V. M. Slipher and M. L. Humason) and distances (from Hubble, who won the naming competition). Hubble also shared the $1000 prize for best paper presented at the AAAS meeting, with parasitologist Lemuel Cleveland, whose paper was on the digestion of cellulose by termites. Folklore (to which I have, shamefully, succumbed) has given the other half to Dayton Miller for his square-wheeled failure to replicate the

Michelson–Morley experiment. And the author's getting this right is characteristic of the book.

Another such item is the 1909 pre-discovery of the direction to the Galactic Centre by Karl Bohlin, for which the reference is provided, though she has missed out our friend Easton, who in 1900 put the Sun at the centre of a circular galactic disc, but the centre of the spiral-arm pattern over in Cygnus. My list of 'bravos' has 15 other items, of which you get only two: (*i*) van Maanen's report of solar magnetic field varying with latitude (conceivably a precursor to his report of rotation of spirals in his proper-motion measurements), and (*ii*) H. P. Robertson's 1932 surprise at finding more than 150 references on what he called relativistic cosmology already in the literature, given that it has been claimed elsewhere that the subject did not exist in the 1930s.

Of course I also have a list of 'nos', 'oopses', 'ums', and 'ers' (the details of the Chamberlin–Moulton hypothesis, location of Pluto relative to comet orbits, inadequate credit to Crommelin in connection with the 1919 eclipse expeditions, and so forth), but they are definitely outweighed by the positive items. The 'tell me more' list has a few items where I know the answer (Frank Capra was the one to photograph Einstein during his Caltech visit because he was a Tech alumnus; and poet Alfred Noyes was on hand because his brother was Caltech's 'founding chemist'). This sort of thing is what is called "extra value" in a book review. There are also items where I don't know the answer but would like to (who was the Mt. Wilson colleague who redid and confirmed van Maanen's spiral-rotation results; what did Shapley mean by a subordinate system of stars with the Sun fortuitously near its centre; and what are those animals in front of Grace and Edwin Hubble on their wedding day?).

A final, unnumbered chapter answers the question, "whatever happened to?", concerning 13 scientists and four observatories that were important in the main story. It is perhaps inevitable that for virtually all, one has to say of the period covered in the book that "this was their finest hour".

Conflict-of-interest statement: author Bartusiak cites one of my papers (on the Curtis–Shapley debate) and kindly does not point out its errors. But, of much greater significance, she has generously given me copies of the several hours of taped interviews with Joseph Weber that she recorded while writing an earlier book on gravitational radiation called *Einstein's Unfinished Symphony*. — VIRGINIA TRIMBLE.

Hidden Universe, by L. Lindberg Christensen, R. Fosbury & R. Hurt (Wiley, Chichester), 2009. Pp. 145, 30 × 25 cm. Price £14·99 (hardbound; ISBN 978 3 527 40866 5).

During the past half-century or so, we have witnessed a remarkable revolution in observational astronomy. Until the mid-20th Century, our view of the Universe was almost entirely restricted to the narrow band of electromagnetic radiation that could penetrate directly to the ground and which was visible, as the authors remark, "to our eyes or to sensitive photographic plates loaded at the focus of increasingly large telescopes". Since then, with the advent of new technology and the ability to place instruments in orbit, the entire electromagnetic spectrum — from radio waves to gamma-rays — has been opened up to the astronomer's gaze.

This book presents a timely and stimulating account of the Universe as viewed across the whole of the electromagnetic spectrum. Its first three chapters set the scene with accounts of the nature of light and vision, the view from the ground, and a description of space observatories. The next five deal with the

Universe as seen through different bands of the electromagnetic spectrum, and the final chapter draws the strands together to present a multiwavelength view of the Universe, achieving that synthesis with aid of a series of images of the active galaxy Centaurus A.

Whereas the primary impact of the book resides in its exquisite images of astronomical phenomena and of some of the ground-based and space-borne instruments which have generated them, the explanatory text, too, is a model of lucidity. The background science which enables the reader to make sense of the images and to appreciate the information that is contained within them, is explained concisely, but with great clarity, in a form that is easy and enjoyable to read. Jargon is kept to a minimum, but important technical terms are not shirked. The excellent captions contain enough detail to enable the reader to understand the significance of each image, and to see it as much more than merely a beautiful picture — though the aesthetic appeal of the images is no less important for that.

It is exceedingly hard to find anything to criticize in this well-written, splendidly illustrated, and skilfully designed book. Perhaps the only perplexing feature is the presence of eight completely blank pages between the image credits and the multi-wavelength images of the Whirlpool galaxy on the inside back cover. With such a wealth of images 'out there' it does seem a shame not to have used that space to showcase a few more. But the bottom line is clear. This book is a masterpiece of its kind — one which can be read straight through or dipped into and enjoyed at random, again and again. Highly recommended to any general reader who wishes to savour the beauty of the cosmos and to absorb the wealth of knowledge that has flowed from our multi-wavelength view, *Hidden Universe* will also be a source of pleasure and heightened understanding for students, amateur astronomers, and professionals alike. — IAIN NICOLSON.

Illustrated Encyclopedia of the Universe, by R. Dinwiddie (Dorling Kindersley, London), 2009. Pp. 512, 26 × 22 cm. Price £22 (hardbound; ISBN 978 1 4053 3309 2).

