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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2010 November 12 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

O. LAHAV, *Vice-President*
in the Chair

The Vice-President. As Vice-President of the RAS, I am standing in for Roger Davies who unfortunately couldn't be here today.

The first item is quite special, the presentation of the RAS Gold Medal in Astronomy to Professor Douglas Gough, and it gives me great pleasure to present it on behalf of the Society — Douglas Gough was unable to receive it at the NAM 2010 in Glasgow, so we should award it on this occasion. I shall first read the citation.

The Gold Medal for Astronomy is awarded to Professor Douglas Gough of the University of Cambridge. Professor Gough has played an internationally leading rôle over more than four decades, making seminal contributions to our understanding of stellar astrophysics. Early in his career he recognized that the observed oscillations of the Sun's surface, so-called helioseismology, could be used to probe the solar interior. It was used to good effect by the space-based *Solar and Heliospheric Observatory* (*SOHO*), and the ground-based Global Oscillation Network Group (GONG). Professor Gough then applied the same technique to other stars, and not surprisingly coined the term 'asteroseismology'. And allowing astronomers an insight into the interiors of both the Sun and the stars, he understood that the structure was more complex than previously assumed. Alongside his research work, Professor Gough is described as a truly inspiring teacher, who has made extensive contributions to education and public outreach. For all these reasons, he is a most worthy recipient of this, the Society's highest honour, and with great pleasure I invite Professor Gough to receive his medal. [Applause.] Congratulations from all of us!

So now we are ready to start with the scientific part of this meeting. We have three distinguished speakers here, in fact all of them from the local neighbourhood. The first one I would like to invite is Dr. Hiranya Peiris, my colleague from University College London, who will tell us about 'Fingerprints of the early Universe'.

Dr. Hiranya Peiris. The cosmic microwave background (CMB) is the left-over heat from the Big Bang. When we observe this radiation, we see the Universe when it was only 370 000 years old. Now, 14 billion years later, it has cooled to microwave frequencies. The CMB is nearly uniform. The slight variations of one part in $\sim 10^5$ in its temperature, hotter or colder than the average, reflect initial inhomogeneities in the matter and radiation that later collapsed under gravity to form clusters and galaxies. Those fluctuations carry information about the origin, composition, and evolution of the Universe, and theories of the origin of the Universe make detailed predictions about their statistical properties. Given the extreme conditions in the early Universe, the CMB is our best hope of uncovering fingerprints of the physics operating at very-high-energy scales, inaccessible to Earth-bound particle accelerators.

But what created those primordial inhomogeneities? Current cosmological data are, for the first time, precise enough to allow detailed observational tests of models of the very early Universe. The initial conditions of the Big Bang are thought to have been set during 'inflation', a period of exponential expansion which might have caused the Universe to grow by at least a factor of 10^{26} in an infinitesimal time ($\sim 10^{-33}$ seconds). The expansion was driven by a hypothetical quantum field called the *inflaton*, which sourced *negative pressure* and *accelerated expansion*. The physical size of the Universe grew so much that it became much larger than the distance that light could have travelled since the Big Bang (*i.e.*, our observable horizon). Any inhomogeneities that preceded inflation were erased and the Universe became flat and smooth throughout our observable patch, in the same way that the surface of the Earth looks flat when viewed from a small aircraft, even though its global shape is spherical. However, the theory also predicts that tiny quantum-mechanical fluctuations in the inflaton field resulted in the perturbations imprinted on the CMB and the large-scale distribution of galaxies. This is the currently dominant theory for the generation of the initial inhomogeneities.

Most inflation models predict that the fluctuations are Gaussian with an approximately scale-free power spectrum. Both predictions have been verified with great accuracy by many CMB surveys, most notably the NASA satellite mission *WMAP*. Most of the current constraints have come from measurements of the CMB intensity, but even more information is encoded in its polarization; already the measured anti-correlation between the intensity and curl-free 'E-mode' polarization anisotropies indicates that adiabatic, super-horizon fluctuations were present when CMB radiation was generated. Improved measurements of the polarization, particularly the weaker divergence-free 'B-mode', are set to become the focus of CMB observations in the next decade.

ESA's *Planck* CMB satellite, which began scanning the microwave sky (from the distant second Lagrange point of the Earth–Sun system) in 2009 September, is the successor to *WMAP*: a leap forward in resolution, sensitivity, and frequency coverage. It has an ambitious goal: it is designed to extract all available information from the primary temperature fluctuations of the CMB as well as making a great advance in our understanding of the sub-dominant CMB polarization signal.

The fundamental microscopic origin of inflation is still a mystery. Basic questions remain unanswered, like: what is the inflaton?, what is the shape of the inflaton potential?, and why did the Universe start in a high-energy state? Data from next-generation cosmological surveys such as *Planck* will help us move beyond a phenomenological understanding of the early Universe, and find out the answers to such questions.

The Vice-President. Questions?

Mr. H. Regnart. Thank you very much indeed! I didn't quite manage to read your figures with the percentages for dark energy, dark matter, and baryonic matter. Could you say them again?

Dr. Peiris. I can tell you some generic figures, but they vary very slightly at about the per-cent level depending on what model you are fitting to the data: it's about 4% baryons, about 25% dark matter, and the rest, 71%, that's dark energy.

The Vice-President. You showed us the beautiful curve of multipole harmonics, and the quadrupole is discrepant — that has been known for a while, but people have worried a lot about it. Some say it is cosmic variance, others say something else. I guess you cannot tell us anything about *Planck* yet, but can you just give us your own assessment of the quadrupole problem?

Dr. Peiris. I don't think it's necessarily a problem, because even though the measurement is low by about 2σ, it's completely dominated by cosmic variance there, so to get the statistical significance is not really an issue. There are other issues at low multipoles, which might or might not be there, and they could be confirmed by *Planck*. If you have two different experiments with completely different setups getting the same thing, you can see whether there is a systematic issue. But, on the actual amplitude of the quadrupole, I don't think personally that there is a problem.

Professor D. Lynden-Bell. Since you were quite so clear in many of these things, could you explain to everybody why the dark energy and the dark matter cannot be made of atoms? [Laughter.]

Dr. Peiris. For dark matter, regardless of any other consideration, of course it can be made of atoms, because what you need is something like a dust, something with zero pressure. But we had very good constraints from Big Bang nucleosynthesis as to how many baryons have been synthesized in the early Universe; so for that particular case, we know that it can't be dark matter because there aren't enough baryons for the dark matter that cosmological measurements tell us that there is. For dark energy, it cannot be baryons, because we need the expansion rate of the Universe to accelerate, and for that we need a negative pressure; that is the only way to do it. Phenomenologically, a scalar field could do it, Einstein's cosmological constant could do it, but baryons cannot do it.

Professor P. Murdin. I'm very struck by the fact that the whitest area of your diagram showing where there is the best understanding is right on the cosmic microwave background, and that very early on we were very unclear, and now we're very unclear. Is that because this diagram comes from the *WMAP* team [laughter] or is there another reason for it?

Dr. Peiris. No, it comes from McMahon, and has been adapted by me. He is someone who builds CMB experiments, so perhaps it's not exactly the *WMAP* team but it is a CMB-related diagram. The physics of the CMB itself is not that complex: we are looking at early enough times at things that are very linear. The only thing that might be slightly harder to model is primordial magnetic fields, which are expected to be very, very small at those early times, to be compatible with modern observations in the late-time Universe. But the actual physics of the CMB, once you have potential wells which must be the source of dark matter, is basically baryon physics. It's photon scattering with electrons; that physics is very well understood.

The white region was meant to signify, really, the Standard Model of particle physics, which is extremely well tested in the régimes that are indicated.

The *Large Hadron Collider* is going to do measurements at about a TeV, and that's about where our direct probes of the Universe end. We can't do experiments about dark matter, because we don't know the particle nature of it; we can only do cosmological measurements of dark energy; and at those very early times we can't do experiments at all. So such a régime would represent the *International Linear Collider* times 10^{10}! And that might never be built. There is basically no Earth-bound experiment that can directly probe the physics of that era. So the only signal from that era to us now is what is imprinted in the CMB, or possibly in the cosmic neutrino background, but that is really, really hard to measure.

The Vice-President. If there are no further questions, let's thank Hiranya once more. [Applause.]

I would like to invite our next speaker, Professor Joanna Haigh, from Imperial College London, to talk to us. This will be on a topic a little bit closer to home, close to the present epoch, and that's on 'Solar irradiance variability and climate.'

Professor Joanna Haigh. Historically meteorologists have viewed the topic of how changes in the Sun might influence climate near the Earth's surface with some scepticism. It is clear, on time-scales of tens to hundreds of thousands of years, that changes in the Earth's orbit would affect the irradiance incident at the top of the atmosphere, and its distribution over the globe, but the influence of innate changes in the Sun, on decadal time-scales, seemed far less plausible. That the Sun showed varying activity was not in doubt, with the '11-year' sunspot cycle having been observed for many centuries, but measurements of solar irradiance, made from the Earth's surface, showed no robust correlation with sunspot activity. Indeed, even the sign remained in doubt — with the reasonable speculation that the presence of dark sunspots might reduce the total emission. Since the advent of satellite-borne radiometers in the late 1970s, however, it has become clear that total irradiance is greater when the Sun is more active, by about 0·1% at the maximum relative to the minimum of the 11-year cycle.

Based on simple energy-balance arguments, and with a knowledge of the sensitivity of climate to radiative forcing, this would translate into the global mean surface temperature experiencing a variation of around 0·1 K due to the solar cycle. Analysis of observational temperature records have concurred with this, but shown a non-uniform geographical distribution, with largest response in surface temperature (of approximately 0·5 K) in mid latitudes, and bands of warming extending through the depth of the troposphere (up to about 12-km altitude). Above this, in the stratosphere, greatest warming appears in the tropics. Zonal winds in the lower atmosphere show a solar-cycle response in which the mid-latitude jetstreams (and associated storm tracks) move slightly poleward when the Sun is more active.

Understanding the mechanisms whereby these patterns of response are established is important in terms of assessing the rôle of human activity in past and future climate, as well as providing a good testbed for current understanding of atmospheric dynamics and physics. Mechanisms for a solar influence on the lower atmosphere can be broadly grouped into 'bottom-up' effects, involving heating of the Earth's surface mainly by visible and near-IR radiation, and 'top-down' effects, in which solar ultraviolet radiation heats the stratosphere and the downward influence takes place by dynamical coupling. Previously at Imperial College we have shown that the observed patterns in temperature and wind may be qualitatively reproduced by experiments with climate models in which solar UV is enhanced. In those experiments Earth surface temperatures were fixed, so that no changes in the temperature of the surface could be introduced from heating by enhanced visible-wavelength radiation. Furthermore, the amplitude of the signal was found to be enhanced if ozone concentrations in the stratosphere were allowed to respond to the increased solar UV. The magnitude of the modelled response, however, was smaller than that observed. From those experiments we concluded that UV heating of the stratosphere makes an important contribution to the solar effect on climate, and that the magnitude of the UV change and, importantly, its effect on ozone, were crucial.

For these reasons we have been particularly excited by the advent of the *SORCE* satellite. Since 2003 it has been making the first-ever measurements across the solar spectrum from X-ray through UV, visible, and near-IR wavelengths, at high spectral and temporal resolution. In particular, daily measurements by the *SIM* instrument of the spectrum between 0·2 and 2·4 μm provide a new perspective for solar radiative forcing of climate. Calibrated data are now available for the period 2004 to 2007 from the *SORCE* team at the Laboratory for Atmospheric and Space Physics, University of Colorado at Boulder. The peak of the most recent solar cycle occurred in 2000–2002, and from then until about 2009 December the Sun's activity declined; thus the *SIM* data represent a period of lessening total irradiance, also measured independently by the *TIM* instrument on *SORCE*.

The difference between 2004 and 2007 in solar spectral irradiance measured by *SIM* was quite unexpected, being unlike that predicted by the multi-component empirical models (based on activity indicators such as sunspot number and area) which were previously in common use. The *SIM* data indicated a decline in ultraviolet from 2004 to 2007 by a factor of four to six larger than previously thought, and an *increase* in visible radiation, compared with an expected small decline. Independent measurements in the 116–290-nm range made by the *SOLSTICE* instrument on *SORCE* also indicate substantially more ultraviolet variability, and the spectrally integrated *SIM* data are consistent with the *TIM* measurements.

We have used a 2-D (latitude–height) model of the atmosphere, with fairly complete representations of radiative and photochemical processes but parameterized dynamics, to assess the possible influence of these changes in solar spectral irradiance on the structure and composition of the middle and lower atmosphere. With the accepted empirical solar spectra, the 2-D model shows values of stratospheric ozone larger everywhere in 2004, relative to 2007, with greatest impact about 0·7%, near 40 km altitude. With the *SIM* spectra the 2-D-model ozone concentration is up to 2% greater near 35 km in 2004 relative to 2007. Furthermore, the model results showed 1·5% *less* ozone near 60 km, due to catalytic destruction by the OH radicals produced when excited oxygen atoms, produced by enhanced UV radiation, react with water vapour.

To assess whether either of those responses was consistent with actual ozone behaviour over the same period, we performed a statistical analysis of contemporaneous data from the *MLS* instrument on the *Aura* satellite. The short time period makes precise attribution to solar effects difficult, but the best fits showed results more consistent with the *SIM* simulations than with those using the empirical spectra, presenting a solar-induced decline in ozone of a few per cent, from 2004 to 2007, near 35 km, and an increase of about 1% near 60 km. Given statistical uncertainties those results cannot be used to confirm the values of the *SIM* measurements, but by no means do they draw them into question.

In terms of the radiative forcing of climate change, it has been established that, in predicting the potential change in global mean surface temperature, the most robust measure of radiation is the change in cross-tropopause flux. We thus investigated the radiation crossing the tropopause in our 2-D model experiments. With the empirical spectra at the top of the atmosphere, the flux of radiation at the tropopause was 0·08 W m^{-2} greater in 2004 than in 2007, consistent with previous studies. With the *SIM* spectra, however, the flux was 0·1 W m^{-2} less in 2004, representing the change in visible/near-IR radiation because the UV flux was largely absorbed in the stratosphere. This gives a rather surprising conclusion: solar radiative forcing of climate is out of phase with solar activity, at least over that three-year period.

In terms of the processes involved in determining the impacts of solar variability on climate, we conclude that the bottom-up mechanism, driven by visible/near-IR radiation, would act in the opposite sense to that previously thought, while the top-down mechanism, driven by UV changes, would be much larger. We await with some impatience *SIM* measurements of the spectral composition of solar irradiance as the Sun emerges from its extended period of low activity.

The Vice-President. Thank you very much. Questions, please?

A Fellow. Will *SIM* and *SORCE* or similar instruments be continuing to collect data?

Professor Haigh. Yes, this is of course the really intriguing thing, and the scientists in Boulder are busy calibrating the most recent data — the solar activity is just going up, the visible radiation has just started to go down — the instrument could still be wrong, but we've got to wait and see.

A Fellow. How long will it be operating for?

Professor Haigh. A few more years.

A Fellow. Joanna, that long-term trend in the solar irradiance you didn't want to talk about; you said it was controversial. Is that because people can't agree that it is actually there, or is it because they can't agree about whether it is important, or forcing a general warming trend?

Professor Haigh. It would definitely be important, although its shape is not that it could actually explain all of recent global warming. Deriving the long-term trend requires making some assumptions, and solar physicists appear to disagree about these. For example, if you take in different components of solar irradiation coming from sunspots, from faculae, and from the network on the Sun, you can get different balances in how much it would have gone up given certain sunspot coverage of the Sun in the past. That plot I showed is from Judith Lean, who is one leading authority in this area, but others, like Sami Solanki from the Max Planck Institute, think it's slightly larger. I don't believe any of the solar physicists think it is big enough that you could explain global warming.

Professor D. W. Kurtz. Historically the proxies for temperature and the solar activity looked as if they were correlated, yet you are suggesting they should anti-correlate! I am confused!

Professor Haigh. Yes, they should be anti-correlated, if that variation over the three years applies over solar cycles.

Professor Kurtz. No, but what I'm saying is that the historical record looks correlated, not anti-correlated.

Professor Haigh. Yes, in the long-term the regression would have to do the opposite but it is not obvious that changes on 11-year-cycle time-scales would apply to longer-term solar variability.

The Vice-President. If there are no more questions, let's thank Joanna once more. [Applause.]

The Vice-President. It gives me great pleasure to introduce the next speaker, Professor Steve Miller, from University College London. Steve Miller has combined in his career research in atmospheric physics, together with communication of science, so it's most appropriate that he is delivering this year's Harold Jeffreys Lecture. So I would like to invite Steve to give his talk, which has the intriguing title, 'Do extrasolar planets go bang?'

Professor S. Miller. [It is expected that a summary of this talk will appear in *Astronomy & Geophysics*.]

The Vice-President. Thanks very much for a fascinating talk, taking us not only from molecules to extrasolar planets, but also illustrating that nature somehow connects the A and G parts of this Society more than it usually seems! Let us take a few questions, please.

Mr. M. F. Osmaston. I think the question you are posing actually may not occur, in the sense that I have been developing a picture of these exoplanets where we are seeing the star and the exoplanet shortly after it emerges from its obscuring dust cloud; in that situation, the exoplanet would not have been able to see the adjacent star, so it would have been able to nucleate, quite cool, in that environment. This is why we see so many planets that have been discovered to be within 10 stellar radii of their host stars; this seems to be a possible solution. They may actually go off bang or not, according to your view, because they are now exposed to their star, and for us to see.

Professor Miller. Yes, but a lot of people still hold to the view that hot Jupiters formed in regions similar to where we have Jupiter now, and migrated in. And I'm afraid I'm not sufficiently knowledgeable about the details of those kinds of migration and formation models — or your model — to be able to differentiate between the two. But we do know that Jupiter did not migrate in, or not very far. And we do know that the Sun would have been incredibly active about 10 million years after it switched on, and therefore, whether or not H_3^+ saved the day, in my view it is still a reasonable question to ask the people who want to model the formation and evolution of Jupiter itself.

Mr. Osmaston. The problem is there only if you regard the star and the planet as being coeval in age; in 51 Peg, hosting the first exoplanet of this type that was discovered, the star is eight billion years old, but the planet couldn't possibly be that old — not in that environment.

Professor Miller. I think this is clearly an area of controversy that is outside my expertise, and I would like to suggest people like James Cho and John Papaloizou discuss with you about formation scenarios.

The Vice-President. Other questions? Yes, Hiranya.

Dr. Peiris. Can you go back to your slide that had the *Keck* spectrum of H_3^+ emission in Jupiter's aurora? So you said that the vertical bright bands extend off the limb of the planet.

Professor Miller. They do, yes!

Dr. Peiris. Is there any significance to the fact that the extension is asymmetric?

Professor Miller. Yes, the significance is that the auroral oval is asymmetric. There is an offset of about ten degrees between the rotational pole and the magnetic pole, so we happen to have caught the auroral oval where the aurora is brightest, on the dusk limb — because there we are really cutting through the auroral ovals, whereas our slit actually misses the auroral oval on the dawn limb. But there's still a lot of H_3^+ around there: we're getting very high concentrations of H_3^+ where we get the main auroral oval. And just incidentally, the reason the temperature drops is because this is where the most powerful energetic electrons are precipitated, and they penetrate deeper into the atmosphere, where the temperature is going to be lower, because we are in the thermosphere, where we've got a monotonically increasing temperature profile rising with altitude. So thank you for asking that question because I was able to get something else in as well!

Dr. D. McNally. Steve, you've made H_3^+ sound a wonderfully useful molecule. Is it still true that you cannot twist it into any shape or form that would be useful with respect to the diffuse interstellar features (DIBs)?

Professor Miller. I'm almost certain the answer to that is 'yes'! It has a very complicated spectrum, and I showed all those different types of transitions; of course, in basic rovibrational spectroscopy, only the fundamental spectrum is allowed, overtones are not allowed — yet H_3^+ has overtones, hot overtones, double overtones, and forbidden bands. So it does all sorts of weird and wonderful things because it's very un-harmonic and doesn't obey classical spectroscopic rules; but I just don't think you could bend it to fit the DIBs, I'm afraid. If I can, I'll let you know.

Professor Lynden-Bell. We know that the decametric radiation from Jupiter, related to Io, only really occurs *vis-a-vis* the north pole of Jupiter, rather than the south pole; you know, it's when the north pole sweeps past at 90 degrees, and when the north pole sweeps past at 240 degrees CML, that you get a burst from Jupiter. Now, do you see the auroral spot from Io just as well in the southern hemisphere as you do in the north?

Professor Miller. The answer to that is 'no' for the aurora and 'no' for the Io spot, and the reason that you see the northern aurora much more clearly, when you get decametric bursts, is that it's the tilt *plus* the offset from the centre of the planet; so that makes it always easier to see the northern auroral regions rather than the southern auroral regions. I mean, they go backwards and forwards, sometimes we see them very well displayed. If I go very quickly back to that movie, then you can see the northern aurora coming into view as it rotates out of the limb of the planet, whereas the southern auroral oval is always near the terminator of the planet. So that's why you do see the bursts from the northern auroral regions rather than the south.

The Vice-President. Good; if there are no more questions I will ask you to join me in thanking Steve once more. [Applause.]

Just two brief closing remarks: first, may I remind you that the usual drinks reception is in the RAS library — all are welcome immediately after this meeting; and second, to mention that the next monthly A&G open meeting of the Society will be held here on Friday, 2010 December 10.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2010 December 10 at $16^{h}\,00^{m}$
in the Geological Society Lecture Theatre, Burlington House

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

The President. I have sad news to deliver at the beginning of this meeting, to announce the deaths of former Fellows. Professor Adriaan Blaauw, who was the first ESO Director General and Emeritus Professor at Leiden and Groningen, and an Honorary Member of the Society, died on 2010 December 1. Dr. Allan Sandage, who was an emeritus staff member at the Carnegie Observatories in Pasadena, was awarded the Eddington Medal, and was also an Honorary Member of the Society, died on November 13. Dr. Brian Marsden, Director of the Minor Planet Center (MPC) from 1978 to 2006 and holder of the RAS medal for Services to Astronomy, died on November 18. And I heard yesterday that Professor Donald Blackwell died on December 3; Donald was a former President of the Society. Finally, Professor John Baldwin, who was a Fellow of the Society, died on December 7. Please now rise for a moment's silence. Thank you.