As an astronomy educator, teaching the subject across a wide spectrum of ages, I am constantly asked for book recommendations, in particular for introductory texts. This is certainly one book I would recommend. Its comprehensive coverage ensures that it has something for everyone in one dense, lavishly illustrated, and well-written volume. It is also extremely good value; the density of information packed into its 512 pages is astounding. Beginning with a short tour and introduction to the Universe, it provides the reader with an overview of its scale, structure, and evolution in the first 55 pages. It then gives a comprehensive coverage of observational astronomy, including how to observe the sky with the naked eye and with astronomical instruments (20 pages), with a small section on astrophotography. The next 28 pages cover 'Exploring space', possibly a slightly misleading title, taking the reader on an excellent tour of how our knowledge of the Universe has evolved from ancient times. The bulk of the book, the 'Guide to the Universe', has subsections entitled 'The Solar System', 'The Milky Way', and 'Beyond the Solar System'. Once again these sections are comprehensive, with a good mix of text, graphics, and illustrations.

The layout mixes historical information, about people and observations, together with contemporary results and up-to-date interpretations. However, the coverage of spectroscopy is very limited, which is a great pity, and there are very few images of real spectra. The final part of the book, covering 152 pages, introduces the constellations in great detail, and then provides a fine set of monthly star charts (northern and southern skies), with insets showing the Zodiac and the monthly position of the planets, together with information on special astronomical events over this period, such as meteor showers and eclipses. This 2009 edition, however, covers planetary and special-event data from 2007 until 2012; it would have been good to have extended the data to at least 2015 or thereabouts. Each star chart can be used for latitudes 20° – 60° N and 0° – 40° S, ensuring a good Earth coverage for potential users. Finally the book ends with an 8-page glossary and substantial index. The dense layout of the book may not suit everyone and the text size on many pages is very small and some of the images and graphics are tiny. These minor issues aside, there is no doubt that this volume is an excellent addition to the library of anyone who needs a comprehensive coverage of astronomy. — JOHN GRIFFITHS.

The Pluto Files: The Rise and Fall of America's Favorite Planet, by N. deGrasse Tyson (W. W. Norton, London), 2009. Pp. 194, 21·5 × 16·5 cm. Price £14·99/$23·95 (hardbound; ISBN 978 0 393 06520 6).

"Dear Dr. Neil Tyson degrasse. At first, remember all of those kids that sent you bad letters? Well, I want to apolijize [*sic*] all the things that we were wrong about. We're sorry about giving you mean letters saying we love Pluto but not you. I'm very sorry, it'll be ok." This 2008 March letter from a schoolchild in Florida, used as the frontispiece of *The Pluto Files* (alias *The Plutophiles*), illustrates a phenomenon I have also noticed — namely, that whole classes of American schoolchildren, once so eager to inform (in no uncertain terms) 'Pluto killers' such as the author and myself that Pluto was their favourite planet (mine is the Earth), have for the most part accepted the IAU's 2006 'demotion', while so many of their elders (including some professional astronomers who should know better) have not.

As the "most visible exponent" of the decision to "cast Pluto out of the pantheon of planets" in the main display at the American Museum of Natural History in New York, Neil Tyson has borne the brunt of the wrath of those who aver that "it's like he's in a different universe" and that "Pluto is a true-blue American planet, discovered by an American for America". After a brief account of how Pluto came to be found and named, together with the usual comparisons with Uranus and Neptune and the early disagreements on what to call Ceres and the other small cisjovian bodies, Tyson discusses how the discovery of Charon finally allowed Pluto's mass to be satisfactorily determined, how the *Voyager* measurements conclusively showed that there are no anomalies in the motions of Uranus and Neptune to attribute to a 'Planet X', and how some of the other small bodies found beyond Neptune beginning in the early 1990s have Neptune-crossing orbits that are very similar to that of Pluto.

Inspired by a 1998 article in the *Atlantic Monthly*, Tyson describes his own foray from plutophilia when he wrote a popular article himself in which he both felt "compelled to defend Pluto's honor" as being "deeply in our twentieth-century culture and consciousness" and rationalized that Pluto had gone "from being the runt of the planets to the undisputed King of the Kuiper Belt". The *Atlantic Monthly* article had mentioned my suggestion that the minor planet number (10 000) should be assigned to Pluto, as well as Mike A'Hearn's suggestion (which I supported) that Pluto should have 'dual status' as both a major and a minor body. Since the number (10 000) would need to be assigned to *something* soon, the press had become aware of the situation, and it was the resulting misinformation that precipitated the "clumsy press release" (Tyson's words) from the IAU secretariat stating that the *status quo* would be maintained until the IAU policy was broadly and officially changed. Although a vote conducted by the Minor Planet Center among the readers of its publications showed that 65% of those responding (82% from outside and 51% from inside the US) were in favour of (10 000) Pluto, the opportunity to use a nice, round number was therefore lost.

As "the witty, affable, multicategorizer", I participated, with four colleagues, in the 'Pluto's Last Stand' debate Tyson organized at his Museum on 1999 May 24. As he notes, two of us (A'Hearn and I) continued to go for 'dual status', two (Alan Stern and David Levy) were unabashed Plutophiles, and one (Jane Luu) was for "uncompromising iceballhood" (a position I could also accept). Until reading this book, however, I had not appreciated that this was to be "the night Pluto fell from grace", convincing those involved with the upcoming Museum display that "Pluto needn't retain any kind of status at all, except for reasons of nostalgia".