That takes me to the programme of the talks, and our first speaker is Colin Snodgrass from the Max Planck Institute for Solar System Research, Katlenburg-Lindau, and he will be speaking about '*Rosetta*'s view of an asteroid collision'.

Dr. C. Snodgrass. In 2010 January the LINEAR survey discovered P/2010 A2, an object with a strange comet-like appearance but an asteroid-like orbit in the inner main belt. Initially thought to be a new example of the recently identified population of main-belt comets, its strange shape (a 'headless comet') led to suggestions that this was actually the trail of debris from a recent collision. Its orbit, closer to the Sun than the other known main-belt comets, also suggested that water-ice-sublimation-driven activity was unlikely. Our team obtained images of the trail with the 3·6-m *New Technology Telescope* at La Silla and the 5-m *Hale* telescope at Palomar. Others took data with a variety of telescopes, including the largest ground-based telescopes and the *Hubble Space Telescope*. The *HST* images (obtained by D. Jewitt *et al.*) were particularly impressive, revealing a complex X-shape at the head of the trail and a nucleus of around 120-m diameter separated from the dust trail. However, the trail shape evolved very slowly, and consequently either cometary or collision models could be used to explain its appearance.

The reason for the slow change in the observed morphology was the relatively small change in viewing geometry from Earth over the weeks and months after the discovery. We could see only the 2-dimensional projected shape of the trail on the plane of the sky. To get a 3-dimensional picture of the trail, we obtained images from an entirely different position, using the *OSIRIS* camera on board the ESA *Rosetta* spacecraft. *Rosetta* was entering the asteroid belt for its fly-by of asteroid 21 Lutetia in 2010, and although it was not particularly close to P/2010 A2, the difference in observing angle gave us the necessary stereo view. Using the small *OSIRIS* cameras as space telescopes presented some technical challenges, as they are designed for close-range imaging and not telescopic work, but we were able to obtain a low-resolution image of the dust trail and find its orientation.

We used the technique of Finson & Probstein, which is commonly applied to comet tails, to model the motion of dust particles and find scenarios that reproduced both the Earth-based and *OSIRIS* images. The only solution that matches what was observed from both viewing positions is an instantaneous ejection of dust (interpreted as most likely to be a collision) within a week of 2009 February 10. This ruled out any extended cometary activity and showed P/2010 A2 to be the first directly observed collision between asteroids. This finding was supported by independent results from high-resolution *HST* images taken over a number of months.

Collisions between asteroids are an important process in the evolution of the Solar System: building asteroids in its early history and subsequently shaping and eroding them, producing dust and small fragments that eventually reach Earth's surface as meteorites. Until recently such collisions had only been inferred from the record they left behind (asteroid families, dust bands, craters seen in spacecraft-based imaging, *etc.*), not directly observed. We were able to use these observations to constrain a number of properties of this collision.

The President. Thank you very much. Any questions on asteroid collisions?

Professor D. W. Hughes. I'm a bit worried about your 9×10^7 objects smaller than 120 metres: when I calculated it I got a value of about 1000 – 10 000 more than that. Would that screw up your theory altogether?

Dr. Snodgrass. No, it's all order-of-magnitude stuff [laughter].

Professor P. G. Murdin. Are there any pre-discovery observations?

Dr. Snodgrass. The LINEAR team looked back in their data and found it a couple of months before the discovery images, but at the time of the actual collision it was about 20° from the Sun so no-one would have seen it and we don't think it was in the field of view of any of the solar satellites.

Dr. J. G. Morgan. How bright in the infrared would the collision have had to have been, and have you managed to check if any detectors were looking in the right direction at the right time?

Dr. Snodgrass. Well, again it was too close to the Sun for anyone realistically to be looking there at the time. I don't know how bright it would have been in the infrared.

Professor A. Fitzsimmons. This is just a follow-up to Colin's comment there. As soon as Colin and his team verified the date of the collision plus or minus five days, we went back and checked — this thing has done an amazing escape act by not being visible to anything we had. So first it was out of view of the *LASCO* coronagraph on *SOHO*. Then we have the twin *STEREO* spacecraft looking at either side of the Sun. But due to their relative positions the asteroid was on the wrong side of the Sun from both *STEREO* spacecraft so we couldn't see anything.

Dr. G. Q. G. Stanley. Can you, after that collision, see any spin on the nucleus?

Dr. Snodgrass. Unfortunately it is so faint that even with 10-metre telescopes or with *HST* we were stacking up hours of observations to get the nucleus out, so I don't think anyone has the time resolution to get the light-curve on it. We considered applying for *VLT* time to test whether it could be a rotational break-up or could it be spinning fast, but a 120-metre-diameter asteroid could be spinning in a matter of minutes and be stable. For something of 120 metres' diameter you need at least a 5-minute integration even to see it, so you can't measure the rotation if it is faster than that.

The President. Thank you very much, Colin. [Applause.] Our second speaker will undoubtedly have something of great importance to say for our third speaker. Our second speaker is Rick Tanner of the Health Protection Agency and he is going to tell us about 'The HAMLET project, the determination of the radiation exposure for astronauts'.

Dr. R. Tanner. The HAMLET project is the Human Model Matroshka for Radiation Exposure Determination of Astronauts and is funded by the European Union with collaborations in Germany, Poland, Austria, Hungary, and Sweden. I belong to the Health Protection Agency, which is the UK involvement in this project. The object of the contract is to improve the radiation-risk assessment for the long-term exploration of space.

We now have a long history of people going into space and to the *Space Station*; in the future we expect that they will be going to Mars. To date 511 people have travelled in space; the longest single flight by an individual was more than a year and the longest accumulated time in space is more than three years. Most of the astronauts have been Russian or American, and the *International Space Station* (*ISS*) currently houses six astronauts.

The UK provides a component of the European dosemeter which measures the radiation dose received in space. There are several schemes for getting men to Mars but the journey is likely to take between 545 and 920 days. Unfortunately, Mars does not provide the shielding afforded by the Earth's magnetic field and atmosphere, so the radiation-dose rates on the surface of Mars are significantly higher than those on Earth.

The most penetrating radiation is high-energy cosmic rays which come from supernovae outside the Solar System, but moderated by the solar cycle. These are heavy, charged particles, difficult to shield, and are very damaging. In terms of atomic weight, hydrogen is most abundant but some common heavy nuclei such as iron may contribute quite a bit to the dose experienced by astronauts. Other components come mainly from the Sun, including the particles of the solar wind — electrons, protons, and helium nuclei, which again vary with solar activity. More erratically, solar flares and coronal mass ejections can increase the dose rate by a factor of 1000. Astronauts have about 20 minutes' warning of such events, so they can get back inside the *ISS*.

The magnetic field of the Earth attracts protons and electrons to the van Allen belts. The *ISS* travels through these and they are a major part of the received dose. The particle energy at the *ISS* can cover over eight orders of magnitude from cosmic rays downwards. Another source of radiation comes from the spacecraft itself. The hull is made from aluminium with a density of about 5 gm cm^{-3} and this can create a source of secondary radiation from the incoming primary flux, such as pions, muons, *etc*., as can the astronauts themselves. In terms of shielding, NASA prefers aluminium but the dose rate increases when primary cosmic rays are encountered. Lead is actually worse due to a build-up of radiation. An electromagnetic field would help, but even better would be liquid hydrogen, as this would lead to fewer interactions.

What does this mean for radiation protection? We mainly deal with workers in the nuclear industry and hospitals, and space forms a small part of our remit. We investigate how cancer cells are produced. High doses of radiation can lead to nausea, cataracts, and skin reddening. In terms of risk assessment, everything is based on the absorbed dose, which is a measure of the energy deposited in the tissue. A dose of 1 sievert of radiation means a 5% chance of getting cancer, and this scales linearly with the dose. As an example, a radiation worker on the ground will typically receive a few milli-sieverts per year, or less. Dose rates depend on altitude; at ground level it may be typically 0·1 micro-sievert per hour. This doubles at 3-km altitude but is 40 times higher at 10-km altitude, and where the *ISS* flies the dose rate is around 21 milli-sieverts per hour, which is appreciable. NASA has its own methods for defining lifetime safe limits, which are different for males and females. For higher altitudes than the *ISS* we lose protection from the Earth's magnetic field and get the full effects of Galactic cosmic rays which may affect the central nervous system. As yet the acceptable level of risk above low Earth orbit is not yet defined either by NASA or the International Commission on Radiation Protection (ICRP).

Current research involves using models of the human head and body, called Matroshka, both outside and inside the *ISS*. They are simulation space suits which contain 6000 passive thermoluminescent detectors and seven active ones. We are building up data from both inside and outside the *ISS* and the data are being analyzed independently by labs in Germany, Austria, and Hungary.

We find that the dose rates are higher over the South Atlantic Anomaly where there is a lack of magnetic field. We are estimating the neutron impact at 40 and 70 MeV on the outside of the Matroshka facility. In one year it receives the ground equivalent of 20 years on Earth, so whilst individuals on Earth receive an insignificant dose, in the *ISS* the neutron flux constitutes 25% of an astronaut's received dose.

In conclusion, we have enhanced our understanding of HAMLET's dosemeter responses, which has helped us to determine the dose deposition within astronauts, and hence we have a much better understanding of the radiation exposure of long space flights. [Applause.]

Mr. J. Stone. Are you aware of the work done by Kim Ward of Rutherford Appleton Lab: he was at the space conference yesterday presenting a paper showing how a small magnetic field could produce a proportionally larger-than-expected deflection of some of these particles.

Dr. Tanner. This was a 45-minute presentation that was cut down, and that was one thing that got cut out; they work just across the road from us so we know that that is one option for long-distance spacecraft.

A Fellow. Are the detectors that are carried on aircraft of a similar nature to the ones you are instrumenting on the *Space Station* in comparison to the high-altitude regular flights?

Dr. Tanner. Airlines are quite sensitive to having too much conspicuous instrumentation on their flights. Concorde had monitors on it the whole time — the dose rates were twice as high but you got there twice as fast, so the dose wasn't really any bigger. But somehow the airlines have got away from all of their staff wearing dosing jackets, which they probably should do really; they think of them as detrimental to the appearance of their uniforms. So although routinely aircraft are not monitored there are programmes to estimate the real doses of the staff and crew. Essentially, when measured they are the same as all dose rates.

Mr. H. Regnart. Do you anticipate at this time, in advance of being able to give a definitive opinion, that it is likely to be safe for humans to go to Mars and back?

Dr. Tanner. The radiation risks would not be deemed acceptable for someone in a normal job. Of course there are other risks as well, but whether this is the biggest risk is another matter. The estimates are about 5% chance of cancer for going to and from Mars, but it depends on how long the flight takes and the shielding methods employed on the way. I don't think that it will stop us going to Mars.

A Fellow. If you have a hydrogen spaceship, the answer is then to put the cabin inside the fuel tank. [Laughter.]

The President. Let us thank Rick again. [Applause.] The next speaker is Piers Sellers, who is a British-born astronaut, and he is going to tell us about 'Getting to orbit and staying there'. Before we start that talk I want to say that we are being joined by the Friends of the Royal Astronomical Society and I would like to welcome those people.

Dr. P. Sellers. I'm learning a lot tonight. If the asteroids don't kill you then you'll get cancer! [Laughter.]

I was on the last shuttle mission in May this year; it was the 32nd flight that *Atlantis* had made, and it took six of us and a large Russian module to the *International Space Station* (*ISS*). The crew was composed of Mike Good, a US Army colonel who is a flight-test engineer; Dr. Garrett Reisman, who is a mechanical engineer from Cal Tech; the shuttle pilot was Tony Antonelli who is a test pilot and flies F-18 jets for the US Navy, as does Ken Ham, who was the commander of this mission; Steve Bowen is, uniquely for the astronaut corps, a submariner, and a specialist in nuclear engineering; I am a climate scientist. We are supported by hundreds of people at Mission Control in Houston and Kennedy Space Center in Florida.

I'd like to say a little about training, the most difficult part of which is space walking. The training facilities include the neutral-buoyancy lab which contains a tank of water 200 feet long, 100 feet wide, and 50 feet deep. A mock-up of the *ISS* sits at the bottom of the tank and we practise adding new modules dressed in old space suits which weigh 250 lbs. We have to be lowered into the tank by a crane but once in the water we are weightless. Virtual-reality techniques are also used to enable single astronauts or pairs of astronauts to simulate the actions needed to carry out the various repairs and additions, and it allows them to practise interacting with one another whilst carrying out assigned jobs.

Launch is always a challenging business because we are still using chemical propellants. The shuttle itself is the most complicated machine ever assembled — it contains three million parts, many of which, such as the on-board computers, are duplicated or triplicated. The shuttle is also the first fly-by-wire aircraft and the technology used is now in general use in aviation, but when it was developed it was far ahead of its time. It weighs 140 tons and it has two engines in the back for orbital manoeuvring and re-entry and a further three larger engines for lift-off. The fuel is not stored on the shuttle but in a large separate tank which is mated to the shuttle on the launch-pad. The weight of all this requires a further two booster rockets which run on solid propellant in order to get airborne. They can't be switched off! The weight of the whole assembly is 2400 tons, of which 2200 tons is highly volatile fuel, so then everyone not flying retires to a distance of three miles. [Laughter.]

The main engines start first and once they are running correctly the two solid boosters are fired up and the retaining bolts released. The whole craft then rotates on its vertical axis and heads down range to the orbital plane of the *ISS*, which is at 51 degrees to the equator. After two minutes of boneshaking ride the solid boosters are ditched over the Atlantic, to be retrieved and re-used. After another six minutes the main engines are cut, and after that the main tank is jettisoned. The shuttle then heads for the *ISS*. In 2006 when I visited the *ISS* it weighed 100 tons and had one solar array. This year it is nearly finished, weighs 500 tons, and is home to six people. We are hoping that we will get 15 years of use out of it. The orbital crew are always pleased to see the shuttle arrive — it means a supply of fresh food! The present crew consists of three Russians, two Americans, and one Japanese.

In the mission that I was on, the shuttle bay contained all the new equipment to be fitted to the *ISS*, and its 65-foot arm was used to move it from the bay towards the *ISS*. The module to be fitted weighed eight tons and was the size of a school bus. It is to be used for storage and contains a small laboratory. The astronaut operating the shuttle arm can see everything *via* a panoramic cupola fitted to the *ISS*, including the whole shuttle bay.

Sleeping is difficult; some people attach themselves to bulkheads by Velcro straps. One guy has been known to sleep unattached and it is quite spooky to bump into him in the middle of the night. [Laughter.]

After seven or eight days it was time for us to return to Earth. An engine burn was carried out over the Western Pacific to drop out of orbit, and landing took place about 40 minutes later. The shuttle was now a glider and approached the landing strip at an angle of 30 degrees. Landing took place at 250 mph, about twice the speed of a conventional aircraft, and if it didn't stop at the end of the runway then it would have ended up in the large alligator pond. [Laughter.] [At this point the speaker showed a 15-minute video which showed the STS-132 mission from launch to landing.]

I expect that next year will see three final missions involving *Atlantis*, *Discovery*, and *Endeavour*. So, was it all worth it? I think so: we got the *ISS* and the *Hubble Space Telescope* out of it. There will be a bit of a gap before the next US national spacecraft appears.

Before I finish I would like to hand over this RAS medal to the President. The Society asked us to take it into space, and this we did on STS-132. Thank you very much. [Applause.]

The President. Thanks, Piers. Astronomers are renowned for being well travelled but I'm sure that it will be many many years before anyone has travelled more than that medal. Thank you so much for bringing it back safe and sound to us. We'll continue to award the Jackson-Gwilt medal many times in the future but that particular one will remain in the Society's apartments here in Burlington House. I'm sure there will be an enormous number of questions.

A Fellow. Do you vent the cargo bay all the way up and are the satellites and things inside it vented?

Dr. Sellers. Yes, there are electrically operated doors on the side and they are opened on launch and all the air floods out and it's a vacuum when you get into orbit. When you come back there are periods when they are closed to stop plasma getting into the payload bay and then opened again quickly to get air in to equalize the pressure all the way down. An interesting thing I've just remembered here: as soon as we get into orbit we open the payload-bay doors. If we don't we would come straight back down because the thermal radiators that do all the cooling are on the inside of the doors, so we have to open them and expose them to space. A neat bit of design.

Mr. Regnart. Out of consideration for others I'll be ultra-brief. From the age of five I have had a passion for space flight; I had an imaginary trip to Mars and back for tea at that age. But the only way I could now justify human space flight because of the cost, the danger, and the ecological overall effect on the Earth of such an expenditure of resources, is as an insurance policy if we had to protect ourselves from an impact with a bolide, comet, or asteroid, as a backup in case mechanical robotic means were not adequate for avoiding a collision, or possibly even to use a spaceship or ark if we wreck the Earth completely.

Dr. Sellers. Well, Stephen Hawking has recently started talking about how it would be nice to have humanity spread out a bit thinner in the Solar System. We are in the dug-out-canoe phase of human space exploration and we are obviously going to get better at it the more we practise.

Dr. Stanley. You are speaking to a room full of jealous people here! When you are going into orbit, when do you feel zero gravity? Does it grow gradually or do you suddenly realize it?

Dr. Sellers. There are three phases to launch. First phase is when you still have the solids on, and these things crackle and pop and you can feel the vibration and you are being banged around in the cockpit, but you also feel the G's build up from about 1·2 up to about 2 when the tanks fall off, and then the G's slacken off a bit because you have lost that part of the thrust. Then you accelerate up to 3 G and you are being really mashed in your seat and it's hard to breathe; it can be likened to having two people sitting on top of you. But the engines throttle to keep you at 3 G so you don't exceed the stress limits of the vehicle, which is only rated to 3·5 G; the margins are really small there. So there you are roaring along at 3 G towards main-engine cut-off and when that happens the engines just stop, you are at 3 G and then ... nothing, and suddenly everything is floating around the cabin.

A Fellow. I have been responsible in the past for lighting space stations in TV productions, so I am fascinated to know how bright the Earth light is, because I'm not sure if I put enough in.

Dr. Sellers. Earth light is blindingly bright. When you go out and look through your visor on a space walk, the oceans look like blue neon. It has the look of glowing. When you go skiing at high altitude and you take your goggles off and look around, that is bright — well, it is even brighter than that.

A Fellow. What about the effect on the *Space Station*? You have the Sun coming in and you have the Earth light.

Dr. Sellers. You have a lot more diffuse light: on the day side you have the illumination from the Sun and you have a lot of reflection from the Earth and other bits of the *Space Station*, so even in a dark corner it is not black. There is always something in there you can see, although on the night side it can get rather gloomy.

Professor Hughes. It sounds like a wonderful story of 100% success, but did anything go wrong?

Dr. Sellers. Oh, something always goes wrong. We had problems with our reaction control system, problems with navigation, problems with radar. We had problems with the arm which locked up completely, and all the computers gave up. Actually all the computers on the station gave up for a while and we had to reboot the whole station, and that was while the arm was up with Garrett standing on the top for 25 minutes. He enjoyed the opportunity just to look around and take photographs. But things always go wrong; you build enough redundancies and work-arounds so that you can get around almost every problem. But in systems this complicated and at our level of technology now, you can guarantee that something is going to turn up every day and you are going to have to fix it. It's a bit like having an old British car [laughter].

Dr. T. Horbury. Firstly, thanks for inspiring so many people, including me, to go into space science. The question is, do you think that we will see NASA astronauts on a Dragon SpaceX rocket in the future?

Dr. Sellers. Absolutely! SpaceX have successfully launched and test-flown a Dragon, and their contract is with NASA. We gave them the money to develop that vehicle, and their contract is to take goods to the *Space Station*, and it would be great if we could use it to take astronauts backwards and forwards because, as you know, soon we will be relying solely on the Russians to get us up and back. So I see some promise there with the commercial sector for at least getting us to low Earth orbit, and hopefully that will free up NASA and other space agencies to go and do something beyond low Earth orbit, so when you are out of school you can go and do that.

A Fellow. Do I understand that you had to take out US nationality? Because it is the *International Space Station*, I am curious about that. And maybe you have some comment on the ITAR regulation? Is that why you had to take out US nationality?

Dr. Sellers. No, I couldn't even apply if I wasn't a US national, I couldn't even fill out the form. So I became a US national in 1991 and then applied three times and was accepted in 1996, by which time I was 41, so there is hope for everybody. That's the way it is; NASA astronauts are US citizens but they have a fair number of naturalized ones and that is very fair-minded of them. For those of you who don't know, ITAR is the legislation that the US put in there to stop rocket technology being exported to Russia and other places — we thought it was ending up in North Korea and elsewhere, but as it turns out all our business is with Russia now so it is now a problem for both sides. So we have to work around that all the time. I'm hoping that one goes away.

Mr. S. Coulter. Could you say what use the Americans are making of training facilities like Star City in Moscow?

Dr. Sellers. They train differently from us, and they are organized differently from us. Their cosmonauts come from the air force and a few engineers from Energiya, which is their civilian rocket corporation. But basically the air force is in charge of the training out of Star City and they run a terrific operation. They have been training people to do Soyuz flights and station flights for 30+ years and they are very good at it — I have done some of it myself and these guys know what they are doing. I remember the first time I went there they showed us all these charts, but now 'powerpoint' is a Russian noun and theirs are flashier than ours. So there has been a lot of progress in technology in training, but their training methods are very good and we really have no complaints.

Mr A. Sekhar. If something goes wrong; say if you have to make a risky decision, where does the decision come from?

Dr. Sellers. It depends what it is, and it depends on the urgency. There are a lot of decisions that you have to make as a crew or as a crew member by yourself if it requires an immediate response. But we are trained to do all that stuff where we have suit failures — I had one, for instance. Where you have a fire, de-pressurization, or toxic release in the *Space Station* you better get on it straight away and not wait for a phone call. Other ones, like the computer crash on station, we could have gone straight in there and rebooted it ourselves, but it was prudent because we had time to call home and they actually faxed up a procedure that guaranteed a better chance of success. So, you are a team, use your resources as much as you can, and that is what we do.

The President. You said you had a suit failure; what did you mean by that?

Dr. Sellers. They told me that I was running out of gas, but it was wrong. The point is, you don't know it's wrong, it's happening so you have to do what is necessary to cover for it.

Mr. P. D. Hingley. Would you sign up to go to Mars?

Dr. Sellers. Absolutely, who wouldn't? I think Mars is the Holy Grail of manned exploration for this and the next generation. It had an ocean and a warm climate a long time ago, and who knows what is in the rocks there. I can't wait for someone to go there and have a look.