As threatened in the IAU press release, various IAU committees continued their deliberations "on what exactly a planet should be". Then, in 2005 July came Mike Brown's announcement of the discovery of three new ultraneptunian (yes, one of his correspondents had chided Tyson for not knowing the difference between transneptunian and ultraneptunian) objects comparable to or larger than Pluto. So now the IAU had to act. As Tyson describes, a final 'Planet Definition Committee' was charged with preparing a proposal shortly before the IAU was to hold its General Assembly in Prague. Just days before the anticipated General Assembly vote, this committee announced its proposed resolution that there would be eight 'classical' planets, Ceres would be a 'dwarf planet' (although the committee was actually a bit vague about this term), and Pluto, Charon, and Eris (the name to be given in 2006 September to the largest of Brown's discoveries, although Tyson indicates his preference for Brown's original unofficial name of Xena, "after the buff, buxom, leather-clad, sword-wielding warrior princess of cable television who spends much of each weekly episode kicking medieval butt") would be 'plutons'. But all 12 would be 'planets', with "many more surely to come".

While the idea was mainly to incorporate objects large enough to be in hydrostatic equilibrium, the committee's fatal mistake was to include Charon, *i.e.*, to consider that Pluto and Charon together formed a 'double planet'. Furthermore, shortly before the General Assembly vote, the word 'classical' was transferred from the principal to a secondary resolution, and the meaning of 'dwarf planet' was extended to include Ceres, Pluto, and any non-satellite sufficiently massive to be 'nearly round' but not so that it had "cleared the neighbourhood around its orbit". After the principal resolution had been handsomely passed, the participants were carefully advised that it was the presence or absence of the adjective 'classical' that assured a positive or a negative answer to the question "Is Pluto a planet?" Since this secondary resolution was rejected by a large majority, Pluto's fall from grace was therefore complete.

Of course, most of that large majority was aware of the apparent tautology that the rejection also meant that "a dwarf planet is not a planet", or in the IAU's other official language, "*une planète naine n'est pas une planète*". In German, however, there is no logical problem with the statement that "*ein Zwergplanet ist nicht ein Planet*". For this reason, some IAU members had been attempting to replace 'dwarf planet' with a single word, such as 'planetino', which could be used essentially in all languages. By introducing the further two-part resolution recognizing Pluto as the prototype of the 'transneptunian (or ultraneptunian) dwarf planets', and calling these 'plutonians' (a change from the earlier 'plutons'), the Resolutions Committee showed that it was both aware of this problem and wanted somehow to recognize the historical significance of Pluto. But this still left Ceres out in the cold (so why not also have 'cereans'?). As it happened, the prototype idea for Pluto was adopted, but 'plutonians' were very narrowly defeated. Nevertheless, since dwarf-planet Ceres has already long been (1) Ceres, there was a green light for the Minor Planet Center to establish (134340) Pluto, and in 2008 May, the alternative term 'plutoids' (too similar to 'haemorrhoids', according to Stern) was adopted by the IAU Executive Committee as shorthand for 'ultra/transneptunian dwarf planets'.

Tyson concludes his story with reactions to 'Pluto's Judgment Day', including 'The Great Planet Debate' in 2008 August, in which he argued, more cordially than he had anticipated, with "Pluto's pitbull", Mark Sykes. Despite their lack of convergence on how to define a planet, they agreed "that the IAU had body-slammed Pluto on this one. And that a more enlightened solution to the problem awaited us all." Certainly, there needs to be a definition of 'planet' that also encompasses exoplanets, but the IAU decision was both a compromise and a necessary start, given that the eight currently acknowledged in the Solar System really comprise two groups of four. If it be thought inappropriate for the IAU to have conducted the vote it did, don't state legislatures have anything better to do? "Whereas... whereas" (nine of them), "now, therefore, be it resolved by the legislature of the State of New Mexico that, as Pluto passes overhead through New Mexico's excellent night skies, it be declared a planet and that March 13, 2007 be declared 'Pluto Planet Day' at the legislature". Appendices include the full text of this resolution that was passed, together with one from the California legislature that wasn't, together with a number of songs: "They met in Prague and voted, now Pluto's been demoted; oh, Pluto's not a planet anymore".

Given the light-hearted way in which this book is written, I hesitate to point out errors, but most readers of *The Observatory* will be surprised to read that Herbert Hall Turner was a former Astronomer Royal (p. 9) and wonder about the whereabouts of Eaton College (p. 10). — Brian G. Marsden.

Proceedings of the Twenty Sixth General Assembly Prague 2006 (Transactions of the International Astronomical Union XXVIB), edited by Karel A. van der Hucht (Cambridge University Press), 2008. Pp. 505, 24·5 × 17 cm. Price £68 (hardbound; ISBN 978 0 521 85606 5).

Here it is folks! The publishing event of the year (unfortunately, the year 2006) that you have all been waiting for — the official report of the Pluto vote at the IAU General Assembly in Prague. It ceased to be a planet by 237 votes to 157, with 30 abstentions. In fact the only issue that was nearly tied was the vote on "plutonian objects" as the category name for Pluto-class objects, which failed by 186 votes to 183 (prompting the oldest teller to suggest anyone who wanted a recount should go to Florida).

The item for which I have bought every issue of 'Transactions B' since 1970 is gone: the membership list with addresses, phone numbers, e-mails, and so forth. The address-only version goes back to the 1922 General Assembly (GA), and I have used Transactions IB to XXVB in a very large number of assorted historical and other reviews. What *is* here includes official reports of the business meetings of divisions, commissions, and working groups; statutes and bylaws; membership (names only, though e-mail addresses are given for Division and Commission officers) by country and by commission; and reports of the actual GA events and of the actions of the Executive Committee between GAs.

Other items are of genuine importance to at least part of the astronomical community (redefinition of barycentric dynamical time and other coordinate issues, for instance). And there are a number of items one might glance at for fun: (*a*) a photograph of a working group (p. 156) captioned "Catalogue of Eclipsing Binaries", whose gender balance is that of an earlier generation; (*b*) Division XII (pp. 282–283), which took the record for number of working groups abolished (but Division XI created more new ones); (*c*) the US membership (p. 315), which exceeds that of the next four countries summed (yes, we pay more dues, but not proportionately so); (*d*) the best hissy fit [temper tantrum — Ed.] (p. 207), though the conclusion that particle astrophysicists do not regard the IAU as important to their activities is probably correct; and if it is any consolation to the former WG chair, they don't take the International Union of Pure and Applied Physics very seriously either; (*e*) two working groups (pp. 501, 503) with standard-sized organizing committees but, respectively, zero and one member each; (*f*) the suggestion (p. 221) that the present author can't count, had confused six organizing-committee members with 15 Commission members, and so was mistaken in thinking the latter number small compared with the several hundred members of several other commissions; and (*g*) the very impressive list of achievements (pp. 230–241) of the Commission on Astronomy Education and Development and its programme groups in establishing contact with potential new member countries and organizing schools for young astronomers around the globe. — VIRGINIA TRIMBLE.

How Spacecraft Fly: Spaceflight Without Formulae, by G. Swinerd (Springer, Heidelberg), 2008. Pp. 284, 24 × 16·5 cm. Price £15/$27·50/€19·95 (hardbound; ISBN 978 0 387 76571 6).

In recent years, considerable concern has been expressed in Europe and North America with regard to the falling number of students who are studying maths and science, and the predicted shortage of scientists and engineers in the future as the 'Apollo generation' reaches retiring age. Author Graham Swinerd admits to being one of the lucky few who lived through the birth of the Space Age, when some new feat of space exploration seemed to grab the headlines almost every week, and our knowledge of the Earth and the Universe began to be transformed by the latest engineering marvels to be carried aloft from Florida or the Soviet Union. Swinerd was so influenced by these historic events that he made a career of researching and teaching spacecraft engineering and design.

Many books and articles were written to mark the 50th anniversary of *Sputnik*, the world's first artificial satellite. Although this book is one of the more modest contributions to that large body of literature, I suggest that it offers far more 'meat' than the vast majority of its competitors. In these pages you can find a straightforward description ("without mathematical formulae", as the subtitle stresses) of more or less every topic that a budding spacecraft engineer would need to know. Hence, after a brief introduction that includes the discoveries of such luminaries as Kepler, Newton, and Einstein, the book concentrates on fundamental topics such as types of orbits, forces influencing orbits, rocket propulsion, the space environment, and the multitude of systems and subsystems associated with spacecraft design. Although most of the book concentrates on automated spacecraft, the requirements and problems linked with human spaceflight are briefly discussed toward the end of the book. Finally, Swinerd looks at possible technological breakthroughs that may one day transform space activities — ranging from space elevators and nuclear thermal propulsion to rather more fanciful prospects, such as rockets powered by nuclear fusion or antimatter engines.

This is certainly an interesting and highly readable volume which offers one of the best introductions to spacecraft engineering that I have yet come across. In conclusion, the author expresses the hope "that this book will play a small part in inspiring young people to get involved in space science and engineering" — a hope that I heartily endorse. — PETER BOND.

Space Conquest: The Complete History of Manned Spaceflight, by F. Dreer (Haynes, Yeovil), 2009. Pp. 208, 30·5 × 23·5 cm. Price £25/$49·95 (hardbound; ISBN 978 1 84425 573 3).

This heavy, coffee-table-style book is a translation of the French edition published in 2007 with a little updating. Over some 200 pages of text, the book takes you on a leisurely stroll into the past using an array of striking images to reveal the long and amazing history of manned spaceflight.

Those of us able to read this book will arrive at a point where you actually remember the events being discussed. I was pleased to discover that for me this does not happen until page 95, when the text reaches the *Apollo 11* landing on the Moon. I remember the next day being sat down at primary school in front of a television to watch a strange man in a funny suit climb down a ladder. Obviously that's an event we are now all very familiar with. However, the rest of the events, particularly early on, are somewhat dimmer in my mind, so I was glad to read about them.

Dreer starts right at the start with the birth of NASA and the rivalry between astronauts and cosmonauts. This leads gradually to the dominance, in terms of publicity as well as achievement, of the NASA projects: Mercury, Gemini, and Apollo. This was not a foregone conclusion given the amazing early leap forward in Russia led by their chief engineer, Sergei Korolev. His story is as amazing as that of the better known Wernher von Braun (see **129**, 160). The German/American NASA team had the advantage that they were unlikely to be sent to Siberia, or worse, if they failed.

It's a sad reflection of the facts that half the book is about the first ten years of manned spaceflight while the other half is about the following nearly-40 years. It always amazes me to realize how fast things moved in the early days compared to the seemingly glacial pace of some current projects. Things have been done since Apollo of course, like *Mir*, the Space Shuttle (both of them), and the *International Space Station*, all described in detail here. It is a little harsh to have only one page (167) on the Soviet shuttle, *Buran*. Having seen it up close in a rather sad display building in Sydney (now closed) some years ago, I can tell you it is very similar to the NASA version, but with one major extra capability — fully automated flight.

There is more than enough technical jargon plus sufficient facts and figures in the book to keep the most avid space-nut happy. Meanwhile the less enthusiastic can just look at the photos, which are very nicely reproduced, and read the main text. I should say that the translation is excellent. I appreciated the fact that having a French author leads to a slightly different perspective than one would get from (say) an American or a Russian author. Dreer is quick to give due credit but also points out the problems and issues that come with pushing the boundaries of what is possible. There are a number of tables giving flight details, rocket performance, and the like, but it is the main text and photographs that sell this excellent summary of where we have been.