Mr. N. Calder. What do you think will be the biggest headlines of the 15 years of the mission?

Dr. Sellers. For the astronomers we've got the *Alpha Magnetic Spectrometer* going up next year, and hopefully that will produce something. I think we'll get some scientific discoveries in biological sciences and some material sciences, but I think we will get an awful lot out of the technical element too, as there are technologies you can only do in space and they are useful in space. So they will help us in the next step outward. But we have a beautiful launcher up there now: six people and about 50% of man-hours are doing payloads and science rather just than keeping the station going. It takes about half the effort to keep the place running and now they have three people equivalent for work.

Mr. Stone. Could you say anything about the next addition to the *Space Station*, or will the next crew member be *Robonaut*?

Dr. Sellers. I met *Robonaut* the other day: I took a kid to go and visit him — it was part of the 'Make-A-Wish Program' and he wanted to shake hands with *Robonaut*. He is an interesting piece of technology in that he has the most adaptive hands of any robot — if he squeezes your hand he won't crush it, he will squeeze it just right because he can feel how well he is doing. If he is next to you and you ask him to raise his hand and he bumps you he won't knock you off your perch, he'll stop, look and then try to adapt. Even though it's an early piece of technology by space-robotics terms, I'm sure we will get something out of it. It would be nice to have more robots working outside the station.

Mr. C. Tribius. I see the altitude of the *Space Station* as always being constant or does it change position?

Dr. Sellers. No, we move attitudes [*sic*] quite a lot. It is meant to be flying along nose first into the wind because the shielding is preferentially arranged on the front side of all the modules. We turn it 180° when a shuttle docks and that is to protect the shuttle's heat shields, so the shuttle has its black heat shield flying away from the wind, thus protected from micro-meteorites. We've had several losses of attitude control where the station just gave up trying to keep position and drifted off, once when both crew members were outside. So they were outside spacewalking and it started creeping around about 20° out.

Mr. Tribius. What is the current altitude?

Dr. Sellers. The current attitude [*sic*] is called LVLH, local vertical local horizontal, so it just flies belly down to the world the whole time, with the solar panels paddling away to stay up against the Sun.

Dr. D. McNally. Do you still have problems with space debris?

Dr. Sellers. We have enormous problems with space debris; it is a constant worry. One benefit from the Cold War is the Colorado tracking, NORAD. They keep us informed of anything bigger than about the size of my fist, of which there are a lot. That would really ruin your day if it hit the *Space Station* at 1 km s^{-1}. So anything that we can see of that size or bigger, we call up the crew and get them to do an avoidance manoeuvre and take us out of the sphere where we think we might hit it. If we can't manoeuvre out of the way in time, the crew gets into their Soyuz and sits it out until the object has gone by, the thinking being that if it hits the *Space Station* you can still undock and get home. Now that leaves everything smaller than this that NORAD can't see and can still hurt you, so you rely on statistics to keep you out of trouble and the shielding on the front of the *Station*. But every time I've been space walking, particularly on STS-121 the last time I went space walking, you could see bullet holes all over the *Space Station*. It is not micro-meteorites for the most part, it's bits of other people's space junk, paint chips, bits off the third stage, and so on — paint chips particularly; we get a lot of those, they just peel off.

Dr. Stanley. Whilst on the subject of space debris, how many M&M's are unaccounted for? [Laughter.]

Dr. Sellers. They kept showing up. We thought that was a great idea, it was only an hour off that we filmed all the stuff and we had choreographed it so we could get maximum benefit. But with the M&M's we thought, how badly can this go wrong? So they kept showing up for the next four days. But they all end up in the air filters on the grills. If you lose something that's usually where it ends up.

Mr. D. Lally. What is the date of the final shuttle mission, and do you know what NASA is going to do with the shuttle craft — will there be a museums tour around the world?

Dr. Sellers. STS-133 is being postponed to February due to tank problems, which you might have heard about. Then I think 134 will be launched in April then maybe there will be a launch of 135; they had to keep a rescue shuttle on stand-by for 134 and that's all gassed up and all the pieces are there so they are talking about trying to launch that and bring all the crew back in Soyuz if they got in trouble. So that may be launched in June, July, or August. In any case, the three shuttles will go to museums; you will be able to see one of them in the Air and Space Museum in Washington DC and it's going to be a horrible fight over the other two.

A Fellow. I'm guessing that when you roll over on your back that's a stress manoeuvre, and am I also correct in thinking that you get undamped torque effects when you move the solar panels?

Dr. Sellers. First question, you are right, stress relief. You go that way during ascent because it's kinder to the attachment system that holds the shuttle; it's better to be pushed into the tank than pulled off it. But it turns out that with the acceleration when you are on a curve like that, you get the impression that you are being pushed that way rather than hanging upside down by your straps. When the solar arrays turn around — we have reaction wheels, big ones the size of railway rolling stock, flying around at 60 000 rpm when they get loaded up — because you are always going the same way, sometimes we paddled the other wheel, the solar arrays come back the other way to help out; but when the reaction wheels get loaded up we stop everything on the station and command the Russian reaction-rocket system to fire and unload all the wheels. So you do a burn and unload the wheels at the same time and the station does not move, that is something to watch; the momentum-management system is pretty good.

The President. I think we had better wrap things up. It has been a very special occasion to have had a talk from you and have you return the medal; that is absolutely wonderful. We all want to thank you, but I want to add one other thing, which is that I would like to remind you that the whole astronaut corps is essential to many of the things that we all work on, both on the *Space Station* and on *Hubble Space Telescope*, and these people are enormously brave on our behalf. So I think we should recognize that and thank Piers now. Thank you. [Applause.] It just remains for me to wish you a happy Christmas and remind you that we will meet again on 2011 January 14.

VIVA PANSPERMIA!

By Chandra Wickramasinghe
Cardiff Centre for Astrobiology

The arguments for panspermia as a mode of origin of life on Earth are far from dead; on the contrary they are now more robust than ever.

The term *panspermia* is derived from Greek roots: *pan* (all) and *sperma* (seed) — seeds everywhere. The underlying ideas go back to the time of classical Greece to philosopher Aristarchus of Samos in the 5th Century BC promulgating the ominipresence of the seeds of life in the cosmos. Panspermia also has a resonance with more ancient Vedic, Hindu, and Buddhist traditions of India stretching back over 4000 years.

The first serious scientific statement of panspermia came from Lord Kelvin (William Thomson[1]) at the 1871 presidential address to the British Association: "Hence, and because we all confidently believe that there are at present, and have been from time immemorial, many worlds of life besides our own, we must regard it as probable in the highest degree that there are countless seed-bearing meteoritic stones moving about through space. If at the present instant no life existed upon the Earth, one such stone falling upon it might, by what we blindly call natural causes, lead to its becoming covered with vegetation."

Three decades later panspermia was placed in an explicit astronomical context by Svante Arrhenius[2], first in a short paper published in 1903 and thereafter in his book *Worlds in the Making*. Long before the discovery of the many remarkable survival attributes of bacteria, Arrhenius inferred that such properties must exist, and cited experiments where seeds had been taken down to near zero degrees Kelvin and shown to survive. Arrhenius also calculated the effect of radiation pressure of starlight on spore-sized particles in space and argued that spores lofted in rare events from an inhabited planet like Earth could be projected at speed to reach a distant planetary system. This came at a time when neo-Darwinian ideas of evolution were at last beginning to gain general support, and it might have been feared that Arrhenius' views would threaten or reverse a hard-won victory over creationist beliefs. The threat to Darwinism was unfounded, however. Darwin did not make any reference whatever to the origin of life in his classic book *Origins of Species*, although in a letter to Joseph Hooker in 1871 he wrote thus: "But if (and oh! what a big if!) we could conceive in some warm little pond, with all sorts of ammonia and phosphoric salts, light, heat, electricity, &c., present, that a proteine compound was chemically formed and ready to undergo still more complex changes …" That conjecture did not necessarily relate to a terrestrial origin of life, although it was later interpreted as such, and formed the inspiration for the familiar primordial-soup model and to theories of chemical evolution that became more or less adjunct to the Darwinian theory.

Opposition to Arrhenius's challenge of Earth-bound theories of life's origin took a ferocious turn with publications by Becquerel[3] and others claiming to *disprove* panspermia from an experimental standpoint. On the basis of experiments that showed certain bacteria to be killed by exposure to ultraviolet light, the argument gained ground that *all* bacteria expelled from a planet would be killed by conditions in space. It is now clear that space-travelling bacteria could be easily shielded from ultraviolet light with extremely thin layers of overlying carbon, and bacteria within interiors of small clumps would be particularly well protected[4–6]. This was not known at the time, and in any case, as Julius Caesar said: "*fere libente homines id quod volunt credunt*" (men readily believe what they want to believe). Thus a firm conviction that panspermia is a defunct theory gained ground and dominated scientific culture for nearly half a century from 1924–1974.

The revival of panspermia as a viable theory started in the mid-1970s with the work of Hoyle and the present author[7–9] seeking to explain the steadily increasing complexity of the organic molecules that were being discovered in interstellar space through radio astronomy and infrared astronomy. At first, rather subtle data-reduction methods were required to infer the presence of complex organics from the earliest infrared spectra of dust[9]. Both Hoyle and the author devoted nearly five years of their professional lives to this project, and by 1983 we inferred confidently that some 30 per cent of the carbon in interstellar dust clouds had to be tied up in the form of organic dust that matched the properties of degraded or desiccated bacteria[4]. That far-reaching conclusion has only come to be further strengthened with advances in stellar spectroscopy in the past three decades. Spectroscopic signatures of PAHs and organic polymers in interstellar space as well as in external galaxies have come to be well established[10–14].

What cannot be denied is that the Hoyle–Wickramasinghe corpus of published work comprised of several books and hundreds of scientific papers seeking to re-establish panspermia as a viable theory on a proper scientific footing came well ahead of the references that are often cited[15]. A swing of opinion in favour of panspermia was facilitated by the discoveries of putative microfossils in meteorites, including the Mars meteorite ALH84001[16]. There was thus direct evidence for the survival of complex organic assemblages after millions of years of transit in interplanetary space. Moreover, dynamical pathways have been identified by which material ejected from one planetary body in the Solar System can reach another distant body[19–22,31,32].

Recent studies of extremophiles and the well-established resistance of bacteria to ionizing radiation[17] and space conditions including hypervelocity impacts[18] lend further credibility to panspermia theories. In a recent review of relevant experimental data on this subject we concluded that despite all the hazards of space a minute fraction of bacteria *must* remain in a viable condition in interstellar clouds between expulsion from one planetary/cometary source and re-accommodation in another[6,19]. For panspermia to work this viable fraction could be as small as 1 in 10^{24}, a condition that would be well-nigh impossible to violate. The picture here is strikingly similar to the sowing of seeds in the wind. Few are destined to survive, but so many are the seeds that some amongst them would inevitably manage to take root.

Hoyle and the present author in their writings have elaborated on the rôle of partially destroyed bacteria noting that viral genomes derived from cells have a much longer persistence under interstellar conditions compared to the much larger bacterial (or eukaryotic) genome. In our monograph[4] *Proofs that Life is Cosmic* published in 1982 we wrote thus (p. 14): "Viruses, and viroids still more, have the advantage of being smaller targets for damaging radiation than bacteria. Thus about 100 kr (of ionising radiation) is needed to produce a single break in the nucleic acid of the smaller viruses, and in excess of 1 Mr for viroids. In addition to this advantage, viruses can use the enzymic apparatus of host cells to repair themselves, even to the astonishing extent of being able to 'cannibalise', a process in which several inactivated viral particles combine portions of themselves to produce a single active particle."

The currently popular view that all the organics now known to exist in interstellar clouds represent steps towards life is not justified. We have recently reviewed the astronomical evidence that supports the idea that these molecules are most probably derived from life[14,20] — the interstellar medium is a veritable graveyard of cosmic bacteria. The detritus of bacterial life in interstellar clouds would range from charred bacteria (resembling anthracite grains), genetic fragments of cells representing viruses and viroids, to PAHs and smaller organic molecules.

The present author's views do not contradict Wesson's restatement[15] of this process as *necropanspermia* — cute term indeed! However, it is impossible to maintain, as he does, that all bacteria expelled from a source will be killed in interstellar transits. Indeed, explicit mechanisms for viable interstellar transfers of microorganisms have been identified by several authors[21,22]. The first introduction of life onto our planet (or indeed any planet) must involve the introduction of a viable microorganism — not a fragment of genome, a virus, or a dead microorganism. Subsequent evolution of life would be greatly speeded up with the more prolific injection of viruses that could insert genes into already evolving cells. We have argued that this process could not only lead to epidemics of disease but also contribute to evolution[13,23,24].

An initial injection of a viable cellular life form, which takes root and begins to evolve, would be augmented genetically by viruses carrying genes for the development of all other possible life forms[4,23,24]. This grand *ensemble* of genes for cosmic evolution would in our model have been delivered in comet dust to our planet throughout geological time[4]. The earliest evidence of bacterial life on the Earth is between 3·8 and 4 Gya during the Hadean epoch[25] which was characterized by an exceptionally high rate of comet impacts. Comets that delivered water to form most of the oceans probably delivered the first viable bacterial cells that subsequently evolved. From an initial small bacterium (typified by *Mycoplasma genitalium*) that had ~ 500 genes, life evolves over a 4-billion-year timescale to produce mammals with genomes consisting of some 25 000 genes. Modelling the correlation between average gene number (N) and time elapsed, t, leads to an empirical relationship

$$N \sim 500 \exp(t/\tau), \qquad (1)$$

where the value of τ is close to 1 Gy (see ref. 26; also Joseph, Wickramasinghe & Wainwright, in press). Equation (1) may be taken as defining the development of gene complexity within a physically connected set of planets, $dN/dt \propto N$ implying a capture rate of genes proportional to cross-section in an open Darwinian system of evolution. Working backwards from $N = 500$ at 4 Gya to lead to a simple viral-sized genome of say $N = 10$ (the virus of *E coli* φX174 has 11 genes), nearly four *e*-folding times are involved, giving a total evolutionary timescale of nearly 8 billion years, longer than the age of the Earth (*cf*. refs. 28 & 29).

If we had secure knowledge that life did in fact originate on Earth, the theoretical foundation of Earth-bound abiogenesis models would also be sound. However, in the absence of such knowledge the fullest exploration of all possibilities is surely justified. Hoyle and the present author first estimated the odds against life arising in a primordial soup on the Earth on the basis that an entire bacterial genome arises as a one-shot random-assembly event. That this estimate is vastly exaggerated has been pointed out by biologists who argued that an initial set of enzymes needed to kick-start biological evolution might be smaller than the final set[30,33]. Even after taking account of this valid objection, the odds against the emergence of an ancestral cell or protocell remains minuscule. With, say, 10 genes in an evolvable protocell genome, the probability of its emergence assuming ~ 10 sites per gene required to be correctly filled by one of a set of 20 amino acids is $\sim 10^{-130}$. This, in the author's view, is far too small for an occurrence in any terrestrial setting. Nor could Complexity Theory resolve the issue — the requirement is not for the emergence of complexity *per se*, but for the emergence of a highly specific complexity configuration which entails similar improbability hurdles.

The arguments of evolutionary biologists for abiogenesis could be far more profitably set in an astronomical or even cosmological context[27], with panspermia taking care of the rest.

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"TO SEE THE WORLD IN A GRAIN OF SAND" A VINTAGE IDEA IN PHILOSOPHY IS REALIZED IN A MODERN THEORY OF PHYSICS

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The quotation in the title from the 18th-Century philosopher Blake finds a mathematical form in a new theory, where the perceived world is embedded in a bigger, higher-dimensional universe.

If you could blow up your surroundings to twice the size — and yourself — would there be any real difference? This is not a silly question. It concerned the English philosopher William Blake (1757–1827), whose musings on the size of things are neatly summed up in the appealing phrase quoted in the title above[1]. Believing that there was no essential difference between a thing and a scaled-up version of it, several leading physicists in the 1970s constructed new cosmologies based on that principle. These included accounts by Dirac, Hoyle and Narlikar, and others, with scale-invariant versions of Einstein's theory of General Relativity. The latter was extended to include a new scalar field in addition to gravity, by Dicke, who opined that only the ratios of like quantities made any operational sense in physics. These theories were all very technical, and used the notion of a curved four-dimensional map which welds together the common notions of space and time in the manner made famous by Minkowski and Einstein. Besides being based on 4-D space-time, all of the noted theories showed another property: they were not adopted by the physics community, and failed to displace Einstein's theory as the working model for cosmology.

Recently, things have changed. Following work by several groups in the 1990s, a way was found to give a mathematical account of the Universe, which includes Blake's philosophical vision of worlds within worlds. We do not need to delve into details, but in the simplest version, the new theory may loosely be called *five*-dimensional relativity. The addition of an extra dimension to the familiar ones of space and time enables us to view 4-D space–time 'from outside'. The astute reader may well ask immediately about the nature of the fifth dimension, to which an answer is that it is connected to the masses of particles and the presence of matter. Technical journals contain papers dealing therefore with space–time–matter theory[2], or its alternate version of membrane theory[3] (where space–time is envisaged as a 4-D surface in 5-D). The 5-D theory contains the 4-D one of Einstein, a fact guaranteed by a result from differential geometry known as Campbell's embedding theorem. That theorem languished in the annals of physics for about 70 years, until its rediscovery showed how to extend gravitational theory from 4-D to 5-D to … *N*-D, while ensuring agreement with the classical tests of relativity. Here we do not intend to replay the dirge of differential geometry, but rather focus on the beauty of Blake's vision of the world within a grain of sand.

Variations on this idea have appeared in different genres, from theology to science fiction. For example, in Poul Anderson's novel, *Tau Zero* (Doubleday, 1970), a spaceship travels at ultra-relativistic speeds so that it approaches a state of zero time, when a brilliant point-like object appears in the cabin, which on closer examination proves to be a mini-universe. This is the kind of evocative image which would have appealed to Blake.

If artists carry icons in their minds, then so do scientists. For the physicist, one of the most important things in his mind-gallery is the *Planck* mass. This is the fundamental quantity formed from Planck's constant of action, Newton's gravitational constant, and the speed of light. It is about 10^{-5} gram, and is believed by many physicists to mark the 'cross-over' between quantum mechanics and classical theory as embodied in Einstein's general theory of relativity. Either by the uncaring chance of numbers or by the heuristic hand of fate, the Planck mass is about the same as the mass of Blake's grain of sand.

If a picture is worth a thousand words to the artist, an equation is worth a million words to the physicist. For the reader who suffers indigestion from geometry, forbearance is requested for the following short discussion. Pythagoras may be blamed for the inculcation in the young that the size s of something in the ordinary 3-D space, x, y, z of our perceptions, is given by $s^2 = x^2 + y^2 + z^2$. Actually, in a vast Universe, we can make measurements only in our local neighbourhood, so this formula should be adjusted to include only very small differences in the quantities concerned: $ds^2 = dx^2 + dy^2 + dz^2$. However, as any geophysicist or astrophysicist will attest, it is easier in practice to measure things not in terms of rectangular coordinates, but in spherical ones. The reason is that things in nature — like the Earth — *are* approximately spherical in shape. They are better described in terms of the radius r and two angles, θ and ϕ, akin to latitude and longitude. Then $ds^2 = dr^2 + r^2 (d\theta^2 + sin^2\theta\, d\phi^2)$. However, *anything* can be measured in these coordinates. Particular attention should be paid here to the presence of r^2 in the second part of the expression, because it will reappear in 5-D guise below. But before that, we need to note that relativity obliges us to widen the scope of the 3-D measure to include the time t, resulting in the 4-D measure of (local) space–time: $ds^2 = dr^2 + r^2 (d\theta^2 + sin^2\theta\, d\phi^2) - c^2dt^2$. The speed of light c appears here because cdt is the distance covered during the lapse of a small time dt; and the signs have been adjusted in the expression to reflect our differing perceptions of time and space, and to ensure that $ds^2 = 0$ corresponds to a light ray, by which nearly all information in astronomy is collected. The expression for ds^2 as written gives the square of the size of a portion of space–time, near to us in space and at about the same time as our experience. This is the 'interval' of Special Relativity. The noted form needs further adjustment for large distances and lag-times, resulting in the corresponding interval for the *curved* space–time of General Relativity. However, we eschew the details of this, since tomes have been written about the bending effects of large masses and gravitation on space–time. This enables us to proceed to the fifth dimension, which is not hard to do because the route is analogous to how 3-D was extended to 4-D above.

For the extra dimension, we need a new coordinate. Let us call it l, a length. Then just as we use units such as centimetres and seconds for measurements in space and time, we agree to measure l in terms of a unit, say L. This implies that we can make measurements of the fifth dimension which are objective, insofar as they involve the ratio l/L, which is a pure number. To finish the extension from 4-D to 5-D, it is useful to note that work in differential geometry shows that we can add on the fifth dimension to space–time as a simple ('flat') piece. The result is a 5-D distance measure or interval which contains the 4-D one, and has the form $dS^2 = dl^2 - (l/L)^2ds^2$. This tells us how the (square of) distances in five dimensions are composed of distances in four dimensions (ds^2) and the extra dimension (dl^2). In this formula, the factor l^2 which multiplies 4-D space-time is the analogue of the r^2 in the 3-D case considered above, and is likewise variable. In this regard, the quadratic l-factor in the 5-D case is also the analogue of the quadratic t-factor in the 4-D Milne model of the Universe, and in both cases the relative motion between frames is smooth and regular. (In fact the canonical metric in 5-D is a scaled-up version of the synchronous metric in 4-D cosmology, with regular motion measured by the change in l.) It is clear from the 5-D perspective that our cosy 4-D world may either grow or shrink, depending on whether the factor l/L becomes greater or less than unity. During such a blow-up or collapse, viewed internally in 4-D, things stay as normal, governed by the standard laws. Indeed, it may be proven by using Campbell's theorem that the form just given includes *all* the standard results of Einstein's General Relativity which make up the stuff of modern astrophysics. These results apply to stars like the Sun and black holes like the one inferred to lie at the centre of the Milky Way.

Black holes and other exotic objects form an inventory for modern astrophysics that is far wider than that of traditional astronomy. (A good non-technical review of current cosmology and its underpinnings is Halpern's book[4].) The philosophers of Blake's day were able to describe the world in terms of relatively simple, non-mathematical principles. But the world of the modern cosmologist is a perplexing subject described in terms of bent 'spaces' and arcane algebra[5–7]. Luckily, only one extra dimension appears to be necessary to account for the strange discoveries of recent years, and it is useful to make some comments on this before returning to Blake's vision.