The book ends on the more pertinent question of where we are going and also who will be driving, noting the ambitions of nations such as China. Now that the *International Space Station* is nearing completion, the challenge is to capture the public imagination by following in the footsteps of those brave, crazy, and highly skilled men and women described in this book. I wish the new crews luck and suggest you, and they, read this book to place it all in context. — PAUL O'BRIEN.

Handbook of Star Forming Regions: Volume 1, The Northern Sky, and **Handbook of Star Forming Regions: Volume 2, The Southern Sky,** edited by B. Reipurth (Astronomical Society of the Pacific, San Francisco), 2008. Pp. 1023 and 890, respectively, 23·5 × 15·5 cm. Price $77 each (about £50) (hardbound; ISBN 978 1 58381 670 7 and 978 1 58381 671 4).

This is a bold attempt to catalogue everything we know about every known star-forming region in our Galaxy. There is a chapter on each major region, written by an expert observer, or group of observers, who has studied that particular region. Some regions have more than one chapter devoted to them — Orion has twelve! Some minor regions are pushed together into a single chapter between them. The resulting tome was so large that it had to be split into two volumes, one for the northern hemisphere, and one for the southern. As far as I can see, no significant region has been omitted, and there are even some that I had never heard of.

A grand vision such as this should be heartily applauded. Whenever I observe a region with which I am unfamiliar, I can turn to this book to see what is known about it, and I can turn to the reference lists for further reading. In addition, whenever I take on a new student and give them a set of data to work on, I can point them to the relevant chapter of these books as a starting place for their background reading. Something as useful as this should surely have been done before. No doubt many have thought of it, but most have baulked at the enormity of the task. Not so Bo Reipurth. It appears that editing the massive *Protostars and Planets V* (*PPV*) conference proceedings did not put him off further large undertakings, but rather energized him to want to take on more.

It's a pity that there wasn't a conference at which all of these papers were presented — I for one would have signed up to such a gathering — because in some ways this is how this book reads: as a conference proceedings with only the invited review papers included. Consequently, it contains what would be most of the best parts of a typical proceedings volume. The quality of the contributed chapters is pretty high on average, with only minor exceptions.

There is not space here to comment on each chapter individually, although it is fair to say that all have pretty comprehensive reference sections, and it is generally hard to think of omissions, at least in the regions I know a little. I will just pick out a couple of such regions, one from each volume, to give a flavour of the book.

The aforementioned chapters on Orion begin with an overview chapter, which sets the big-picture scene not just for Orion but also for all nearby star-forming regions, including the origin of the Gould Belt. The remaining Orion chapters are split up in a fairly logical manner. They, in turn, detail pretty much everything that is known about each of these sub-regions. In fact so much is written here that one is almost tempted to think what is the point of observing Orion any more? Clearly this is not a sensible view-point, given technological improvements in cameras and spectrographs that occur all the time, but I can imagine some new PhD students being put off just by the sheer enormity of what is written here.

In the south I turned first to Ophiuchus, given that this is a region that I have studied for many years. My first impression was that not enough space had been devoted to this important region: only 30 pages, compared to 400 pages for Orion. This may be just my prejudice, but it does seem a little unbalanced. There is a great deal of information in this chapter, but much is missed out. Presumably the editor told each of the authors how much material he required for each region, and this was all that was asked of Ophiuchus.

My only other quibble is with the manner in which the authors were selected. There was no well-advertised, open, refereed competition, to which anyone who wanted to offer a chapter could apply, as there was for *PPV*. Authors were simply invited by the editor to write a chapter. Whilst this led to very many obvious suspects being called on (and in fact Reipurth himself contributes to several chapters), there are some chapters with notable omissions, and there is something of a bias to the American side of the Atlantic. However, these are really only minor quibbles, and I for one will be recommending our library to buy a copy of each volume. — DEREK WARD-THOMPSON.

Exploding Superstars: Understanding Supernovae and Gamma-Ray Bursts, by A. Mazure & S. Basa (Springer, Heidelberg; Praxis, Chichester), 2009. Pp. 168, 24 × 17 cm. Price £19·99/$29·95/€29·95 (paperback; ISBN 978 0 387 09547 0).

We're not supposed to judge a book by its cover, but here we shouldn't even take the title at face value. A large part of the content of this slim volume (the main text runs to just 124 pages) is concerned with cosmological implications and applications of supernovae, at the expense of any substantial discussion of, say, supernova remnants. This is natural enough, of course; the use of SNe Ia as standard candles, a growing understanding of the nature of gamma-ray bursts, and the emergence of consensus cosmology are surely among the most exciting and high-profile developments in astrophysics of the last decade or so*, and the three are related in obvious ways.

Nevertheless, pulling out the relevant threads from stellar astrophysics and cosmology, and weaving them into a single narrative, is not necessarily an easy task if one wants to go beyond the superficial, descriptive level of introductory texts. This is the challenge addressed by the authors (two very active researchers in the CNRS), whose successive chapters offer potted courses in observational cosmology; the formation of the first stars, and stellar nucleosynthesis; core-collapse and thermonuclear supernovae; gamma-ray bursts; and back to various broadly cosmological topics. Appendices cover relatively advanced topics (*e.g.*, degeneracy, quantum tunnelling, metrics), with a moderate smattering of equations, absent from the rest of the book.

The translation from the French original (performed by Bob Mizon, of 'Campaign for Dark Skies' fame) is completely unobtrusive, as a good translation should be. As a result, the eye glides so easily over the page that only the more outlandish claims act as a brake: "the greater part of the volume a [red giant] occupies is empty space …the density less than that of the best vacuum we can create on Earth", while fusion at advanced evolutionary stages requires that particles must "counteract the ever stronger repulsive forces between nuclei

*I drafted this review the day when it was widely reported in the media that a "Gamma-Ray Burst Smashes Cosmic Distance Record": GRB 090423 at $z = 8{\cdot}2$.

containing more and more electrons". Hydrostatic equilibrium is illustrated by the balance between gravitational and centrifugal forces, which isn't HSE as I understand it (are planetary orbits examples of hydrostatic equilibrium?), and equipotential surfaces in general (and Roche lobes in particular) surely aren't "places where the force of gravity has the same value".

Other than these occasional obvious gaffes, there's little to be too unhappy about in this book, but nevertheless I did come away with a vague sense of mild dissatisfaction, which I attribute to a variety of individually minor reservations. First, the figures: very many are disappointingly familiar, giving the impression of being often selected for convenience rather than relevance (even the mandatory 'cosmic budget' pie-chart is pulled straight off the web). They're in greyscale throughout, but a seemingly arbitrary selection is reproduced in a block of colour plates towards the end of the book. Secondly, who are the intended readers? Whoever they are, they evidently must have an interest in supernovae, yet require an explanation (mid-way through the book) of what a 'spectrum' is. Thirdly, and perhaps most importantly, I think the varied content could be better organized; to me, the book really fails to tell a coherent story, jumping back and forth between topics (for example, 'vacuum energy', although anticipated in Chapter 2, doesn't get a further mention until three pages before the end of the book).

In the absence of any serious competition, I'd be content to recommend this semi-popular account to undergraduates and serious amateurs as an accessible introduction to stellar-scale 'big bangs' and beyond, but a few corrections and a bit of editorial tweaking could greatly improve any future editions. — IAN D. HOWARTH.

RS Ophiuchi (2006) and the Recurrent Nova Phenomenon (ASP Conference Series, Vol. 401), edited by A. Evans, M. F. Bode, T. J. O'Brien & M. J. Darnley (Astronomical Society of the Pacific, San Francisco), 2008. Pp. 360, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 674 5).

Few conferences, and subsequent proceedings volumes, are devoted to a single astronomical object outside the Solar System. One of the few examples that springs to mind is the supernova 1987A in the Large Magellanic Cloud which, because of its relative proximity and the availability of space-age instruments for its study, became a seminal reference point of the study of supernovae. The recurrent nova RS Ophiuchi plays a similar rôle in the study of these phenomena. Since the outbursts recur, there will be repeated opportunities for detailed study, but the ~20 year intervals are not regular. Two (1967 and 1985) did take place in the era of space astronomy, but only for the most recent in 2006 has full coverage of the electromagnetic spectrum been available.

The workshop held at Keele University a little more than a year later, in 2007, was a timely opportunity to review the results of the many observations, and the proceedings published as part of the ASP conference series would be valuable for that reason alone. However, there is considerable added value from the way this particular volume has been constructed. It is divided into five discrete sections. The first three deal mainly with background material and incorporate a number of invited contributions that serve as review articles to set the scene for the fourth section, which is devoted to the 2006 observations and accounts for roughly half the book. The final short section provides some material on related systems.

This is a timely and useful book on the 2006 RS Oph outburst and essential reading for anyone interested in recurrent novae. Since the next outburst is likely to be another 20 years or so in the future, by default this volume will remain relevant longer than many proceedings. However, the strong editorial control of the structure and high quality of the finished product will ensure that this book will have truly lasting value in the time leading up to the next event. — MARTIN BARSTOW.

The Variable Universe: A Celebration of Bohdan Paczyński (ASP Conference Series, Vol. 403), edited by K. Z. Stanek (Astronomical Society of the Pacific, San Francisco), 2009. Pp. 190, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 682 0).

Bohdan Paczyński added at least half a dozen extremely valuable ideas to our astronomical inventory. Best known in recent years have been the certainty that gamma-ray bursters must be extragalactic (finally demonstrated more than 20 years after their initial discovery) and the possibility of looking for massive dark objects *via* gravitational microlensing (the search for which would inevitably also find a wide range of other sorts of variables). The former is addressed in two, and the latter in four, of the conference talks contained in this volume. There is also a serious discussion of black-hole accretion discs, including, of course, the optically and geometrically thick 'Polish doughnuts'. The other four items are essentially reminiscences of varying length and scientific content.

Walter Lewin shares a number of e-mails written by Paczyński between 1991 and 2003. Scott Tremaine reproduces the warm words he spoke at a memorial service that preceded the actual symposium. George Preston, undoubtedly the most informative, remembers Bep's first stay in the United States, spent at Lick Observatory in 1962–63, as part of a series of Polish visitors who held combined research and service positions. But Preston also discusses the work on binary stars and stellar evolution Paczyński did during the decade starting about then. The final conference talk and final book chapter are by your present writer (slightly censored by the editor, as one or two of the other contributions seem to have been), and contains a mix of significant science not elsewhere mentioned and anecdotes of reasonable veracity.

In addition to appropriate astronomical images, both old and new (including some pages of the documentation of the Paczyński stellar-evolution code), there are a number of photographs, the most touching without doubt of Bohdan and Hanka on their wedding day in 1964 and of him in a wheelchair at the time of his 2006 Russell Lecture for the American Astronomical Society, a little more than a year before he died. At 190 pages and $77, the book cannot exactly be said to be a bargain, but you might get a farthing's worth of fun trying to identify the other 37 astronomers who appear with Bep in a 1970 photograph of the summer staff of the Institute of Theoretical Astronomy in Cambridge. All are, I believe, still living, apart from Fred Hoyle (IOTA director), Willy Fowler, Ed Salpeter, and Phil Solomon. Margaret Burbidge was busy that day (though Geoff is there), so I am the only non-secretarial female in the picture. — VIRGINIA TRIMBLE.