In space–time–matter theory, the extra coordinate l enables our cosy world with 'distances' ds to be embedded in a larger one with measure dS (see above). The coordinate l is at right angles to space–time, and can extend arbitrarily far. So in terms of the unit L, the factor $(l/L)^2$, which rescales our world, can be arbitrarily large. Conversely, our world as viewed from a 5-D perspective can be arbitrarily small — as small, for example, as a grain of sand. Incidentally, the size of the unit L turns out by a comparison with General Relativity to be related to the cosmological constant Λ. This mysterious quantity measures the properties of what used to be called empty space, but is now known to be the seat of energy fields and called the vacuum. The precise relation is $\Lambda = 3/L^2$. Observations of the motions of galaxies show that they are being pushed apart by some kind of dark energy, the simplest form of which is just the field associated with Λ. Sprinkled throughout the vacuum are clumps of ordinary matter, such as the galaxies. This common-or-garden stuff may be shown to be connected to the places where the fifth dimension 'pokes' into our 4-D space–time, hence the name 'induced-matter theory', which is another term for space–time–matter theory.

The alternative version of 5-D relativity is known as membrane theory because its extra coordinate is measured from a kind of surface or membrane where the geometry is sharply deformed. That singular surface is identified with 4-D space–time, to which all of the interactions of particles are confined. A consequence is that particles interact with forces much greater than gravity. A major motivation for the theory is that it can explain the real masses of the elementary particles, which differ greatly from the theoretical Planck mass noted above. Both versions of 5-D relativity share a common mathematical structure, as shown by Ponce de Leon[8] and others. But their physical interpretations differ greatly: space–time–matter theory envisages an unlimited extra dimension, while membrane theory sees it as squashed.

The philosopher Blake could hardly have foreseen these flights of physical fancy. However, mental flexibility was the man's trademark, and he would probably not have been flabbergasted to learn that the perceived world may be embedded in a larger structure. Also, the possibility of describing Nature in mathematical terms was already established by Blake's time, since Newton's *Principia* had been published in 1687, fully 70 years before Blake's birth.

Blake's vision of a world in a grain of sand is included in space–time–matter theory, for we can blow up the size of the 5-D super-world as much as we wish, without disturbing the relations of the embedded 4-D mini-world. It is also possible to go in the opposite direction, if desired. From the 5-D perspective, it is as if our conventional world is being viewed through the photographer's zoom-lens.

One can zoom in, or one can zoom out. Tantalizingly, the mathematics of the model do not tell which is the case. The situation is akin to models of the Universe based on General Relativity, which can in principle either expand or contract. Despite the power of the 5-D model, the direction of its evolution has to be put in 'by hand'.

Ramifications of models of this sort have recently been reviewed in the books by Carr[6] and Wesson[7]. Increasing the number of dimensions N for the world opens a warren of possibilities, which multiply as N grows. In the present account, attention has been focussed on $N = 5$, which leads to the two versions of relativity discussed above. Both have foundations which are in the nature of philosophical goals: to explain the origin of matter and to explain the masses of particles. But both are typified by extensive mathematical superstructures, which are essential to deriving results which can be compared to observations. While the philosophy is simple and understood by nearly everybody, the mathematics are complicated and fully comprehended by relatively few. A little reflection shows that in modern science this is a common state of affairs. It raises anew an old question: what is the difference between philosophy and physics? People in both camps are supposed to have a common goal, namely a better understanding of the world and our place in it. Yet, the proponents from both camps appear often to be at loggerheads. One essential difference between physics and philosophy is, of course, that the former systematically uses mathematics while the latter often does not. But if we follow men like Bertrand Russell (who had a foot in both camps), then we should view mathematics as a *language*; and as such, it should help rather than hinder progress. Unfortunately, at present, many philosophers are engrossed in word-games, and many physicists are only interested in adding a decimal point to already-established results.

This mutually-sterile stance would have been of concern to the people on both sides, who by virtue of history, are now held in universal respect: philosophers like Plato and Wittgenstein, and physicists like Einstein and Eddington. To be blunt: philosophers need to learn about the power of mathematics, and physicists need to lighten up with a dose of ideas.

Blake was an expert at thinking "outside the box" (to use modern parlance). Since his time, science has gained a kind of ascendancy in the public mind, due largely to its successful application to technological problems, including those in medicine. However, no matter how successful modern physics may appear, its career cannot continue indefinitely without the injection of new ideas. To get fresh results, it is to be hoped that physicists will heed Blake's call to be wary of the "mind-forged manacles" of reason.

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SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 218: HD 115461, HD 116247, HD 116345, AND HD 120006

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The four stars are all near the eastern margin of the field of the North Galactic Pole (NGP) as defined by the circle of Galactic latitude 75°. HD 115461 and HD 116247 are double-lined pairs of main-sequence stars, with periods of about 10·3 and 7·2 years, respectively. The other two objects are single-lined giants with types of about K0 and K4 and periods of about 1·6 and 10·6 years.

Introduction

This is another paper devoted to stars discovered to be spectroscopic binaries near the North Galactic Pole (NGP) in the survey[1] by Yoss & Griffin of all the late-type stars in the *Henry Draper Catalogue* that are at $b > 75°$. As in so many other cases, most of what is already known about them is what was given in the survey[1], which unfortunately is not retrieved by any of the *Simbad* bibliographies, so it is made to appear that very little, or in some cases *no*, astrophysical information (apart from the *HD* types) is available for the stars. The spectral types, absolute magnitudes, and *z*-distances estimated from *DDO*-style photometry in the survey are given in Table I for the four stars discussed here; the *Hipparcos* absolute magnitudes are listed for comparison in the two instances in which they exist.

TABLE I

NGP Survey[1] results for the four stars

Star	*V*	*(B–V)*	*Type*	M_V	*z*	$M_V(Hp)$
	m	*m*		*m*	*pc*	*m*
HD 115461	9·19	0·56	G0 V	+4·4	89	+3·1 ± 0·5
HD 116247	9·58	0·52	F8 V	+4·0	129	
HD 116345	9·27	0·92	K0 III–IV	+2·3	241	
HD 120006	8·31	1·41	K4 III	+1·2	258	+0·2 ± 1·0

It does not take much space to describe such papers as *Simbad* records for the stars (catalogue-type ones that simply report the preliminary radial-velocity data obtained in the writer's survey[1] are omitted here). There is nothing on HD 115461 or 116247. For HD 116345 there is a set of *UBV* measurements by Häggkvist & Oja[2] ($V = 9^{m}{\cdot}22$, $(B-V) = 0^{m}{\cdot}92$, $(U-B) = 0^{m}{\cdot}72$); the star was also included in an investigation by Soubiran *et al.*[3] in which one radial velocity was measured for it, but as no date was given it is not of utility for the orbit. For the relatively bright object HD 120006 there is *UBV* photometry by Oja[4] ($V = 8^{m}{\cdot}33$, $(B-V) = 1^{m}{\cdot}42$, $(U-B) = 1^{m}{\cdot}72$), but nothing else apart from a paper by Famaey *et al.*[5] which gives a mean radial velocity that is doubtless derived from such of my observations as were to be found in the data base in Geneva of observations made with the OHP *Coravel*. (That paper also gives an M_V value, supposedly derived from the *Hipparcos* parallax, of $+0^{m}{\cdot}71$.

It obviously implies a distance modulus of $7^{m}\cdot60$, nearly half a magnitude less than is given by the present writer's slide-rule as corresponding to the parallax of 2·45 arc-milliseconds; allowance for any interstellar absorption would worsen the discrepancy.)

All four stars are to be found towards the following (eastern) margin of the NGP field, as may be understood from their *HD* numbers (in the whole field, the numbers range from about 102 000 to 122 000). HD 115461 is at the relatively high declination of +38°, about 4° following α CVn (Cor Caroli); HD 116247 is about 4° north-following β Comae; HD 116345 is about 3° following α Comae and is only about 15′ away from HD 116479, which was treated in Paper 206 in this series; and HD 120006 is about 1° south-preceding the fifth-magnitude HR 5195 and is within a degree of HD 120531 (Paper 190).

Radial velocities and orbits

As in most of the recent papers in this series, and specifically in those referring to NGP stars (Papers 203, 206, 212, 215), the principal sources of radial velocities are the Haute-Provence (OHP) *Coravel*[6] and the Cambridge instrument that is optically similar to it; in the present case a very few additional measurements come from the original Cambridge spectrometer[7], from the one[8] at the Dominion Astrophysical Observatory (DAO) 48-inch reflector, and from the ESO *Coravel*. For all the stars treated here the first observation of all was made with the old Cambridge instrument by Dr. G. A. Radford when he was a graduate student. The numbers of observations made with each instrument for each star is shown in Table II. Inasmuch as HD 115461 and 116247 are double-lined, and the available preliminary reductions of their traces in the Geneva data base are in many cases single-lined or actually wrong ('inverted' — the secondary dip has been supposed to be on the wrong side of the blended pair), the majority of the OHP velocities of those two stars are unusable. The single-lined ones are included here in Tables II, III, and IV and in Figures 2 and 4, but are necessarily given no weight in the orbits; the inverted ones, which include one ESO observation of HD 115461, are omitted altogether. It has regrettably proved to be no longer possible to obtain properly performed reductions from the Geneva data base, to which the writer himself has no access.

TABLE II

Tally of usable radial-velocity measurements of the four stars

Numbers in brackets refer to additional observations, reduced as single-lined from blended traces of double-lined stars and thus not serviceable for orbit determination

Source	*HD 115461*	*HD 116247*	*HD 116345*	*HD 120006*	*Totals*
Cambridge (old)	0 (1)	0 (6)	2	2	4 (7)
DAO	1 (2)	0 (1)	1	3	5 (2)
OHP	8 (13)	2 (19)	17	14	41 (32)
ESO	0 (0)	0 (1)	—	—	0 (2)
Cambridge (new)	34 (1)	30 (1)	29	53	146 (2)
Totals	43 (17)	32 (28)	49	72	196 (45)

HD 115461

This is a troublesome star observationally, having very unequal double lines that are blended together for 90% of the orbit. The period is just over ten years, during which there is only one year when the two 'dips' in the radial-velocity traces can be seen separately. Only at such phases can the dip strengths and

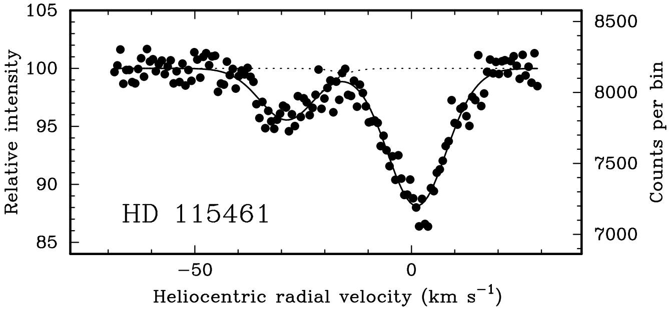

FIG. 1

Radial-velocity trace of HD 115461, obtained with the Cambridge *Coravel* on 2008 July 2, at the node of the orbit, when the two dips just become separated and their individual profiles can be seen. Knowledge of the dip profiles facilitates double-lined reductions of traces in which the two dips are blended together.

widths be assessed, so that they can be imposed on the reductions of blended traces, which in the absence of such information would be almost irreducible. A trace taken almost exactly at the one favourable node is reproduced here as Fig. 1. The mean ratio of dip areas is found to be 1 to 0·35, equivalent arithmetically to a magnitude difference of $1^{m}\cdot14$; for the widths of the two dips, $v \sin i$ values of 5 km s^{-1} for the primary and zero for the secondary have been adopted. The radial velocities are set out in Table III. The OHP velocities of the stars treated here have, as usual, been subject to an addition of 0·8 km s^{-1} in an effort to place them on the zero-point normally adopted in this series of papers; to bring the Cambridge *Coravel* data of HD 115461 into systematic near-agreement, they have been corrected by −0·8 km s^{-1} from the 'as reduced' values. There is one observation made with the original Cambridge instrument, but it was inevitably measured as single-lined. There are also three DAO observations; two of them are blends, but in the other case the primary star alone was measured, and since the phasing was such that there would have been no blending the measurement can be accepted for the orbit. With the OHP velocities and the DAO one being weighted ¼ and the secondary's velocities weighted ⅒ of the primary's, the *ensemble* readily leads to the orbit that is plotted in Fig. 2 and has elements as follows:

P	=	3751 ± 6 days	$(T)_4$	=	MJD 54694 ± 6
γ	=	−13·12 ± 0·05 km s^{-1}	$a_1 \sin i$	=	402 ± 4 Gm
K_1	=	9·19 ± 0·08 km s^{-1}	$a_2 \sin i$	=	502 ± 10 Gm
K_2	=	11·49 ± 0·22 km s^{-1}	$f(m_1)$	=	0·184 ± 0·006 $M_\odot$
q	=	1·250 ± 0·026 (= m_1/m_2)	$f(m_2)$	=	0·360 ± 0·021 $M_\odot$
e	=	0·530 ± 0·006	$m_1 \sin^3 i$	=	1·17 ± 0·06 $M_\odot$
ω	=	20·0 ± 1·0 degrees	$m_2 \sin^3 i$	=	0·93 ± 0·03 $M_\odot$

R.m.s. residual (unit weight) = 0·29 km s^{-1}

Viewed with a sufficiently jaundiced eye, Fig. 2 seems to show the secondary velocities to have a tendency to lie above the computed curve all through the long apastron side of the orbit. No doubt it is the observations near the periastron-side node, where their comparatively large distance from the γ-velocity gives

TABLE III

Radial-velocity observations of HD 115461

Except as noted, the sources of the observations are as follows:
1987–1998 — OHP Coravel (weight ¼); 2000–2010 — Cambridge Coravel (weight 1)

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
1974 Mar. 2·22[*R]	42108·22	−13·8		0·645	—	—
1987 Mar. 3·95	46857·95	−8·4		1·911	—	—
1988 Feb. 1·51[†]	47192·51	+0·9	—	2·000	+0·8	—
Mar. 13·09	233·09	−1·3	−27·8	·011	−0·3	+0·4
1990 Jan. 31·07	47922·07	−17·7	−8·5	2·195	−1·2	+0·4
1991 Jan. 30·10	48286·10	−17·7	−7·1	2·292	−0·1	+0·4
1992 Jan. 14·17	48635·17	−15·5		2·385	—	—
20·20	641·20	−15·0		·387	—	—
Feb. 26·56[†]	678·56	−16·2		·396	—	—
Apr. 25·09	737·09	−14·8		·412	—	—
Aug. 13·85	847·85	−15·3		·442	—	—
1993 Feb. 15·20	49033·20	−15·0		2·491	—	—
July 7·94	175·94	−14·5		·529	—	—
1994 Jan. 3·23	49355·23	−14·0		2·577	—	—
May 2·04	474·04	−14·9		·609	—	—
Dec. 13·21	699·21	−14·2		·669	—	—
1995 Jan. 8·22	49725·22	−13·5		2·676	—	—
June 5·99	873·99	−13·5		·715	—	—
1996 Mar. 31·07	50173·07	−12·4		2·795	—	—
1997 Mar. 31·10[‡]	50538·10	−7·4	−21·6	2·892	−0·5	−0·7
May 3·06[‡]	571·06	−6·3	−21·5	·901	−0·1	+0·3
July 20·93	649·93	−4·2	−23·5	·922	+0·1	+0·6
Sept. 8·79	699·79	−2·7	−24·9	·935	+0·3	+0·8
Dec. 22·22	804·22	−1·0	−30·1	·963	−0·6	−1·0
1998 Apr. 29·03	50932·03	0·0	−29·1	2·997	−0·3	+0·8
July 9·90	51003·90	−2·6	−26·7	3·016	−0·9	+0·6
1999 July 12·30[†]	51371·30	−13·4		3·114	—	—
2000 Apr. 7·11	51641·11	−16·7	−9·2	3·186	−0·4	−0·1
June 12·98	707·98	−16·6	−6·6	·204	+0·1	+2·1
July 16·94	741·94	−17·5	−7·4	·213	−0·7	+1·1
2001 Jan. 14·24	51923·24	−17·0	−7·3	3·261	+0·4	+0·5
Mar. 5·20	973·20	−17·4	−6·2	·275	+0·1	+1·4
July 10·94	52100·94	−17·6	−7·2	·309	+0·1	+0·2
2002 Jan. 1·26	52275·26	−17·8	−7·0	3·355	−0·1	+0·4
Mar. 1·16	334·16	−18·1	−8·3	·371	−0·4	−0·9
May 22·99	416·99	−17·7	−6·9	·393	0·0	+0·5
2003 Feb. 21·14	52691·14	−17·5	−7·3	3·466	−0·1	+0·5
May 12·00	771·00	−17·2	−5·5	·487	+0·1	+2·4
Aug. 16·86	867·86	−16·8	−6·9	·513	+0·3	+1·3

TABLE III (*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
2004 Apr. 3·14	53098·14	−16·8	−10·4	3·575	−0·3	−1·5
2005 Jan. 23·25	53393·25	−15·5	−9·8	3·653	0·0	+0·4
May 12·01	502·01	−15·3	−9·4	·682	−0·3	+1·4
2006 Mar. 2·16	53796·16	−13·1		3·761	—	—
2007 Feb. 4·17	54135·17	−9·9	−18·9	3·851	−0·3	−1·4
Apr. 2·02	192·02	−8·2	−18·3	·866	+0·5	+0·3
May 2·05	222·05	−8·5	−17·9	·874	−0·3	+1·4
June 15·98	266·98	−7·9	−21·0	·886	−0·5	−0·7
July 22·92	303·92	−6·6	−20·8	·896	0·0	+0·5
2008 Jan. 6·27	54471·27	−1·9	−26·7	3·941	+0·6	−0·3
Feb. 16·19	512·19	−1·4	−27·0	·952	0·0	+0·8
Mar. 31·07	556·07	−0·2	−29·0	·963	+0·1	+0·1
Apr. 24·05	580·05	−0·2	−29·2	·970	−0·3	+0·5
May 20·98	606·98	+0·6	−29·8	·977	+0·1	+0·3
July 2·95	649·95	+0·7	−29·2	·988	+0·1	+1·1
2009 Jan. 14·26	54845·26	−5·6	−22·5	4·040	−0·1	+0·1
Mar. 6·18	896·18	−7·5	−19·1	·054	+0·1	+0·9
Apr. 22·04	943·04	−9·4	−18·1	·066	−0·1	−0·2
May 26·98	977·98	−10·2	−16·7	·076	+0·1	−0·1
2010 May 24·03	55340·03	−15·7	−8·3	4·172	+0·3	+1·2
July 18·92	395·92	−16·0	−7·9	·187	+0·4	+1·2

*R Observed by G. A. Radford with original spectrometer.
†Observed with DAO 48-inch telescope; weight ¼.
‡Observed with Cambridge *Coravel*; weight ½.

them much greater leverage, that mainly define the secondary amplitude. The apparent slight systematic residuals could probably be corrected merely by assigning a small rotational velocity to the secondary dip in computing the twin velocities from the wholly-blended 'dips' which constitute the observational material that delineates the apastron side of the orbit. But that is not the proper way to assess a $v \sin i$, and no effort has been made to bring the observations into line by any such artifice, although the fitting of the (relatively few) traces that are resolved like Fig. 1 by no means precludes the possibility that the secondary $v \sin i$ could be up to a few km s^{-1}, and does actually demonstrate the appreciable (~5 km s^{-1}) rotational velocity of the primary star.

The V magnitude difference between a pair of F/G dwarf stars may be estimated from the ratio of areas of their dip signatures in radial-velocity traces; Griffin & Suchkov[9] proposed multiplying the magnitude-equivalent of that area (in this case 1^{m}·14) by the empirical factor 1·15, making $\Delta V \sim$ 1^{m}·3. Another estimate can be made from the mass ratio. Leaving aside the various tabulations and looking instead at the lower envelope of masses well attested from binary systems by Andersen[10], we find that log m changes very regularly by about 0·013 per spectral sub-type in the relevant region of colour index, so the observed q of 1·25 (making a Δ(log m) of nearly 0·1) would indicate a difference of seven or eight sub-types. From the combined type of G0 and the colour index of 0^{m}·56 (Allen[11] puts the G0 V colour at 0^{m}·58) shown in Table I we could deduce types

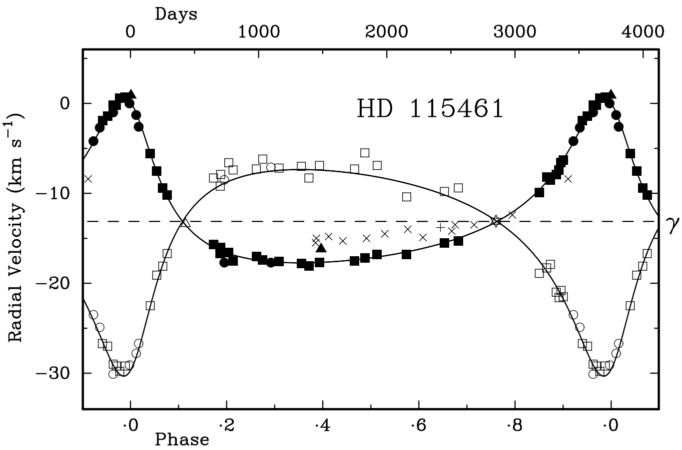

FIG. 2

The observed radial velocities of HD 115461 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. Measurements of the primary are plotted with filled symbols, those of the secondary with open ones. The great majority of the data come from the Cambridge *Coravel* (squares), but two pairs of measurements are from OHP (circles), and there is one measurement of the primary (only) from the DAO (triangle, the highest point of all). OHP velocities that were reduced as single-lined and are therefore of no use for the orbit form the sequence of crosses; the plus represents the one 'original Cambridge' observation, the open triangle exactly at the cross-over of the velocity curves near phase ·1 is a single-lined result from the DAO, and the open star analogously near phase ·8 is one from the Cambridge *Coravel*.

of F8 and G6 V for the individual components of HD 115461; the (interpolated) tabular[11] ΔV between those types is in almost exact agreement with the $1^{m}\cdot 3$ estimated from the dip areas.