Des quasars aux trous noirs, by Suzy Collin-Zahn (EDP Sciences, Paris), 2009. Pp. 480, 17 × 24 cm. Price €39 (about £37) (paperback; ISBN 978 2 7598 0377 4).

Should scientists themselves describe the progress of science? Certainly. And if historians of science generally have a sterling basic scientific knowledge, who else can best deal with the evolution of a field than a first-rank specialist?

This is what Suzy Collin-Zahn offers here. An astronomer at Paris–Meudon Observatory, she is an undisputed active-galaxies specialist. One might fear that an expert would use abstruse language or unfortunate shortcuts, but this is seldom the case in this book. Here and there, a non-specialist reader could be tested though, in spite of many insets and a number of appendices (including a *précis* to 'talk astrophysics').

It is not easy to tell for which readership this book has been tailored. Inquisitive minds for science will like it. Perhaps they will find some scientific themes a bit hard to comprehend and will prefer to learn about quasars in more introductory treatises of astronomy. In this volume, they will, however, discover the backstage of science. They will see that research is no quietly flowing river and does not progress linearly. It is made of hesitations, of failed attempts, of blockages, of recessions and spectacular progress, of false theories leading to endless discussions, of unexpected discoveries, and of impassioned controversies.

In many respects, this book will appeal to astrophysicists too. They will read with interest the history of quasars. The older ones will undoubtedly recognize many of the players mentioned and the events described. Those whose main field is distant from galaxies will find an opportunity to update their knowledge. On the other hand, it is virtually certain that the description of the difficulties of the author's scientific career will leave a bitter taste, albeit a familiar one. Many will find that the already gloomy picture she sketches could even be more darkened to reflect the power struggles, the jealousies, and the resentments that are met frequently.

The book chastens the rush for publishing and publicity. Press releases are continually issued to herald discoveries generally not deserving such an honour, and sometimes retracted shortly afterwards — a habit initially American, but eagerly adopted by Europeans. Science bows to media hungry for sensationalism and too much open to pseudosciences.

The author also exposes the shortcomings of the refereeing system, involving readers assessing the validity of manuscripts submitted for publication. She insists on the serious insufficiencies of the evaluation criteria for scientists, such as the citation indices. She rightly complains about the inconsiderate increase of administrative tasks eating up the researchers' time. However, research has always had its captivating and wonderful sides. The history of quasars — the core of this volume — amply provided such rewards. Discovered by chance in the early 1960s, quasars are exceptional objects. It took about twenty years to determine that their power originates in a giant black hole they host. It took twenty more years to become convinced that most galaxies, including our Milky Way, are also hosting one. The history of quasars illustrates very well the erratic advance of science. It shows how a field gets structured after half a century of meanderings, finally reaching a consistent physical model and a new understanding of the Universe.

With a sober presentation and monochrome illustrations, this book (written in French) is available for a reasonable price. The style is fluid and contributes to a captivating read. There are virtually no typos. — J. Manfroid.

Einstein's Telescope: The Hunt for Dark Matter and Dark Energy in the Universe, by E. Gates (W. W. Norton, London), 2009. Pp. 306, 24 × 16·5 cm. Price £18·99/$25·95 (hardbound; ISBN 978 0 393 06238 0).

Einstein's Telescope is a thoroughly interesting and well-written popular-science book about gravitational lensing. Describing General Relativity and having to dispel the deeply ingrained (but false) wisdom that light travels along straight lines could have made for a challenging subject. Gates' explanation of this adopts the usual devices of rubber sheets deforming space-time and triangles on spheres — but her version is engagingly written and easy to follow. The main part of the book then explores what the bending of light has told us about the Universe, concentrating on the dark matter and dark energy from the subtitle. We get an up-to-date discussion of an on-going debate. The book seems to have been written in late 2007/early 2008, and covers all the latest ideas: from neutrinos to supersymmetry, and quintessence to modified gravity. It is organized logically, rather than being constrained to strict chronological order, which makes for a gripping trail through the Universe.

My one small disappointment was the occasionally impersonal style of the text. This comes down to individual taste, but I would have enjoyed the tale of the hunters as well as the hunt. Since Einstein, there have been innumerable colourful characters in the field. I felt I missed out on their human stories when the descriptions reverted passively to how "physicists can do this" or how "that has been done by science".

Overall, it is a very enjoyable read, and I will happily recommend it. The profound and mysterious subject matter — the fundamental constituents of the Universe itself — was always going to make it interesting. But above and beyond mere discoveries, the importance of the quest is grippingly and persuasively conveyed. If every government minister and funding body were forced to sit down with the first page, they'd reach the last one. And we'd get ourselves a brand new telescope. — RICHARD MASSEY.

The Sun and How to Observe It, by J. L. Jenkins (Springer, Heidelberg), 2009. Pp. 224, 23·5 × 17·5 cm. Price £21·99/$34·95/€34·95 (paperback; ISBN 978 0 387 09497 7).

In recent years, the Sun seems to be the 'hot' topic as far as new publications go. There appears little that hasn't already been covered, so where does this book fit in? If you are thinking of taking the plunge into solar observing or a deeper dive into Hα or Ca II *K*-line observing or imaging, then you'll not be disappointed with Jamey Jenkins' comprehensive handbook. The author is quite obviously an experienced solar observer and imager, contributing to the AAVSO Sunspot Programme since 1990. He avoids the trap of angling his book primarily at the American market, making his advice readable and relevant to those outside the USA.