The spectral types and absolute magnitudes derived photometrically in the Yoss–Griffin survey[1] are naturally based on the assumption that the observed objects are single stars. Where that assumption is not fulfilled and the binary consists of unequal stars, as HD 115461 does, both the spectral type and the absolute magnitude must represent weighted means of the components' properties. Since the mean spectral type is necessarily later than that of the primary star, the absolute magnitude assigned to the system will be fainter than the true value for the primary alone, quite apart from not taking cognizance of the additional contribution of the secondary to the total luminosity of the system. Those considerations go a good way towards reconciling the absolute magnitudes found in the survey[1] and by *Hipparcos* for HD 115461, which as they stand differ by $1^{m}\cdot 3 \pm 0^{m}\cdot 5$ (the quoted uncertainty being that of the *Hipparcos* value alone).

HD 116247

HD 116247 is one of the most difficult stars of all in the NGP field to observe and to understand satisfactorily — even more so than HD 115461. For a start, it is unusually faint for an *HD* star. Moreover, despite its *HD* type of K0 it is actually of far earlier type than that — it has the colour index of a late-F star, and gives a correspondingly feeble dip in radial-velocity traces. Worst of all, it is double-lined, but the principal contribution to the dip is a wide feature arising from a star that has significant rotation, and the weak secondary dip is never resolved from it, moving merely from being blended on one side of the main dip to being blended on the other. Because the two dips are never seen separately, their width and depth parameters are not easily determined; they must be averaged from the results of reducing the best traces with all the parameters left 'free'.

The 7¼-year orbital cycle has been seen round three times since the star was placed under routine observation in 1989. The radial velocities are inevitably rather ragged, the primary's because of the width of its dip and the secondary's because they are measured from a weak dip blended with, and often superimposed upon, the primary's. Unlike the usual case, however, there is no extra difficulty in determining the twin velocities at times when they are almost identical, because the relatively sharp secondary feature is scarcely less visible when it is centred within the primary one than it is at other times. The observational situation is illustrated by Fig. 3, which shows a nodal trace that is representative of the best that can be obtained of the star with the available instrumentation.

The literature includes a catalogue[12] (not retrieved by *Simbad*) of radial velocities, by Woolley *et al.*; it includes two measurements of HD 116247. They were obtained photographically in 1967 with a prism spectrograph giving 66 Å mm^{-1} at Hγ on the Kottamia 74-inch telescope, but being single-lined and moreover having listed standard errors of about 6 km s^{-1} they are not of utility here. A spectral classification of G5 IV was made from them. The first photoelectric radial-velocity observation of HD 116247 (as of all the other stars discussed in this paper, as noted above) was made by G. A. Radford, in this case in 1973, with the original photoelectric spectrometer[7]; a second, in reasonable agreement with the first, was made by the writer with the same instrument, but not until 14 years later. When the star was re-observed with the OHP *Coravel*, in 1989, the velocity had clearly changed, so the star was placed on the spectroscopic-binary programme. Not until the 14th measurement with that instrument, however, in early 1994, when (as we can now see) the system was right at the more favourable node of the orbit, was the double-lined nature of the trace recognized. Although in principle all of the subsequent *Coravel* observations, which were given greatly increased integration times in comparison with the earlier ones, ought to able to yield velocities for both components of the system, the available reductions treat only two of those

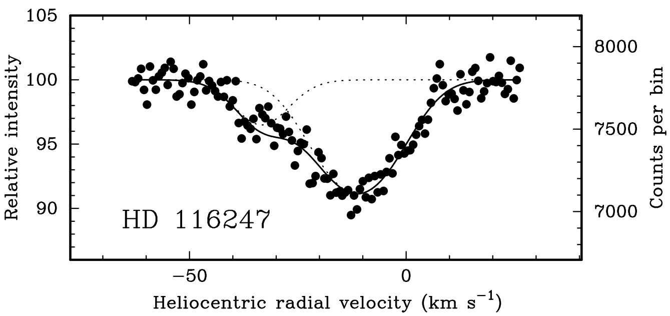

FIG. 3

Radial-velocity trace of HD 116247, obtained with the Cambridge *Coravel* on 2008 June 30, right at the node of the orbit (ϕ = ·081), where the velocity separation of the components is as large as it ever gets.

TABLE IV

Radial-velocity observations of HD 116247

Except as noted, the sources of the observations are as follows:
1989–1998 — OHP Coravel (weight ½); 1999–2010 — Cambridge Coravel (weight 1)

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}
1973 Mar. 30·11*R	41771·11	−15·3		0·207	—	—
1987 Mar. 21·13*	46875·13	−13·2		2·139	—	—
1989 Mar. 29·11	47614·11	−20·7		2·419	—	—
May 3·04	649·04	−20·9		·432	—	—
30·97*	676·97	−21·0		·443	—	—
1990 Jan. 27·16	47918·16	−21·9		2·534	—	—
Feb. 12·38†	934·38	−21·7		·540	—	—
Mar. 27·07*	977·07	−23·4		·556	—	—
Apr. 30·99*	48011·99	−24·1		·569	—	—
1991 Jan. 28·19	48284·19	−24·0		2·672	—	—
May 25·01*	401·01	−23·5		·717	—	—
1992 Jan. 20·22	48641·22	−24·0		2·807	—	—
Apr. 29·09	741·09	−29·3		·845	—	—
June 26·98	799·98	−29·3		·868	—	—
Aug. 13·86	847·86	−28·6		·886	—	—
1993 Feb. 18·15	49036·15	−25·9		2·957	—	—
Mar. 19·12	065·12	−26·5		·968	—	—
July 7·95	175·95	−19·7		3·010	—	—
Sept. 12·79	242·79	−15·6		·035	—	—
Dec. 30·23	351·23	−11·6		·076	—	—
1994 Feb. 16·17	49399·17	−9·6	−29·1	3·094	+0·8	+4·2
May 1·08	473·08	−12·1	−34·5	·122	−1·0	−2·1
Aug. 2·90	566·90	−16·1		·158	—	—
1995 Jan. 8·23	49725·23	−17·6		3·218	—	—
June 6·01	874·01	−19·7		·274	—	—
1996 Apr. 1·09	50174·09	−21·3		3·388	—	—
1997 Apr. 16·08‡	50554·08	−22·2		3·531	—	—
Sept. 9·81	700·81	−23·3		·587	—	—
1998 July 9·94	51003·94	−24·6		3·702	—	—
1999 July 10·26§	51369·26	−30·4		3·840	—	—
Dec. 27·26	539·26	−31·1	−9·3	·904	−0·3	+0·2
2000 Jan. 9·24	51552·24	−31·2	−11·0	3·909	−0·4	−1·5
Feb. 11·20	585·20	−30·3	−10·7	·922	+0·4	−1·1
Mar. 4·15	607·15	−30·4	−11·8	·930	+0·2	−2·0
Apr. 24·04	658·04	−30·0	−11·2	·949	−0·5	−0·1
June 12·94	707·94	−26·9	−15·7	·968	+0·3	−1·9
July 21·92	746·92	−24·4	−17·9	·983	−0·2	−0·6
2001 Mar. 12·16	51980·16	−9·6	−35·7	4·071	+0·6	−2·2
May 8·05	52037·05	−10·2	−34·3	·093	+0·2	−1·0
June 30·97	090·97	−11·5	−34·7	·113	−0·7	−1·9

TABLE IV (*concluded*)

Date (UT)	*MJD*	*Velocity* Prim. km s^{-1}	Sec. km s^{-1}	*Phase*	*(O–C)* Prim. km s^{-1}	Sec. km s^{-1}
2002 Apr. 6·08	52370·08	−14·6	−29·4	4·219	−0·4	−0·6
May 30·97	424·97	−14·1	−30·2	·239	+0·8	−2·1
2003 Apr. 7·05	52736·05	−18·8	−25·3	4·357	−0·8	−0·9
June 5·00	795·00	−18·0	−25·9	·379	+0·6	−2·1
2004 Apr. 17·01	53112·01	−20·9	−22·7	4·499	+0·4	−2·1
May 24·02	149·02	−20·1	−24·4	·513	+1·5	−4·2
2005 May 7·99	53497·99	−25·0	−16·0	4·646	−0·4	+0·7
July 17·93	568·93	−24·3	−19·2	·672	+1·0	−3·2
2006 Mar. 23·12	53817·12	−28·5	−11·8	4·766	−0·9	+1·5
June 11·98	897·98	−27·7	−13·0	·797	+0·7	−0·7
2007 Feb. 4·19	54135·19	−31·5	−12·5	4·887	−0·9	−2·8
Apr. 2·05	192·05	−29·7	−5·7	·908	+1·1	+3·8
July 12·94	293·94	−29·5	−12·3	·947	+0·2	−1·4
2008 Mar. 31·09	54556·09	−11·2	−33·1	5·046	0·0	−0·8
May 2·99	588·99	−10·0	−32·0	·058	+0·5	+1·1
June 30·98	647·98	−10·5	−32·7	·081	−0·3	+0·8
2009 Mar. 27·12	54917·12	−13·9	−32·1	5·183	−0·8	−2·0
May 19·99	970·99	−12·7	−28·8	·203	+1·0	+0·6
2010 Mar. 23·12	55278·12	−17·3	−28·0	5·319	−0·2	−2·5
May 12·00	328·00	−17·8	−22·9	·338	−0·2	+2·0

* Observed with original spectrometer.
† Observed with ESO *Coravel*.
‡ Observed with Cambridge *Coravel*.
§ Observed with DAO 48-inch telescope.
R Observed by Dr. G. A. Radford.

measurements as double-lined; the others are wasted as far as the determination of the orbit is concerned. Since the observing programme was transferred back to Cambridge in 1999, all the traces have been reduced as double-lined. The data are set out in Table IV. In determining the orbit, it has been necessary to give the velocities of the secondary a relative weighting of 1/10 to equalize the variances of the components; the two OHP measures of each component have been weighted ½ in comparison with the Cambridge data. The resulting orbit is shown in Fig. 4 and has the following elements:

P =	2642 ± 15 days	$(T)_4$ =	MJD 51792 ± 9
γ =	−20·98 ± 0·13 km s^{-1}	$a_1 \sin i$ =	312 ± 7 Gm
K_1 =	10·32 ± 0·17 km s^{-1}	$a_2 \sin i$ =	363 ± 16 Gm
K_2 =	12·0 ± 0·5 km s^{-1}	$f(m_1)$ =	0·174 ± 0·011 $M_\odot$
q =	1·16 ± 0·05 (= m_1/m_2)	$f(m_2)$ =	0·27 ± 0·04 $M_\odot$
e =	0·553 ± 0·015	$m_1 \sin^3 i$ =	0·95 ± 0·10 $M_\odot$
ω =	274·5 ± 2·0 degrees	$m_2 \sin^3 i$ =	0·81 ± 0·06 $M_\odot$

R.m.s. residual (unit weight) = 0·63 km s^{-1}

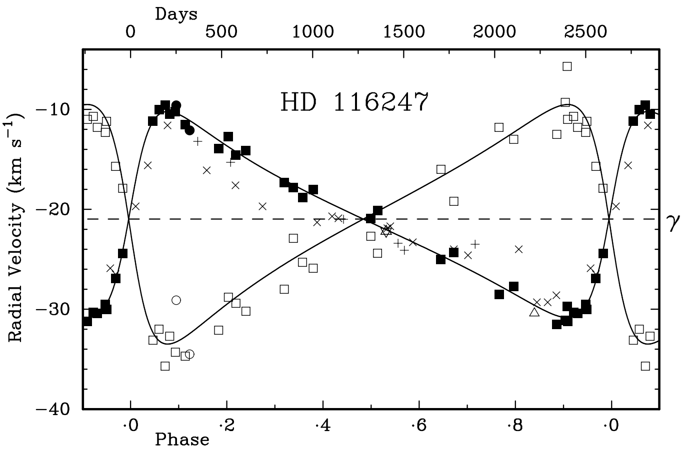

FIG. 4

As Fig. 2, but for HD 116247. The coding of the symbols is identical with that in Fig. 2.

Fig. 4 illustrates the raggedness of (particularly) the secondary velocities, and also how the traces that were reduced as single-lined yield velocities that are close to the ephemeris for the primary near the nodes but tend to fall nearer to the γ-velocity at other phases.

The projected rotational velocity adopted for the primary component of HD 116247, averaged from the best traces, is 15 km s^{-1}; the secondary dip does not appear to be significantly widened, so its rotation has been taken as zero, while its strength (area) has been taken as ¼ of that of the primary. The 4:1 ratio, expressed in stellar-magnitude terms, is very close to $1^{m}\cdot5$, and application of the empirical factor[9] of 1·15, noted in the section on HD 115461 above, between dip areas and *V* magnitudes leads to the estimate $\Delta V = 1^{m}\cdot7$. To obtain the correct combined colour index, that magnitude difference would imply individual spectral types of about F7V and G7V. The mass ratio, however, expressed as $\Delta(\log m) \sim 0\cdot065 \pm 0\cdot022$, indicates, according to the other empirical 'rule' noted above, a difference of only about 5 ± 2 subtypes between the components. In view of the observational difficulties posed by HD 116247 the discrepancy, though disappointing, is probably not to be regarded very seriously; a compromise of about F7 + G5 is the best that can be suggested. If it were thought desirable to accommodate the discrepancy otherwise than as observational error, that could easily be accomplished by supposing the primary star to have made a significant start on its evolution and now to be half a magnitude or so above the main sequence. Indeed, in support of such an idea we could recall Woolley *et al.*'s classification[12] of the integrated light of the system as luminosity class IV; but since we could still not accept the temperature type of G5 associated with that luminosity class (it is much too late to correspond to the colour index) it does not seem equitable to clutch at that particular straw. When a good parallax is available for the object, it will enable a reliable adjudication to be made.

HD 116345

Unlike the two stars treated above, HD 116345 is a single-lined object; its visible component is a (probably 'clump') giant on the lower fringes of the luminosity of such stars. The only literature on the star, as mentioned in the Introduction above, consists of *UBV* measurements by Häggkvist & Oja[2] and its inclusion in an investigation by Soubiran *et al.*[3]; the latter is divided between two papers, the first of which refers to a computer-accessible table that includes an undated radial-velocity measurement. Certain other parameters are also given. In the earlier of the two papers, both of which were largely based on low-*S/N* spectra taken with the *Elodie*[13] spectrograph on the OHP 1·93-m telescope, the star's absolute *V* magnitude was given as $+1^{m}·414$ and the z-distance as 367·0 pc; despite the precision with which those quantities were given, in the later paper M_V had become $+2^{m}·152$ and z 267 pc, not far from the survey[1] results (Table I above) of $+2^{m}·3$ and 241 pc. In both papers a metallicity very close to solar was listed.

Just as in the case of HD 116247 above, for HD 116345 there was an interval of 14 years between the first two radial-velocity observations, both of which were made with the original instrument at Cambridge. The discrepancy between them was at the margin of significance and was not acted upon; it was a very definite discordance between the first two OHP measurements that led to the star's transfer to the spectroscopic-binary observing programme in 1992. Since then it has been seen round a dozen cycles of its orbit, which has the relatively short period of about 1·6 years. All the radial velocities are listed in Table V. The majority has been made with the Cambridge *Coravel*, whose measurements have been adjusted by –0·4 km s^{-1} from the 'as-reduced' values; the OHP velocities, and (arbitrarily) the single DAO one, have been half-weighted in the solution of the orbit, and the two 'original Cambridge' ones have been weighted ¹⁄₁₀. On that basis the orbital elements are:

P	=	591·7 ± 0·7 days	$(T_0)_{18}$	=	MJD 51953·7 ± 2·3
γ	=	−5·71 ± 0·06 km s^{-1}	$a_1 \sin i$	=	29·8 ± 0·6 Gm
K	=	3·66 ± 0·08 km s^{-1}	$f(m)$	=	0·00301 ± 0·00019 $M_\odot$
e	≡	0			
ω	is undefined in a circular orbit		R.m.s. residual (wt. 1)	=	0·34 km s^{-1}

The elements are seen to represent the orbit as exactly circular (Fig. 5). If the eccentricity is left as a free parameter, it takes the value 0·043 ± 0·022 — nearly 2σ but unlikely to be significant. The sums of squares of the weighted deviations in the circular and eccentric cases are 5·73 and 5·26 (km s^{-1})^2, respectively. The latter figure represents 43 degrees of freedom which therefore have an average cost of 0·122 (km s^{-1})^2 apiece, the total number of observations being 49; the extra 0·47 (km s^{-1})^2 in the eccentric case is the price of the two extra degrees represented by e and ω, which evidently cost 0·235 each. The quotient 0·235/0·122 gives the *F*-ratio statistic, $F_{2,43} \sim 1·92$, which is nowhere near statistical significance since even the 10% point of $F_{2,43}$ is as much as 2·43. While that does not *prove* that the orbit is exactly circular, of course, it does show that the available observations are far from demonstrating any non-zero eccentricity.

TABLE V

Radial-velocity observations of HD 116345

Except as noted, the sources of the observations are as follows:
1989–1998 — OHP Coravel (weight ½); 2000–2009 — Cambridge Coravel (weight 1)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
1973 Apr. 26·06*R	41798·06	−4·1	0·837	−0·3
1987 Mar. 21·13*	46875·13	−7·1	9·417	+1·8
1989 Mar. 29·11	47614·11	−8·1	10·666	−0·5
1991 Feb. 5·18	48292·18	−4·3	11·812	0·0
1992 Feb. 27·48†	48679·48	−9·2	12·466	+0·1
Apr. 24·06	736·06	−7·9	·562	+1·2
June 25·97	798·97	−8·1	·668	−0·6
1993 Feb. 15·21	49033·21	−2·2	13·064	+0·1
Mar. 19·13	065·13	−3·1	·118	−0·1
July 7·96	175·96	−7·3	·306	−0·3
Dec. 30·23	351·23	−8·2	·602	+0·4
1994 Feb. 16·18	49399·18	−7·6	13·683	−0·4
May 2·05	474·05	−4·9	·809	−0·5
Aug. 3·89	567·89	−2·0	·968	+0·1
Dec. 13·22	699·22	−4·7	14·190	−0·3
1995 Jan. 3·24	49720·24	−5·6	14·225	−0·5
June 2·00	870·00	−9·9	·478	−0·6
1996 Mar. 31·09	50173·09	−2·4	14·991	−0·3
1997 Mar. 31·12‡	50538·12	−8·3	15·608	+0·3
Apr. 16·09‡	554·09	−8·3	·635	−0·2
May 7·04‡	575·04	−8·1	·670	−0·6
July 20·94	649·94	−4·2	·797	+0·5
1998 July 7·93	51001·93	−8·0	16·391	+0·5
2000 Jan. 9·25	51552·25	−7·1	17·321	+0·2
Mar. 25·11	628·11	−9·7	·450	−0·5
Apr. 30·99	664·99	−9·4	·512	0·0
June 17·96	712·96	−8·5	·593	+0·3
2001 Jan. 7·27	51916·27	−2·4	17·937	−0·1
Mar. 3·19	971·19	−1·9	18·029	+0·2
May 30·99	52059·99	−5·1	·180	−1·0
2002 Feb. 23·20	52328·20	−7·8	18·633	+0·4
Mar. 30·14	363·14	−6·6	·692	+0·4
Apr. 20·08	384·08	−6·3	·727	−0·1
May 17·02	411·02	−5·0	·773	+0·2
2003 Mar. 16·13	52714·13	−6·4	19·285	+0·1
May 16·01	775·01	−8·6	·388	−0·1
June 14·95	804·95	−8·7	·439	+0·4
2004 Feb. 26·19	53061·19	−3·3	19·872	−0·1
Mar. 17·16	081·16	−2·3	·905	+0·4
Apr. 15·07	110·07	−2·2	·954	0·0
May 7·04	132·04	−2·1	·991	0·0
June 12·95	168·95	−2·2	20·054	+0·1
21·96	177·96	−1·9	·069	+0·5

TABLE V (*concluded*)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O – C) km s⁻¹*
2005 Mar. 25·09	53454·09	−9·4	20·536	−0·1
2006 Apr. 6·04	53831·04	−3·8	21·173	+0·2
2007 Mar. 22·14	54181·14	−6·0	21·764	−0·6
May 8·01	228·01	−3·7	·844	0·0
2008 July 8·94	54655·94	−9·0	22·567	0·0
2009 Mar. 30·09	54920·09	−1·7	23·013	+0·4

* Observed with original spectrometer; wt. 1/10.
† Observed with DAO 48-inch telescope; wt. ½.
‡ Observed with Cambridge *Coravel*; weight ½.
R Observed by Dr. G. A. Radford.

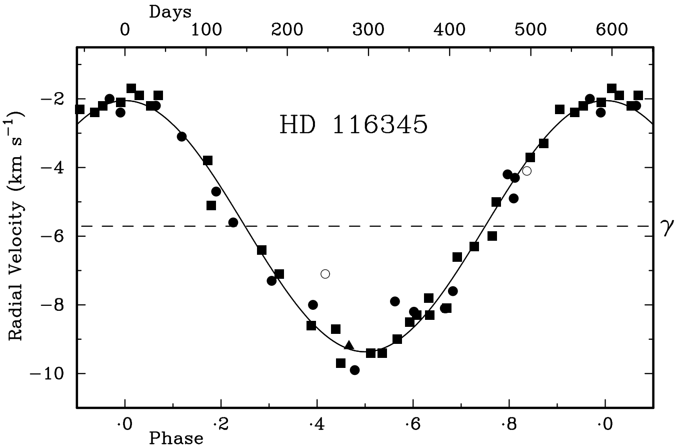

FIG. 5

The observed radial velocities of HD 116345 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. As before, the filled squares, circles, and triangles represent, respectively, observations made with the Cambridge *Coravel*, OHP *Coravel*, and at the DAO. Here, however, the two measurements made with the original Cambridge spectrometer are represented by open circles; it is to be admitted that the 'bad' one is the one that was made by the writer, not by Dr. Radford!

The circularity of the orbit at a period well beyond the one (about 100–120 days) at which even giant stars begin routinely to exhibit eccentric orbits might be held to suggest that it has been tidally circularized, most likely during the epoch when the present primary, or possibly the secondary before it, was at the evolutionary stage of maximum expansion. The likelihood that the primary has passed that stage and is now a 'clump' giant could probably be assessed by a determination of its lithium abundance. The secondary star has not been detected, but if it had passed through its giant-branch evolution, in this system whose period is shorter than those of many barium stars, we might expect

Soubiran *et al.*'s (admittedly automated) analyses to have flagged up a 'barium' peculiarity — which they did not. There is, therefore, some reason to incline towards the conclusion that the secondary is more likely to be a lower-main-sequence star than a white dwarf. The mass function is very small, and does not require the secondary to have a mass of more than about 0·25 $M_\odot$ if the primary is supposed to be 2 $M_\odot$. A sufficiently low orbital inclination, however, would of course allow the secondary to be of any larger mass and thereby enhance its ability to circularize the orbit; just as an example, sin *i* would have to be about 0·3 ($i \sim 17°·5$) to allow it to be 1 $M_\odot$.

TABLE VI

Radial-velocity observations of HD 120006

Except as noted, the sources of the observations are as follows:
1989–1998 — OHP Coravel (weight 1); 2000–2010 — Cambridge Coravel (weight 1)

Date (UT)	MJD	Velocity km s^{-1}	Phase	(O – C) km s^{-1}
1973 Mar. 31·16*R	41772·16	+19·1	0·879	+2·7
1987 Mar. 26·07*	46880·07	15·5	2·193	−0·1
1988 Feb. 1·55†	47192·55	17·3	2·274	+0·5
1989 Mar. 27·13	47612·13	17·2	2·382	−0·4
1991 Feb. 5·21	48292·21	18·5	2·557	+0·2
1992 May 1·10	48743·10	19·0	2·673	+0·7
1993 July 11·94	49179·94	17·6	2·785	−0·3
1994 May 4·01	49476·01	16·7	2·861	−0·1
Aug. 5·89	569·89	15·4	·886	−0·8
1995 Jan. 8·18	49725·18	15·0	2·926	+0·3
June 3·04	871·04	12·4	·963	−0·2
Dec. 27·22	50078·22	9·7	3·016	−0·9
1996 Mar. 29·10	50171·10	11·0	3·040	0·0
Aug. 22·83U	317·83	12·2	·078	−0·2
1997 Feb. 8·21‡	50487·21	14·6	3·122	+0·6
Mar. 1·16‡	508·16	14·2	·127	+0·1
Apr. 16·10‡	554·10	13·7	·139	−0·8
May 10·05‡	578·05	14·5	·145	−0·1
July 19·96	648·96	15·7	·163	+0·7
1998 May 4·09	50937·09	15·9	3·237	−0·4
July 7·96	51001·96	16·9	·254	+0·4
1999 Apr. 9·44†	51277·44	16·8	3·325	−0·4
July 10·30†	369·30	17·3	·349	−0·1
2000 Jan. 9·28	51552·28	17·9	3·396	+0·2
Apr. 6·08	640·08	17·2	·418	−0·6
May 13·98	677·98	17·6	·428	−0·3
June 17·98	712·98	17·6	·437	−0·3
Aug. 2·90	758·90	+17·8	·449	−0·2

TABLE VI (*concluded*)

Date (UT)		*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2001	Mar. 3·21	51971·21	+17·9	3·503	−0·3
	Aug. 1·88	52122·88	18·6	·542	+0·3
	Dec. 20·25	263·25	17·2	·579	−1·1
2002	Feb. 24·17	52329·17	18·5	3·596	+0·1
	Apr. 24·06	388·06	18·6	·611	+0·2
	June 22·97	447·97	18·9	·626	+0·5
	Sept. 2·83	519·83	18·0	·645	−0·4
2003	Mar. 16·14	52714·14	18·3	3·695	0·0
	May 16·06	775·06	18·0	·710	−0·3
	July 15·91	835·91	18·0	·726	−0·2
2004	Jan. 15·24	53019·24	18·9	3·773	+0·9
	Mar. 31·11	095·11	18·5	·793	+0·7
	May 24·03	149·03	17·9	·807	+0·2
	Sept. 7·82	255·82	17·1	·834	−0·2
2005	Jan. 9·24	53379·24	17·3	3·866	+0·6
	Mar. 23·17	452·17	16·0	·885	−0·2
	Apr. 22·05	482·05	16·1	·892	+0·1
	May 23·01	513·01	15·5	·900	−0·2
	July 16·91	567·91	15·3	·914	+0·1
	Sept. 12·81	625·81	14·0	·929	−0·5
	Dec. 17·27	721·27	12·7	·954	−0·5
2006	Jan. 27·28	53762·28	12·4	3·964	−0·1
	Mar. 1·17	795·17	12·1	·973	+0·1
	Apr. 4·09	829·09	11·8	·982	+0·2
	May 9·97	864·97	12·0	·991	+0·9
	June 3·98	889·98	11·3	·997	+0·4
	July 3·93	919·93	10·7	4·005	0·0
	Aug. 7·87	954·87	10·8	·014	+0·2
2007	Jan. 14·28	54114·28	11·3	4·055	−0·2
	Feb. 6·21	137·21	11·5	·061	−0·3
	Mar. 2·20	161·20	11·7	·067	−0·3
	Apr. 2·08	192·08	12·4	·075	+0·1
	May 2·06	222·06	12·8	·083	+0·2
	June 1·03	252·03	13·1	·090	+0·2
	July 12·92	293·92	13·6	·101	+0·3
2008	Feb. 16·21	54512·21	15·3	4·157	+0·4
	June 25·93	642·93	15·5	·191	−0·1
2009	Jan. 6·29	54837·29	17·0	4·241	+0·6
	Apr. 22·06	943·06	16·7	·268	0·0
	July 15·90	55027·90	16·5	·290	−0·4
	Sept. 10·81	084·81	16·9	·305	−0·2
2010	Feb. 1·27	55228·27	17·7	4·342	+0·3
	May 12·02	328·02	18·0	·367	+0·5
	Aug. 15·87	423·87	+17·4	·392	−0·3

* Observed with original spectrometer; wt. 1/10.
† Observed with DAO 48-inch telescope; wt. 1.
‡ Observed with Cambridge *Coravel*; weight 1.
R Observed by Dr. G. A. Radford.
U Observed by Dr. S. Udry.

HD 120006

What little information there is in the literature about this star has already been indicated in the Introduction above; it does no more than reinforce the photometry in Table I (though adding a $(U-B)$ colour index) and disclosing a mean velocity that is probably based on a subset of the data set out here in Table VI. Once again there was a 14-year gap, between 1973 and 1987, before the second measurement was made of the star's radial velocity. The discordance was bad enough to prompt annual measurements but nothing more intensive; for several years the annual measures were near the mean of the first two, and it was not until 1995 that the star was definitely regarded as a binary and observed somewhat more frequently. Rather sketchy coverage of a velocity minimum was obtained in 1995/7; the star was retained on the observing programme and watched much more carefully when that phase recurred in 2006/7. In the computing of the orbit, in this instance all the data were given equal weight except for the two measurements made with the original spectrometer, which were weighted only 1⁄10. The orbital elements are shown below, and the corresponding velocity curve is shown together with the data points in Fig. 6.

$P = 3886 \pm 17$ days	$(T)_4 = $ MJD 53901 ± 18
$\gamma = +16{\cdot}30 \pm 0{\cdot}06$ km s^{-1}	$a_1 \sin i = 180 \pm 5$ Gm
$K = 3{\cdot}88 \pm 0{\cdot}09$ km s^{-1}	$f(m) = 0{\cdot}0155 \pm 0{\cdot}0012\, M_{\odot}$
$e = 0{\cdot}494 \pm 0{\cdot}016$	
$\omega = 161{\cdot}3 \pm 2{\cdot}6$ degrees	R.m.s. residual (wt. 1) $= 0{\cdot}43$ km s^{-1}

Since HD 120006 is considerably ($\sim 1^{\rm m}$) brighter than the other stars treated in this paper, and, being a late-K giant, gives agreeably deep dips in radial-velocity traces, the residuals from the orbit are a bit disappointing; they may be inflated by real jitter in the star's velocities, but the writer does not care to assert that as a fact. If the primary's mass is arbitrarily taken to be 2 $M_{\odot}$, then according to the mass function the secondary must be at least 0·45 $M_{\odot}$ — so it could be no later than type M0 if on the main sequence.

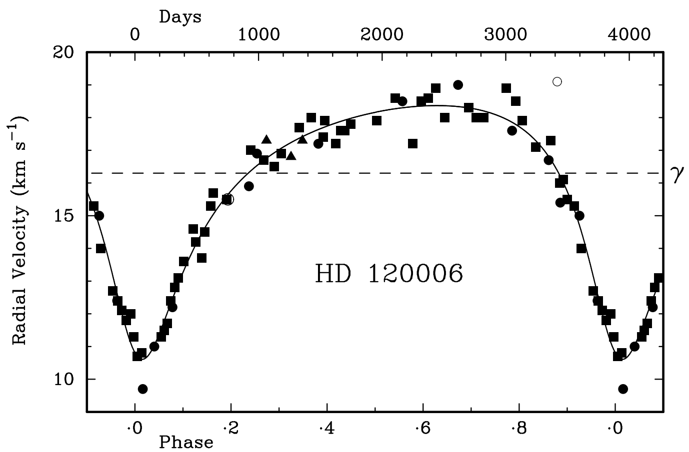

FIG. 6

As Fig. 5, but for HD 120006. (But this time the writer's own 'original Cambridge' observation is the 'good' one!)

Acknowledgement

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PERIODIC BEHAVIOUR OF STARS IN THE GEOS RR LYRAE DATABASE

PAPER 4: THE LONG-TERM BEHAVIOUR OF BLAZHKO'S STAR, RW DRA

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The GEOS database on RR Lyr variable stars has been used to analyse 680 times of maximum obtained over 104 years for the star RW Dra, Blazhko's star. Between approximately 1914 and 1938 the pulsation period of the star went through four large changes, including three occasions when the pulsation period changed by a significant fraction of one per cent over a few hundred cycles. These rapid period changes stopped in about 1940. From approximately 1940 until 1959 the pulsation period was longer than average and slightly variable. This was followed by about twelve years when the pulsation period was shorter than average. Since approximately 1968 the period has probably been constant. The Blazhko period of the star has varied over the 104 years between values of approximately 41·3 days and 42 days.

It is shown that both the shape and amplitude of the Blazhko phase diagram changes but these changes are not clearly correlated with pulsation-period activity. Even during the most chaotic pulsation-period activity the Blazhko phase diagram remained well defined. There is shown to be a strong correlation between the O – C behaviour of both the pulsation and Blazhko variations that would support models for the origin of the Blazhko effect which require there to be beats between the pulsation period and a hypothetical second period. The long-term variation of the Blazhko effect can be explained by assuming that there is a single second frequency which beats with the pulsation period, and changes in the pulsation period are the cause of the changes in the Blazhko period. However, on shorter time-scales the nature of this coupled behaviour changes, suggesting either that the second frequency varies or that other effects can occur. An understanding of these changes will act as constraints on models for the origin of the Blazhko effect in this star.

Introduction

The star RW Dra was discovered to be variable by Ceraski[1] in 1906. In 1907, Blazhko[2] discovered that in addition to the 0·44-day pulsation period there was also a 41·6-day modulation of the pulsation period. Since that time many other RR Lyr stars have been discovered to show these longer-term variations, which are now known as Blazhko variations, in honour of their discoverer. RW Dra was chosen for investigation from the GEOS[3,4] RR Lyr database because of the complex shape of its O – C diagram and the fact that there are 680 times of maximum listed between 1906 and 2010. The pulsation period listed in the GEOS database is 0·442917 days but a slight change to a period of 0·4429145 days was used to produce the O – C diagram in Fig. 1, which now has similar O – C values at both the start and end of the data.

Fig. 1 shows that the period has not been constant over the 104 years covered by the data. The gradients seen in Fig. 1 have been used to calculate new periods for subsets of the data. There was a major change in period at about HJD 2 400 000 (hereinafter omitted) +29 000 (1938). The data from HJD +30 148 (1941) until the latest data in 2010 would be better fitted by a period of 0·4429385 days. From HJD +23 295 (1915) to HJD +29 115 (1938), the data would be better fitted by a period of 0·4427895 days. Thus a pulsation-period change of about 3 parts in 10^4 took place between 1938 and 1941. There are insufficient data between the start of the data in 1906 and 1915 to say anything definite about the best-fit period there. The overall gradient, and hence the average period, is about the same as that in the latter approximately two thirds of the data but, as will be discussed below, even that is not constant. Inspection of Fig. 1 shows that there was also a period change at about HJD +25 379 (1927).

In order to get a better understanding of these period changes, the data in Fig. 1 have been modified by removing two gradients equivalent to the pulsation-period changes given above. The data from just after the major discontinuity near to 1938/41 until 2010 have had a single gradient removed from them, equivalent to adopting the long-term pulsation period of 0·4429385 days. The data between 1915 and 1938 have had another single-valued gradient removed,

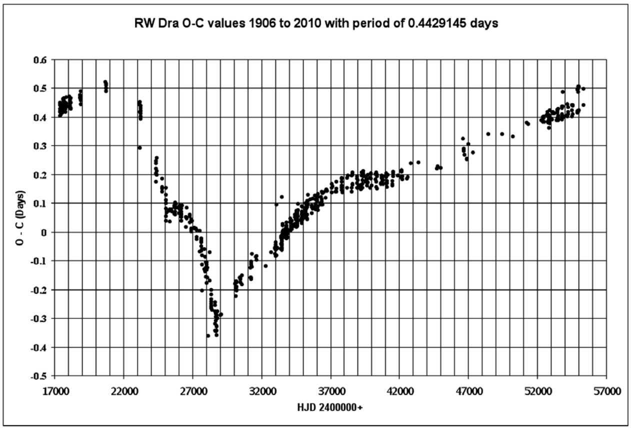

FIG. 1

The modified GEOS O – C values for RW Dra from 1906 to 2010 plotted with an ephemeris of 2434139·314 + 0·4429145 *E* days

equivalent to the long-term pulsation period of 0·4427895 days. Finally, the small amount of data between 1906 and 1915 has been plotted unmodified. It should be emphasized that the gradients removed are arbitrary, as the data do not allow any single solution to the whole data set, but their removal allows the smaller-scale structure in the O – C diagram to be investigated. In what follows, the exact O – C values used for these data do not influence any of the results. The resulting modified O – C values have been used to produce Fig. 2, where the increased vertical scale now allows the lack of stability of the pulsation period to be seen more clearly. Recalling that two period changes have already been incorporated before the values in Fig. 2 were obtained, it can now be seen just how variable the pulsation period has been.

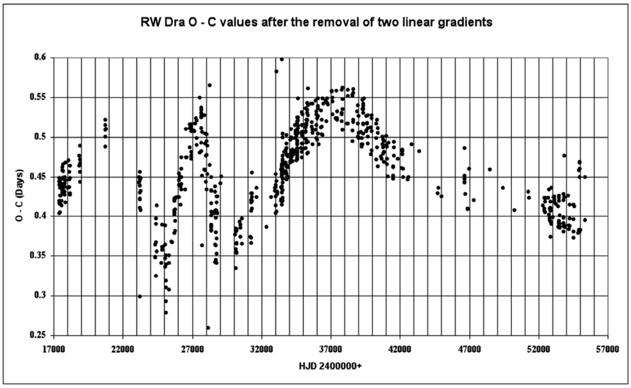

FIG. 2

O – C values from 1906 to 2010 after two gradients, representing two different pulsation periods, have been removed.

Discussion of the pulsation-period changes

Considering only the data from HJD +30 148 (1941) until 2010, and noting that only a single gradient has been removed from these data, it can be seen that there was a longer period (O – C values becoming more positive) from HJD +30 148 (1941) until about HJD +36 695 (1959), and then a shorter period (O – C values decreasing) until about HJD +43 000 (1968), after which the period was essentially constant. The data between about HJD +20 000 (1913) and HJD +30 000 (1941) show period changes at about HJD +22 000, 25 000, 27 500, and 29 000. Nothing definite can be said about the earliest data, 1906–1915, except that the period seems to have been constant with no rapid changes in period over that interval. It should be noted that in order to obtain the O – C values plotted in Fig. 2 there has been a period change of about 3 parts in 10^4, between 1938 and 1941, already incorporated, and thus the additional rapid change in period seen at the same time in Fig. 2 shows just how large that period change was.

How rapid are the pulsation-period changes?

There are only two O – C values, both in 1938, between the rapidly decreasing O – C values before 1937 and the increasing O – C values after 1941. These data indicate that the time interval over which the period changed is less than about three years. If the two points in 1938 are part of the later pulsation-period régime then this change in period occurred in less than about 235 days or 530 pulsation periods. The change in period at about HJD +25 000 (1927) is more completely covered. Detailed inspection of the data over this interval shows that the change in period occurred between the 1927 and 1928 observing seasons. The gap in the data there is 205 days or about 460 pulsation cycles. It is possible that the change in period is more rapid than this but it cannot be much longer. A period change with a similar value, but in the opposite sense, also occurred near to HJD +27 500 (1934). It is possible that the period change took place in the 153-day gap — 345 pulsation periods — between the 1934 and 1935 observing seasons. However, it should be noted that noise in the data means that the exact duration over which the transitions took place cannot be accurately determined. Table I gives the details of the three period changes discussed above. Even if RW Dra is a pathological example of an RR Lyr variable, this still has implications for models of RR Lyr pulsation. The gradients in Fig. 2 before and after the 1938 to 1941 period change indicate that its value was about 8 parts in 10^4 of the average pulsation period. However, it should be noted that a period change of approximately 3 parts in 10^4 has already been removed from the raw data in Fig. 1 in order to obtain Fig. 2. Thus the real period change between 1938 and 1941 was about 0·1%, with lower values at other times.

TABLE I

Details of three pulsation-period changes which occurred between 1927 and about 1938

Approximate HJD	*Approximate year*	*Data gap (days)*	*Period change (parts in 10^4)*
25000	1927	205	+1
27500	1934	153	−2
29000	1938	235(?)	+11

The Blazhko period

The period originally discovered by Blazhko was 41·6 days. Preliminary analysis of various subsets of the data shown in Fig. 2 showed that there was little variation in the Blazhko period, with most of the subsets having a period of just over 41 days. However, the detail in Fig. 2 means that either one has to use very short subsets of the data, which of necessity have larger standard errors, or larger subsets of the data, which have smaller standard errors but which tend to cover a variety of pulsation-period behaviours. The best-fit periods were determined by use of both the Discrete Fourier Transform (DFT)[5] and the Phase Dispersion Method (PDM)[6] as implemented in the PERANSO suite of period-finding programs[7]. Table II contains a list of Julian-date intervals weighted mean epochs, and the best-fit Blazhko periods, along with their standard errors, for those intervals. The earliest six-year subset of data has a Blazhko period of 41·55 days. The most recent ten years have a Blazhko period of 41·45 days. All the other subsets of data show a changing Blazhko period, with a value of 42·06 days when the rapid pulsation-period activity started, reducing to a value of 41·33 days 17 000 days later. Fig. 3 shows the best-fit Blazhko periods in graphical form where the middle (reduced) HJD of each subset of the data is used for the abscissa. The nature of the O – C values is so variable that these data intervals are of different durations and spacing.

The shape of the Blazhko phase diagrams

It became clear while finding the best-fit Blazhko periods to subsets of the data that the shape of the Blazhko phase diagram varied. The complexity of the pulsation-period changes, visible in Fig. 2, means that attempts to find any correlation between the shape of the Blazhko variation and the behaviour in Fig. 2 has not been possible. In earlier papers in this series it has been suggested that the true shape of a Blazhko-type variation is that of a slow reduction in O – C value followed by a more rapid return to the original value. If this is correct then it seems to encounter some difficulties with the variations in RW Dra. In Fig. 4 we show three different phase diagrams obtained from subsets of the data. The caption to each sub-frame gives the years from which the data were obtained. Note that the two top frames contain subsets of the data bins used for Table I and Fig. 4. The reason for this is that the shape of the Blazhko phase diagram can change so quickly that, although it is useful to take large data bins

TABLE II

The best-fit Blazhko periods and their standard errors for nine sub-sets of the data

Bin Number	*Start* (HJD 2 400 000+)	*End* (HJD 2 400 000+)	*Mean* (HJD 2 400 000+)	*Period Days*	*s.e. Days*
1	17422	20795	18166	41·55	0·14
2	23204	25159	24244	42·06	0·22
3	25159	27597	26378	41·85	0·30
4	27597	29000	28299	41·83	0·32
5	30000	32000	31000	41·66	0·03
6	33000	37000	35000	41·50	0·09
7	36000	39500	37750	41·42	0·06
8	39000	43000	41000	41·33	0·06
9	52338	55408	53873	41·45	0·13

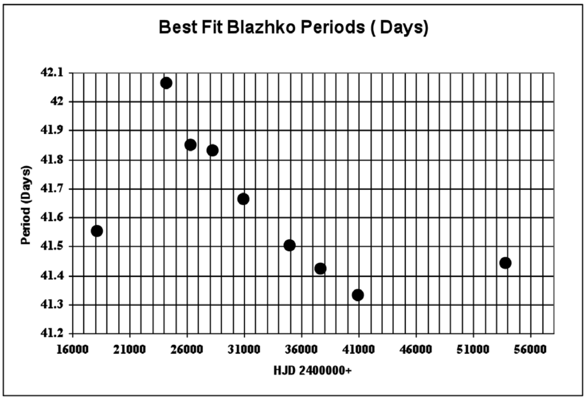

FIG. 3
The best-fit Blazhko periods to the data subsets listed in Table I.

in order to obtain the best-fit period, these large data bins reduce the resolution when we try to demonstrate the changing shape of the Blazhko phase diagrams. The top sub-frame corresponds to a time when there was a rapid reduction in the O–C values prior to the rapid period change between 1938 and 1941. The shape of this curve is what we have previously interpreted as the 'classic' Blazhko shape. It corresponds to a retardation in the time of the pulsation-light-curve maximum, taking about 60% of the total period, while the return takes about 40%. The middle sub-frame corresponds to the years immediately following the rapid change in period between 1938 and 1941 when the pulsation period seemed to be stable. This subset of the data gives an asymmetric phase diagram with broad maxima and rather sharp minima. The bottom sub-frame contains all the most recent nine years of data, during which the pulsation period seems to have been stable, but slightly different from that for the data used in the middle phase diagram. The shape is such that the duration of the phase of reducing O–C value takes only about 40% of the total period, while the return takes the remaining 60%. This is a reversal of what occurred in the top sub-frame and which was also found in RR Lyr[8] and AR Her[9]. It should also be noted that the three sub-frames have all been plotted with the same vertical scale. The peak-to-peak amplitude in the top sub-frame is about 0·08 days while in the two lower sub-frames this value is reduced to about 0·06 and 0·04. Thus not only the shape of the variations changes but so also does the amplitude.