Anyone expecting a deep scientific explanation of how the Sun works will be disappointed, though. This book is primarily a practical guide to observing and imaging solar features in white light, Hα, and the *K*-line. There is a detailed discussion of the types of telescope suitable for solar observing as well as lenses and filters. The author goes on to describe white-light solar features, and how to observe, classify, and record them.

The latter half of the book is dedicated to observing the monochromatic Sun and again there is a detailed look at the various telescopes and filters available. How to photograph solar features is covered within the book's main text, reserving digital imaging to the book's final chapter. Explanations are given to unlock the mysteries of digital imaging, editing, and processing in a simple and uncomplicated style.

I found this book enjoyable and easy to read, providing a comprehensive practical guide to solar observing in one volume. — LYN SMITH.

Meteors and How to Observe Them, by R. Lunsford (Springer, Heidelberg), 2009. Pp. 207, 23·5 × 17·5 cm. Price £27·99/$34·95/€34·95 (paperback; ISBN 978 0 387 09460 1).

American amateur astronomer Robert Lunsford, with over 40 years of meteor-observing experience, has authored this addition to Springer's established *Astronomers' Observing Guides* series. The book is aimed at the beginner in meteor studies, so it quite reasonably has few surprises in its coverage, from sporadic meteors, showers known and annually strong, weaker, more variable, or daytime, to some of those merely suspected, before discussing the main observing techniques an amateur might employ. Naked-eye observing is heavily promoted, helpfully with especial emphasis on the need to collect data to international scientific standards.

Most of the shower chapters are liberally illustrated with radiant-location charts and meteor photos, the latter nicely demonstrating, in an understated way, the range of meteor images an amateur might expect, from the feeble to the sometimes spectacular. There are observing tips too, for example, valuable detail on when best to watch each shower, including from very different latitudes. However, the predominance of non-SI units and some of the text's construction suggest a rather less-globally-intended readership. The observing instructions are generally good, if brief and incomplete for some of the instrumental methods, and although there are few references to guide an enquirer further, there is a selection of group and society internet addresses in the final chapter.

Disappointingly, the explanations of jargon and more technical matters are sometimes too complex for the inexperienced (as in the 'Sporadic meteors' chapter), imprecise (*e.g.*, the scant meteor-physics notes on p. 2), absent (including why there are regularly-spaced breaks in many of the imaged meteor trails), or wrong (for instance, the 'Glossary' entry for "Zenith [*sic*] hourly rate", p. 189). The term 'radiant', and how radiants determine sporadic and shower meteors, essential for understanding all the text from p. 3 onward, remains undefined until p. 145! This all smacks of very poor editing, as do the frequent, if minor, typographical and lexical errors.

Overall, my desire to recommend this book, as one of few commercially-available, introductory, meteor-astronomy texts, is tempered by the flaws which detract from it fully informing its target readership alone. Even so, such newcomers would find much of it useful, and its shower coverage is sufficient to make it a handy work to dip into for more-knowledgeable amateur meteor enthusiasts. — ALASTAIR McBEATH.

CORRIGENDUM

On page 166 of the June issue of this *Magazine*, the information relating to **Sunspots and Starspots** incorrectly stated that the book was *edited* by Jack Thomas and Nigel Weiss when, of course, it should have said that they were the *authors*. The Editors apologise for this error.

EDITORIAL

PRICES FOR 2010

The Editors are pleased to announce that the price of the *Magazine* will remain unchanged for 2010, at £70 or US$140 for institutional subscribers. However, as careful readers will have noted (see **125**, 284; **126**, 308; and **127**, 368), problems with bulk deliveries to overseas subscribers have all too frequently resulted in losses and delays. So we have turned to Royal Mail to ensure reliable transmission to all destinations, with *all* overseas deliveries being sent by airmail at the 'Printed Paper' rate. But this comes at a price, and we are therefore having to levy *a supplementary postal charge for all overseas subscribers of £5*. At present, however, this will *not* apply to those who pay in US dollars, since the current (but still volatile) exchange rate permits the supplement to be covered in the basic annual subscription.

Personal subscribers will continue to get a lower subscription rate, which will be unchanged for 2010, on the condition that they undertake not to re-sell or donate their copies to libraries.

Here and There

THAT WOULD BE DIFFICULT

… the first man to do that was Arthur Eddington, Astronomer Royal and director of Cambridge Observatory, a position once held by Isaac Newton. — *Daily Mail*, 2008 November 16 (Must-see TV).

BRR!

With the onset of winter [on Mars], the Sun dropped low in the sky, and the temperature fell to −1,300C at night. — *The Daily Telegraph*, 2008 November 18.

LAX DRESS CODE FOR OBSERVING

My blazers were not available and so on a whim I wondered what it would be like to have a look at M16 … . — *A&G*, **49**, 6.12, 2008.

MORE LIKE UP THE POLE

… a world class astronomical observatory in Antarctica … (about 1500 km southeast of the South Pole) … — *EAS Publications*, **33**, p. IX, 2008.

SPACE TRAVELLERS BEWARE

Orion … glitters with blight stars … — *The Daily Telegraph*, December Night Sky.

TWICE THE MAN HE USED TO BE

…. J.E. Gunn, …, J.E. Gunn, … — from the author list *Mem. Soc. Ast. Ital.*, **74**, 978, 2003.

AVERTED VISION

The biggest full moon in 15 years. … with the brightness of the moon, stargazers are recommended to look away. — *London Life*, 2008 December 12, p. 18.