The Blazhko O–C diagram

Smith[10] drew attention to the discovery by Tsesevich[11] that the Blazhko O–C diagram for RW Dra varied in anti-phase to the pulsation O–C diagram. The time base of the present set of observations is almost double that available to Tsesevich and therefore a new Blazhko O–C diagram has been calculated, including all the data from 1906 to 2010. Fig. 5 shows the results, and when this is compared with Fig. 1 it will be seen that the suggested anti-phase behaviour has continued over the whole of the present data set. When dealing with the pulsation O–C diagram — Fig. 1 — two gradients were derived from the data, which allowed the determination of mean pulsation periods for the epochs 1915

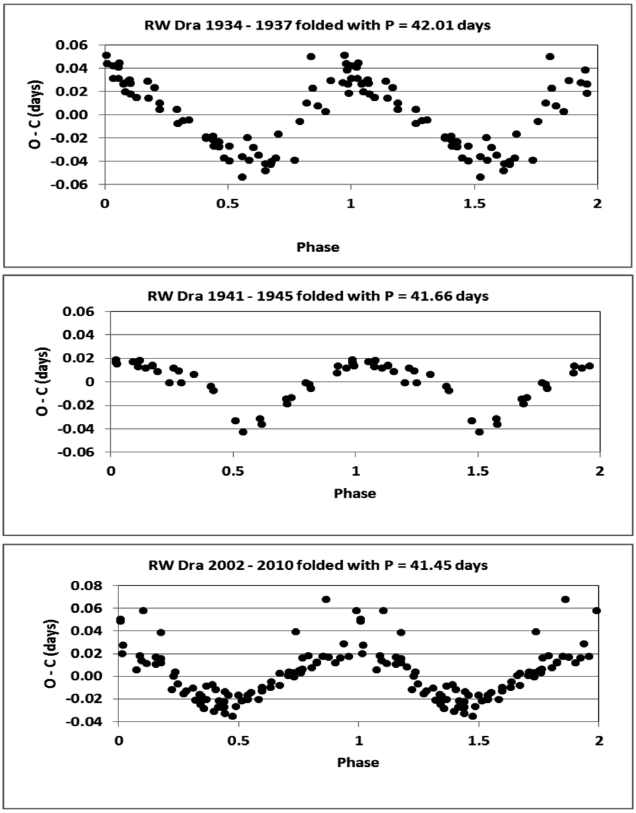

FIG. 4

Three Blazhko phase diagrams. The dates over which the data were obtained are shown in the captions to each sub-frame. Note how the amplitude of the variations in sub-frames differs and the phase duration of rising and falling varies. The phases are arbitrary as all the periods are different. The top figure is a subset of the data in bin 4 in Table II, the middle figure is a subset of bin 5, and the bottom figure contains all the data in bin 9.

to 1938 and 1941 to 2010. Those periods were 0·4427895 days and 0·4429385 days, respectively. They differ by 0·0001490 days, which is 0·00034 of the mean period. The same procedure can be applied to the data in Fig. 5 for the Blazhko O–C values. In that case the earlier data from about HJD +23000 (1921) to about HJD +29000 (1938) have a mean Blazhko period of 41·5558 days while the later data, from about HJD +36000 (1957) to about HJD +54000 (2006), have a mean Blazhko period 41·5426 days. The difference between these two periods is 0·0132 days, which is 0·00032 of the mean Blazhko period. There is nothing in the derivation of either the pulsation or Blazhko O–C values which would cause them to have related values and therefore the agreement between the two values of 0·00034 and 0·00032 is likely to be significant.

In Fig. 1 it can be seen that the total range of O – C values for the pulsation period is approximately 0·8 days. This is 1·8 times the mean pulsation period. From Fig. 5 it can be seen that the total range of O–C values for the Blazhko period is approximately 70 days, which is 1·7 times the mean Blazhko period.

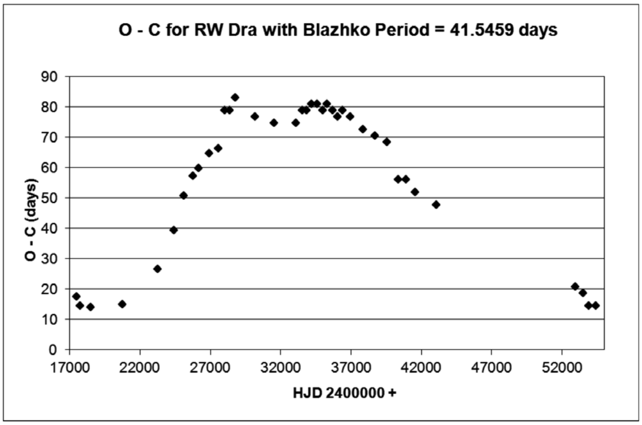

FIG. 5

The O – C diagram for 104 years of Blazhko periods. The period used is that which best fits the whole data set to give similar O – C values at both the start and end of the data.

The close agreement of those two values, 1·8 and 1·7, seems unlikely to be a coincidence. Thus two different methods of analysing the data lead to the conclusion that there is a correlation between the variations in the pulsation and Blazhko variations. A further figure has been prepared to allow comparison of the two variations. Each set of O – C values has been normalized with respect to its own period and then the normalized Blazhko values have been inverted and plotted on top of the normalized pulsation values; Fig. 6 shows the result.

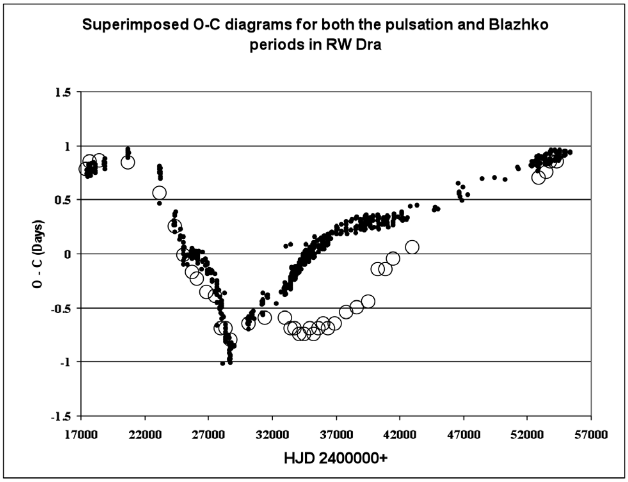

FIG. 6

The normalized O – C values for the pulsation period are shown as filled dots. The open circles are the normalized O – C values for the Blazhko variations and are plotted inverted with respect to the pulsation O – C values.

Considering the whole 104 years of data first, a correlation between the two variations clearly exists. Both the normalized amplitudes and the times when changes of gradient occur seem to be linked. However, there are two epochs over which the anti-phase behaviour breaks down and becomes an in-phase variation. The first of them, lasting about seven years, HJD +25 000 to 27 500, occurred during a time of rapid changes in the pulsation period. The exact duration of the second one is unknown as a lack of data between 1976 and 2000, HJD +43 000 to 52 000, means that no O – C values can be determined for the Blazhko period between those dates. However, it can be seen that the duration of this in-phase behaviour is at least 11 000 days. Perhaps even more remarkably, it does not seem to matter whether the variations are in phase or in anti-phase; the normalized amplitudes appear to remain comparable. This behaviour and its implication for models to explain the origins of the Blazhko effect will be discussed below.

Summary and discussion

If the only available data had been those from about 1940 to 2010, as shown on the rising part of Fig. 1, then this star would probably have been considered to have a pulsation period of about 0·4429385 days, which varied over a time-scale of about 40 years. It is possible that the slow change in period might have been considered to be periodic, possibly as a result of light-travel time in a binary system, or even have been interpreted as signs of stellar evolution. The variations in the earlier data, which are more clearly seen in Fig. 2, mean that any such interpretation is untenable. The large period change between 1938 and 1941 is the first indication that some complications are present. That, in combination with the other rapid changes in period at about 1927 and 1934, and the general rises and falls in the O – C values between 1922 and 1938, means that the star is not likely to be susceptible to a straightforward interpretation. The very earliest data, taken between 1906 and 1915, can only be used as a constraint on the presence of any other rapid period changes during that interval. At its most extreme it seems that the pulsation period can change by nearly 0·1% over a few hundred pulsation cycles, or less. Although this is not enough to limit the use of such stars as distance indicators, using the period–luminosity relationship, the variability is likely to prove challenging for models of pulsating stellar atmospheres. The large amount of activity in the O – C values in Fig. 2 between 1922 and 1938 has been followed by a further 70 years of relative stability. Thus, considering only the pulsation-period changes, we have at least three types of behaviour over the last 104 years: (*i*) there have been three, or more, incidences of very rapid pulsation-period change; (*ii*) the O – C values from about 1920 to 1938 show not only the three rapid pulsation-period changes but also variations in the pulsation period on a time-scale of about 11 years; and (*iii*) the rapid period changes in (*ii*) can suddenly stop and be replaced by slow pulsation-period changes taking decades with no short-term changes

Throughout all this activity the period of the Blazhko variations has remained constant to within about 2%. The fact that the Blazhko period at the time of Blazhko's discovery (early 20th Century) has the same value as all the data from about 2002 until 2010 suggests that the larger, and varying, Blazhko period and the increased pulsation O – C activity are somehow connected. However, because that was a 'one off' event, we cannot be sure whether the two activities are correlated or whether it was a coincidence. In addition we have the changing shape and amplitude of the Blazhko effect as shown in Fig. 4. Note that the two lower sub-frames both relate to data taken during the relative stability of the pulsation period, post 1941. Their amplitudes and their shapes are different. The larger amplitude in the upper sub-frame was present during some of the most rapid and chaotic changes of pulsation period and yet it is still possible to obtain a 'clean' Blazhko phase diagram!

The comparable behaviour between the pulsation and Blazhko variations, as shown in Fig. 6, has implications for models for the origin of the Blazhko effect. Some of these models require there to be a second variation with a period similar to the pulsation period. The suggestion is that a beat will exist between these two closely spaced frequencies which produces the longer-period, smaller-frequency, Blazhko variations. If that were the case then it would naturally explain why variations in the pulsation period should be closely followed by variations in the Blazhko period as seen in Fig. 6. From the values obtained from this analysis for both the mean pulsation and mean Blazhko periods it is possible to calculate what that frequency would be. The mean pulsation periods and mean Blazhko periods for both the earlier and later parts of the data, as described above, were used to derive what value a hypothetical second period would be. For the earlier data the period would be 0·4475584 days and for the later data it would be 0·4477121 days. These periods differ by approximately 3×10^{-4} of the mean period. When the methodology for determining the long-term pulsation and Blazhko periods is considered this difference is negligible.

There is no certainty that the hypothetical second frequency is constant, that it is a single frequency, or that the way in which it interacts with the observed pulsation period remains constant, but at least for RW Dra the hypothesis of a beat origin for the Blazhko effect is tenable. The problem with this interpretation is the change in behaviour between anti-phase and in-phase variations. Whether in anti-phase or in-phase, the amplitudes are comparable, and over the whole 104 years the indications are that the direction of the variations is also coupled, although the sign of this coupling can change between the two states of in-phase or anti-phase. Perhaps the important questions in this context are (*i*) if the second frequency exists, why does its effect on the Blazhko period change, and (*ii*) what physical form does it take in the star (perhaps non-radial variations) that means that it is not as observable as the RR Lyr-type pulsation variations but can only be detected by its production of the Blazhko variations?

A summary of the variations

(*i*) The pulsation period has changed three times in about 12 years by between about one and eleven parts in 10^4 of the mean pulsation period, taking about 500 pulsation cycles, or less, to complete the change, and has then become comparatively stable for 70 years.

(*ii*) The shape of the Blazhko phase diagram can change without any clear correlation with pulsation-period-change activity, but the larger amplitude of the Blazhko variations is possibly associated with increased pulsation-period variations.

(*iii*) The Blazhko period has remained constant to within about 2% over the last 104 years, and the variations in this period are correlated with the changes in the pulsation period, but the nature of the correlation changes.

(*iv*) Any attempt to explain the origins of the Blazhko effect by rotation of a magnetically-active star would have to encompass the possible correlation of increased Blazhko period, but not the shape of the Blazhko phase diagram, with pulsation-period activity.

(*v*) If the origins of the Blazhko effect involve the combination of both radial and non-radial modes of pulsation then some means has to be found to explain the relative stability of the Blazhko variations over times when the pulsation variations are in a state of rapid change or are relatively stable.

Acknowledgements

It is a pleasure to thank an anonymous referee who introduced me to the book by H. A. Smith containing the details of the work of V. P. Tsesevich. The remaining acknowledgements are the same as in previous papers in this series.

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(Note that the GEOS data base is available in two forms, hence the two references, 3 & 4.)

REVIEWS

My Life on Mars: The Beagle 2 Diaries, by C. Pillinger (The British Interplanetary Soeciety, London), 2010. Pp. 357, 24 × 16·5 cm. Price £20 (hardbound; ISBN 978 0 950 65973 2).

"Richard, there's a Professor Pillinger for you on the phone". That was my first (and very unexpected) introduction to Colin back in 2002, on one of his purely ground-based missions to gather historical data about Mars observations, all part of the marvellous *Beagle II* project to search for life on Mars. Later I had some anxious moments when I reported to him the BAA Mars Section's daily records of the movements of a large Martian dust storm in 2003 December, which approached but never quite reached the craft's intended landing site in Isidis Planum. Fellow Martians shared his grief when *Beagle* failed to call home on Christmas morning.

I don't normally like this sort of fly-on-the-wall book, but this one proved to be the exception. With a disarming honesty, Colin Pillinger (pictured on the front cover in a spacesuit) shares his life story with us, from Bristol schoolboy to research student *via* dairy farmer to Fellow of the Royal Society, naturally reserving the greater part of the book for the complex and somewhat tortuous history of the *Beagle II* mission. Colin involved a wide range of people in the project, from artist Damian Hirst to the pop group Blur (who recorded the craft's calling signal which would have been relayed from the Martian surface). The vital rôle played by Colin's immediate family receives appropriate credit.

In this engaging autobiography, Colin describes himself as a risk-taker, and certainly *Beagle II* (originally designed on the back of a beer mat) was always going to involve a fair degree of risk. There were of course all sorts of technical problems to sort out, from vacuum-sealing to airbags. Colin had to solve many of them himself, spending much of his time at the wheel of his car driving between suppliers, sponsors, meetings, and lectures. The biggest struggle seems to have been against certain characters at ESA, whom he calls 'the really bad guys', who were less than keen for *Beagle* to hitch a ride on *Mars Express*. Mars has a habit of being unkind to spacecraft, and it almost seems that each nation is expected to have several attempts at landing before being allowed to succeed. On this occasion it was Great Britain's turn to be disappointed. But, as Colin eloquently writes, many lessons were learnt, and *Beagle II* raised public awareness of science to enjoy a tremendous public following in the UK. It is greatly to be regretted that 2004 did not mark the start of a series of further attempts with more of the planned *Beagle* missions.

There are many priceless quotations and I cannot resist this one, apparently from the Prime Minister's office concerning a visit by the Premier to the Open University in 2005: "The Prime Minister can't be seen in a picture with Colin Pillinger because he was a failure." Always conscious of grammar, Pillinger adds: "Who, me or Tony Blair?" Colin's book will evoke the full range of emotions. You will share his many frustrations as well as those precious moments when key battles were won, and in the end your heart will go out to the supermarket-trolley-sized little spacecraft as you wonder "if only …".

The book was recently launched, and quite literally so: four copies were blasted off in a rocket to a height of 2000 feet, recovered, and then signed by the author. (And you can watch it on youtube.com!) Pillinger understandably says that the book is one of the best reasons why Britain should have its own Space Agency and run its own projects. If you're not British then perhaps you may begin to understand better our idiosyncrasies as a nation if you read this story of *Beagle II* and its very human creator. — RICHARD MCKIM.

Apocalypse When? Calculating How Long the Human Race Will Survive, by W. Wells (Springer, Heidelberg/Praxis Publishing Ltd., Chichester), 2009. Pp. 212, 23·5 × 15 cm. Price £19·99 (paperback; ISBN 978 0 387 09836 4).

About two years ago, I reviewed in these pages (**129**, 157, 2009) an alarming and thought-provoking book that discussed the question of whether human civilization might survive for about as long as it has existed already. It concluded that leaving Earth was not a practicable solution to this problem, that limiting the total population to less than 11 billion was necessary (but not sufficient!), and that international cooperation towards common goals was essential. However, although the authors said that an equilibrium sustainable society was in principle possible if we could make the very difficult transition from our current model of ever-increasing growth, they gave no quantitative prediction for the chances of survival until we attained such a society. Now Willard Wells has tackled essentially that problem, using a remarkably simple model of survivability.

The starting point is to try to find a survival predictor for something for which we know nothing about its durability, and he uses a general formula previously derived by J. Richard Gott III in a paper in *Nature* in 1993, although he first makes it plausible by plotting data for the longevity of business firms and fitting an empirical curve to the data; this curve has the same functional form as Gott's predictor, and variations of this form recur throughout the book. Statistical data from business firms and stage productions are used to test various models based on the probability theory that is the core of the book (so really the book should be critically reviewed by a mathematician specializing in probability). After introducing the reader to key ideas in probability and statistics, Wells starts to develop ideas of probability based first on random-hazard rates (thinking about the odds of being hit by an asteroid, for example), and then ideas based on our own history of survival as a species, and as a civilization. Finally, he introduces an approach using Bayesian theory. Despite the quite technical arguments, Wells manages to make the main text of the book reasonably accessible to the general reader, putting the more mathematical arguments in a long series of appendices (although I felt that genuine mathematicians might feel that the arguments there were insufficiently rigorous, and contained some poorly justified — although not necessarily incorrect — assumptions). I could find no obvious flaws in his arguments, although I did not check every step, and I am certainly no expert in probability theory.

The book is well organized, and is written in an easy style, with some flashes of humour (although innumerate readers may find even the simple version of the mathematics a little dense in places). The crunch comes in Chapter 4, where he actually estimates the probability of the survival of our species, and of civilization, concluding that the half-life of civilization is 8·6 billion people-centuries — that is, about a century if the population stabilizes at 8·6 billion, or less if it does not. This is not a comfortable result, but chimes with Martin Rees's chilling book title *Our Final Century*. On the other hand, he predicts that there is a 70% probability of our *species* surviving — but that a cataclysm that destroys civilization may be necessary for that to be the case. In the final chapter, he reviews ways in which this cataclysm might occur, and suggests that the best way out is to develop sentient androids who could be sent out, perhaps with frozen human embryos in their care, to seek for a new home.

This is a disturbing and controversial book. Even if his detailed numbers may be challenged (and I think someone else should certainly look in detail at all his assumptions), we know well enough that he is discussing a very real and important problem. Unfortunately, even a threat of extinction in about a century is probably too long a timescale for politicians to pay much attention — as Wells points out, we are all rather too prone to concentrate on the immediate threat and not worry about the longer-term ones. But our grandchildren may reap the consequences. — ROBERT CONNON SMITH.

Discovering the Expanding Universe, by H. Nussbaumer & L. Bieri (Cambridge University Press), 2010. Pp. 226, 25·5 × 18 cm. Price £34/$57 (hardbound; ISBN 978 0 521 51484 2).

Three key events in the development of 20th-Century cosmology were the recognition of other galaxies, the demonstration that the Universe is expanding, and discovery of microwave background radiation left from a hot, dense, early phase. Marcia Bartusiak has addressed the first in her prize-winning *The Day We Found the Universe*, and P. J. E. Peebles *et al*. the third in *Discovering the Big Bang*. Nussbaumer & Bieri have now filled in the middle, with *Discovering the Expanding Universe*. The three are very different in style. Bartusiak writes for general readers (though she has found some little-known historical gems, like the two other astronomers who briefly confirmed van Maanen's report of spiral rotation in the plane of the sky). Peebles, Page & Partridge have incorporated extensive reminiscences by many of those involved in the discovery and its interpretation. And Nussbaumer & Bieri are writing serious history of science, making extensive use of original sources, particularly papers published between 1900 and 1950 and the Einstein and Hubble archives, though they begin with Ptolemy, al-Sufi, Sacrobasco, and Cusanus* and end with the concordance model. A 21-page mathematical appendix contains enough General Relativity to get on with and runs in parallel with the main text to present Einstein, de Sitter, Weyl, Friedmann, Lemaître, Robertson, and Tolman universes and the FLRW metric in its modern form.

Standard introductory texts often tell us that Hubble discovered the expansion of the Universe in 1929 with, perhaps, a little help from Slipher (Vesto Melvin, who measured the first redshifts), Humason (Milton Lassell, who measured the later ones), and Einstein (who needs no introduction).

In reality, and this is perhaps the most important point to be carried away from the book: the discovery spread over a decade or so and required theoretical input from de Sitter, Friedmann, Lemaître, Eddington, Tolman, and Robertson, and observations from Curtis, Shapley, and others. The authors, and Sandage in a preface, make the point that Hubble was never fully persuaded that there was real expansion. His one theoretical paper (Hubble & Tolman, 1935) dealt with galaxy counts and the redshift–magnitude relation as tests of the nature of the redshift–distance relation. The available counts and *z–m* data were, of course, almost entirely Hubble's own work, and he thought expansion was not a good fit to them. Nussbaumer & Bieri do not cite that paper (*ApJ*, **82**, 302) among their 13 Hubble references (Lemaître wins with 18).

The authors are, in general, admirably precise in describing what various people thought at various times; for instance, a 1923 postcard from Einstein to Weyl, saying "*Wenn schon keine quasistatische Welt, dann fort mit dem kosmologischen Glied*." (if there is no quasi-static world, then away with the cosmological term) and a letter and a publication from 1931 describing time-variable radius of the Universe (*Weltradius*) as "*wirklich unvergleichlich befriedigender*" (incomparably more satisfying). This is rather different from the "greatest blunder" phrase found in as many different places as the feathers from the burst pillow of Greek myth.

The text ends with an imaginary scene of Einstein, de Sitter, Eddington, Friedmann, Lemaître, and Hubble together drinking Duoro Valley port of 1927 and reflecting on developments from 1930 to 2010, largely with great pleasure.

But the most touching part of the book is the preface by Allan Sandage, who worked with Hubble and Hoyle (separately!), knew Baade, Heckmann, McVittie, Robertson, and Humason, and was told by Lemaître that he should change fields away from cosmology if he couldn't visualize curved space. If 'Uncle Allan' knew he was dying when he wrote, you would never guess it from the text. He is the same warm curmudgeon who, at the 1967 Prague IAU, was "in awe of a mistakenly read badge, rather than the real thing" (meaning Prof. W. Mattig of Freiburg, who first wrote down analytical solutions for the relation between redshift and bolometric luminosity in 1958–59 and remained one of Sandage's heroes). Happily they spent a day together in Basel in the 1990s. Mattig was still alive as the book went to press, and Sandage remained to the end persuaded that the right way to think about the Universe was with galaxies about the size of footballs and that somewhere there is a Higher Authority who wants us to understand. — VIRGINIA TRIMBLE.

*Cusanus = Nikolaus von Kues = Nicolas of Cusa, 1401–1464; Sacrobasco = John of Holywood, c. 1195–1256; al-Sufi = Abu al-Husayn Abd al-Rahman ibn Umar al-Sufi (ar-Rahman as-Sufi in the volume being reviewed), 903–986. We don't suppose that was Ptolemy's real name either, and he hasn't any authenticated dates.

The Power of Stars: How Celestial Observations Have Shaped Civilization, by Bryan E. Penprase (Springer, Heidelberg), 2011. Pp. 360, 26 × 18 cm. Price £35·99/$39·95/€39·95 (hardbound; ISBN 978 1 4419 6802 9).

Over in California, at Pomona College, a private liberal-arts college in Claremont, Bryan Penprase, the Frank P. Brackett Professor of Physics and Astronomy, gives a course on 'Archaeoastronomy and world cosmology'. His lecture notes have been polished up to produce this textbook. The question behind his lecture course is very simple: "How, throughout history, have humans responded to the sky?" Well, there are three answers.

One fits in with today's 'politically correct' view of science and endeavour; throughout history we have looked at the sky, and the wonderful objects it contains, and we have tried to work out what these bodies are, how they move, where they get their physical and chemical characteristics from, where they have come from, and what is their fate. Humanity is curious and wants to understand.

The second answer is equally palatable. We have found the sky useful. Since the dawn of civilization, the bodies in the heavens have been used as compasses, clocks, and calendar markers. We used them to measure the passage of time, regulate our prayers and religious observance, and to aid our hunting, fishing, farming, gathering, travelling, and weather forecasting. The more civilized we become the more we seem to use the sky.

The third answer is more embarrassing and is tied up with the world of astrology. We have used the Sun, Moon, planets, and stars to foretell the future. The fact that today most of us do not believe that this works is neither here nor there. In the past astrology was the spur to observatory building, and the reason astronomers were employed and records kept. There was a strong belief that the bodies in the cosmos affected our lives. And *vice versa*, we also believed that we could affect them. If you wanted to fuel the Sun, and move it back into the northern sky to repeat another spring and summer, many, like the Aztecs, believed that fresh blood had to be provided and sacrifices needed to be made. Life is hard and decisions are difficult to make and if we thought the sky could help we happily interpreted celestial positionings to aid our decision-making.

The problem with archaeoastronomy is that you are trying to work out the percentiles of human effort that was expounded on answering each of the three questions posed above, simply by measuring up physical ruins, studying indistinct and poorly preserved artefacts, reading often-incomplete books recording past mythologies, practices, and cartographies, and enquiring into the views of the anthropologically relevant. Archaeoastronomy is not an easy subject, and there is little consensus.

I greatly enjoyed reading Penprase's book. It was thought-provoking, incisive, world-encompassing, beautifully illustrated, and reasonably sceptical. Did I agree with all his points of view? Certainly not, but that is simply the nature of the subject. — DAVID W. HUGHES.

Galileo and 400 Years of Telescopic Astronomy, by Peter Grego & David Mannion (Springer, Heidelberg), 2010. Pp. 300, 23·5 × 15·5 cm. Price £26·99/$29·95/€29·95 (paperback; ISBN 978 1 4419 5570 8).

This charming book is an easily accessible romp through the history of astronomy, concentrating on what is observable. It starts with the naked-eye results of the Greeks and then focusses, celestial object by celestial object, on telescopic observation since 1609, the year of the first recorded astronomical use of the refracting telescope.

What distinguishes this book from the mundane is the inclusion of a host of small observing projects which will help the observationally minded to appreciate even more the historical struggles of the pioneers. Do you know how Galileo worked out that the typical lunar mountain was 6·5 km high? How would you organize a group experiment to measure the size of the Earth, or a group solar-eclipse watch to estimate the Earth–Moon distance? Can you easily list the top ten challenging Messier objects? If the answer is 'no', then this book will be a great help.

Galileo Galilei is clearly the book's hero. His technical ability to make and improve his own instruments is much admired. As is Galileo's obvious joy in pushing his instruments to the limit, observing as much detail as is possible, writing up his results succinctly, and ensuring that they are published as widely and as speedily as possible. Grego and Mannion are clearly great fans of the Italian 'father of modern science'. Their delightful book strongly encourages us all to try and follow in his footsteps. — CAROLE STOTT.

Highlights of Astronomy as Presented at the XXVII General Assembly, 2009, edited by I. F. Corbett (Cambridge University Press), 2010. Pp. 841, 25 × 18 cm. Price £70 (hardbound; ISBN 978 1 107 00533 4).

The International Astronomical Union's triennial General Assemblies are complex meetings with many parallel sessions. Much of the science is published in the individual volumes reporting the six symposia, and the meetings of commissions (which combine some science with their business) are published in the B volume of the transactions (already reviewed here (**130**, 385, 2010) for the 2009 GA). However, those meetings by no means exhaust all the science that is discussed, and for many years now a *Highlights* volume has been used to publish the Invited Discourses (ID), Joint Discussions (JD), and other Special Sessions (SpS). The current volume is number 15 in the series, and covers four IDs, 16 JDs, and ten SpSs, as well as the Gruber Cosmology Prize Lecture (on measuring the Hubble constant with the *HST*).

It is impossible to give a full account of a volume of this size (and weight: 1·6 kg), so I will just summarize the topics covered. Being 2009, the IYA was the subject of one of the Special Sessions, and Pacini's ID took Galileo as its subject. The other IDs covered water in planets, evolution of structure in the Universe, and low-luminosity stars. The JDs covered galaxy topics (dark matter in early-type galaxies; diffuse light in galaxy clusters; hot interstellar matter (ISM) in ellipticals; the first galaxies; the ISM of galaxies in the far IR and sub-mm); stars (neutron-star timing; Ap and related stars; cool stellar atmospheres; helio- and astero-seismology; η Carinae; and massive stars); modelling the Milky Way; time and astronomy; outflows and accretion; the three-dimensional heliosphere; magnetic fields in diffuse media; and whether fundamental constants are varying with time. As well as IYA2009, the SpSs covered IR and sub-mm spectroscopy as a tool for studying stellar evolution; astronomy in Antarctica; astronomy education; accelerating the rate of astronomical discovery; planetary systems as potential sites for life; young stars, brown dwarfs, and protoplanetary discs; the Galactic Plane; marking the 400th anniversary of Kepler's *Astronomia Nova*; and next-generation large astronomical facilities.

Not all the sessions are fully reported: Simon White's ID on the evolution of structure is represented just by an abstract, while JD 12 on the first galaxies has no report at all, and nor has SpS 10 on the next-generation large astronomical facilities. The editors of a few of the reports wrote a coherent summary of their sub-meeting, but most reports consist of large numbers of short papers or abstracts, and in some cases abstracts of posters. The most amusing acknowledgement (p. 120) was to "my 4-year old son, who carefully checked that there were enough uppercase "A's" in the text"! There is a useful author index, although it was unhelpful to divide it by the kind of meeting. There is something here for everyone, although you will probably want to have it in your library rather than on your own shelf. — ROBERT CONNON SMITH.

Science Education and Outreach: Forging a Path to the Future (ASP Conference Series, Vol. 431), edited by J. Barnes, D. A. Smith, M. G. Gibbs & J. G. Manning (Astronomical Society of the Pacific, San Francisco), 2010. Pp. 553, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 742 1).

The landscape of astronomical outreach is rapidly changing. It is now not enough simply to educate — today's audiences have increased expectations of interaction and entertainment, alongside a presence in the new social media. Many innovative outreach projects are tackling these challenges in an exciting way, and the proceedings from this conference provide a snapshot of activities, disseminating practical suggestions, information, and resources. It's particularly useful because outreach practitioners rarely write up their initiatives for the wider community once they are complete. Most projects are short-term, with effort focussed on designing, carrying out, and evaluating an activity. With funding bodies always in search of new (rather than continuing) projects to support, the field moves on with little opportunity for reflection or publication.

There is a surprising amount that can be readily harvested from reading through this book. I needed a notepad to hand to scribble down the stream of ideas generated as I browsed, and I was continually distracted into chasing through the plethora of web resources provided in the articles. We've all had ideas without the opportunity to try them in practice, and here is a chance to learn from how other people have used similar thoughts. These contributions have a welcome and practical emphasis on sharing outcomes — whether it's the web resources generated, or simply passing on advice about 'lessons learnt'.

I particularly enjoyed a contribution offering suggestions on how (and indeed whether) to incorporate the wide range of new media available into an outreach programme, and another demonstrating the process of the design, development, and evaluation of an 'ideal' exhibit. There were useful suggestions of new ways to employ software I already use for teaching, such as STELLARIUM or the WORLDWIDE TELESCOPE, and results on the effective deployment of astronomical imagery. Of course, many of the projects reported will not be directly transferable to any one person's own situation — but that is a reflection of the wide breadth of material showcased. Another reader would no doubt extract a completely different subset of suggestions and ideas from the same volume: perhaps how to develop an effective mentoring programme; introduce scientific understanding through writing assignments; the use of 'citizen science' projects; or how to piggyback an outreach programme onto large community events in the search for a more varied and non-traditional audience. This book will be a useful resource for both educators and astronomers involved with generating outreach initiatives and activities as a significant part of their work. — CAROLIN CRAWFORD.

Co-Evolution of Central Black Holes and Galaxies (lAU Symposium 267), edited by B. M. Peterson, R. S. Somerville & T. Storchi-Bergman (Cambridge University Press), 2010. Pp. 469, 25 × 18 cm. Price £70/$125 (hardbound; ISBN 978 0 521 76502 2).

Black holes at the centres of normal galaxies were 'a discovery' in 1994, largely thanks to highly resolved images and spectroscopy from *HST*. By 1998, there were enough of them for Magorrian to plot his diagram, showing a nearly linear correlation between black-hole mass and the mass of stars in the galactic bulges (the whole galaxy for ellipticals, some fraction for spirals). The next year, Ferrarese and Merritt pointed out that the correlation was tighter if you used central velocity dispersion rather than mass of bulge stars for the horizontal axis.

Such correlations inevitably invite chicken-*versus*-egg hypotheses and what 'we' called "the potato salad of co-development" (*PASP*, **111**, 410). The title of this conference and its proceedings indicates that the organizers had voted for co-development, though the keynote speaker, T. M. Heckman, briefly mentioned a few data sets favouring black holes first or stars first. The field has moved on so quickly that neither he nor other review speakers mentioned those first crucial papers, though Laura Ferrarese was one of the eight women on the 17-person SOC.

The majority view of the invited speakers was that feedback from the AGN is a key process. Jets heat and remove gas until both accretion and star formation cease, leaving the galaxy 'red and dead' and with the characteristic mass ratio (near 0·005). There are variations with galaxy type and, especially, with redshift, in the direction of downsizing. That is, recent AGNs have smaller central black holes and lower luminosities than those at a z of 2 to 3.

The volume is dedicated to Andrew S. Wilson who died in 2008, and whose work on multi-wavelength observations of AGN is part of the evidence for the co-evolution viewpoint. In addition, one of the three editors, who were also the SOC chairs (as per IAU custom), did postdoctoral work with him at the University of Maryland in the early 1990s. At least one of his papers is cited in at least one of the reviews.

If co-evolution dominates what we see, the next question one wants to ask is how do mergers, generally advertized as important in the evolution of massive galaxies, affect the picture. Do the pieces that merge bring their stars and black holes with them, already properly in proportion, and the products hang on to everything? Or is more star formation and more black-hole accretion and growth triggered, or what? At this point, one misses a standard index. The editors have provided only an index of authors, not topics or astronomical objects. One poster noted that the black-hole-merger product will recoil because of asymmetric emission of gravitational radiation, but says nothing about how this might affect the key correlation. A longer contribution on merger-induced quasars concludes that not all the data sets to which the speaker had access can be taken at face value, or one ends up with contractions like a final black-hole mass smaller than the initial one,

Also absent from the volume are (*a*) a list of participants (mildly annoying), (*b*) a conference photo (neutral), and (*c*) photos of participants pigging out at various social events (just as well).

Symposium 267 took place as part of the IAU General Assembly in Rio de Janeiro in 2009 August (and the editors commendably got the volume into the hands of the publishers in 2010 February). A marker of this environment is that there are 109 poster contributions, many of them of only slight relevance to the stated topic. The problem is that everybody who comes to an IAU General

Assembly wants to be an author of something, and the range of symposia, joint discussions, and special sessions does not necessarily include every possible topic, with extra-galactic astronomy typically somewhat under-represented.

In summary, the symposium probably served its participants well. Readers trying to learn about the subject might have been happier with more pages given to (most of) the review and invited speakers and some culling of less-relevant posters. — VIRGINIA TRIMBLE.

The High Energy Universe: Ultra-High Energy Events in Astrophysics and Cosmology, by P. Mészáros (Cambridge University Press), 2010. Pp. 209, 25 × 18 cm. Price £35/$59 (hardbound; ISBN 978 0 521 51700 3).

An undergraduate textbook on cosmology, particle physics, high-energy astrophysics, and gravitational physics. Wow, that sounds a bit heavy, and all that in one book with only 209 pages, including the references, glossary, short table of astrophysical units, and an index. Mészáros's book is pitched at undergraduate level and is well worth the read but it is not a conventional textbook. It is not a traditional research monograph either, because the scope is too broad and it lacks rigour and depth. Nor is it a popular science book from the *A Brief History of Time* stable. The book leans towards the Dirk Gently fundamental-interconnectedness-of-all-things approach and provides a convincing overview of current research in high-energy phenomena and astronomy. It is just an overview. Readers who want to get to grips with the full details will have to ferret around, starting from the list of references, but Mészáros does a good job of describing the basics of most, if not all, of the topics, phenomena, objects, successes, and unsolved problems associated with the field.

If you need to be convinced that astronomy and astrophysics are more than just stars then read on. "Stars are just the lumpen proletariat of the Universe ..." and "Stars (are) the Universe's worker bees" but "Brilliant as they are, stars pale into insignificance compared to what they become after they can no longer support nuclear fusion." Even if you are already sold on extreme events from the Big Bang to gamma-ray bursts and massive-black-hole mergers, you will enjoy this rather personal account. Following a succinct introduction about the breakdown of the Universe into dark energy, dark matter, and the seemingly insignificant mere 4% of baryonic matter, the second chapter considers the "nuts and bolts of the Universe" and the ingredients of the "Cosmic soup", quarks, leptons, bosons, atoms ... and the four fundamental forces, "three easy pieces and a harder one". The remaining chapters describe the ingredients of the astronomy soup, which are cosmology, X-ray astronomy, gamma-ray astronomy, gravitational-wave astronomy, cosmic-ray and neutrino astronomy, and particle physics at the *LHC* and elsewhere. Mészáros is clearly a fan of the multi-wavelength approach to astrophysical research.

The connecting theme throughout is astrophysical events, or objects, which involve the creation of high-energy particles or the release of huge amounts of energy. We embark on a whirlwind tour of the primordial fireball, extreme inflation, stars, active galaxies, white dwarfs, neutron stars, millisecond pulsars, magnetars, stellar black holes, micro-quasars, gamma-ray bursts, galactic GeV–TeV sources, stellar binary and galaxy gravitational-wave sources, ultra-high-energy cosmic rays, neutrinos from stars, supernovae and the atmosphere, WIMPs, MACHOs, axions, and dark energy. If your favourite object is not listed here don't worry — it's most likely in the book somewhere. Despite the high density and breadth of all this, there is sufficient depth and description of the underlying physics and physical principles to guide the reader and hold it all together.

Although Péter Mészáros is probably regarded as a theoretical astrophysicist, in the book he considers theory, observed phenomena, and the instrumentation in equal measure and amply demonstrates that science is a close collaboration between all three. Whatever branch of astronomy or physics you are engaged in, at whatever level and whether you are playing with spanners, software packages, or mathematics on a blackboard, you are likely to find something new and stimulating in *The High Energy Universe*. — DICK WILLINGALE.

Exploring the X-Ray Universe, 2nd Edition, by F. D. Seward & P. A. Charles (Cambridge University Press), 2010. Pp. 358, 25 × 19 cm. Price £40/$65 (hardbound; ISBN 978 0 521 88483 9).

I like this book. As with the much-used first edition, it summarizes the state of the art in X-ray astronomy through clear text and lavish artwork. Today X-ray astronomy is so routine that it is worth looking back, as in the first chapter, to remember how far it has come in a fairly short space of time. Fred Seward has, almost literally, seen it all happen in X-ray astronomy and Phil Charles has likewise been in the game long enough to know how to present the subject. They adopted an apt title for the book as, following a few chapters on how X-rays are produced and then interact with matter, the bulk of the text concentrates on the wonders to be found in the X-ray Universe.

This is really an observers' book. Rather than give a long, rigorous, mathematical account of physical processes, the authors instead illustrate topics with an array of figures and diagrams which capture the essence of the subject without getting the reader lost in minute detail. I find this a very useful way to start, albeit knowing I will then, sadly, have to look up a formula or ten elsewhere. The friendly, clear style makes it an ideal book for aspiring graduate students or indeed anyone who wants a solid grounding in what we know and how we came to know it. The authors make use of their wide experience in both instrumentation and observations to balance knowledge against inference, which is always a challenge in the world of chi-squared fits. Fortunately, for those of us brought up on 'proper spectra', the increasing use of gratings in X-ray astronomy is reassuring compared to the black art of response functions and the like. The new edition takes into account the new instrumentation which has truly revolutionized the field.

Towards the end of the book, I spent some time just gazing at the colour plates. These should convince you that it's not only *HST* that makes pretty pictures. The price of the hardback edition is not low, but it remains good value. As I said, I like this book and highly recommend it. — PAUL O'BRIEN.

AGN Feedback in Galaxy Formation, edited by V. Antonuccio-Delogu & J. Silk (Cambridge University Press), 2010. Pp. 201, 25 × 17·5 cm. Price £65/$105 (hardbound; ISBN 978 0 521 19254 5).

In 2008 May, a number of the world's experts on active galactic nuclei (AGN) gathered together for a workshop to discuss the influence of AGN on galaxy formation (so-called 'AGN feedback'). This book draws together the proceedings of that workshop with contributions from many of those who attended. It covers a wide range of aspects of AGN feedback: from the rôle of jets in intermittent, weak, radio AGN to explosive, powerful, quasar events; from outflows near the black hole, to effects on galactic scales, and the influence of AGN within clusters of galaxies; from the nearby Universe to high redshift; and from observation to numerical simulations and modelling, and theoretical interpretations. It provides a well-rounded summary of the recent status of this fast-moving research field, as well as a discussion of some of the most important outstanding problems. — PHILIP BEST.

Observational Cosmology, by S. Serjeant (Cambridge University Press), 2010. Pp. 324, 26·5 × 21 cm. Price £75/$130 (hardbound; ISBN 978 0 521 19231 6), £35/$60 (paperback; ISBN 978 0 521 15715 5).

Stephen Serjeant is a Reader in Cosmology at the Open University. Aren't we all readers? Well, yes (and sometimes writers), but the word is also a British academic title for someone who has tenure and is likely to be leading various research teams (items in extragalactic astronomy and active galaxies in Serjeant's case), but who does not have the responsibility or, we fear, the salary, of a professor or director in charge of a whole department or institute. And the Open University is a relatively new one, whose goal is to allow students in somewhat non-standard positions to pursue studies, largely on their own, toward university degrees.

Observational Cosmology is, therefore, intended to be entirely self-contained for a student who knows some physics and the meanings of some astronomical terms. Thus, in particular, the 68 exercises come with fully-worked solutions at the back of the book. I think a text of this sort might benefit from having, in addition, a comparable number of exercises for whose solutions the student would have to look somewhere else, so that they could function as practice exams.

The volume is quite reasonably up to date (*Herschel* got launched between pages 9 and 164), and while the glossary seemingly promised on p. 318 doesn't exist, the pages listed in bold face in the index do mostly have definitions of the terms indicated. Virtually all the items you might think of looking for are there — dark matter and energy, the usual effects (Sachs–Wolfe, Sunyaev–Zel'dovich, Rees–Sciama, Tully–Fisher, Butcher–Oemler), equations for various possible cosmological models, inflation, gravitational lensing, the Lyman-alpha forest, baryon acoustic oscillations — called wiggles in the section head — and so forth. Very few people are mentioned and none indexed — except as eponyms and authors of references. Zwicky makes the text (for tired light) but not the index.

The language is, of course, British English, Thus the author writes "different to" where an educated American will normally write "different from" and a folksy one "different than". Only those of you who have read *The Ghost and Mrs. Muir* will know why I'm sensitive to this particular item!

The author indicates that the book is intended to leave its user ready to begin post-graduate studies in cosmology, and I think it may well serve its purpose. Will you want a copy? As a learner, quite possibly (and this includes anyone teaching a subject for the first time); as a teacher, to use as a textbook, I am not quite so sure — it will be your job to make up that second set of 68 exercises, so that students can practice for exams.

You must know at least one joke (generally involving a city slicker and a farmer) of which the punch-line is "well, if I were you, I wouldn't start from here". The author has chosen to start with Olbers' Paradox (which is, of course, a riddle and not new with Olbers), whose relevance to observational cosmology turns out to be much less than was once supposed. But I am not the author (though my mother's maiden name was Farmer). — VIRGINIA TRIMBLE.

Proceedings of the 2009 Snowbird Particle Astrophysics and Cosmology Workshop (SNOWPAC 2009) (ASP Conference Series, Vol. 426), edited by D. B. Kieda & P. Gondolo (Astronomical Society of the Pacific, San Francisco), 2010. Pp. 169, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 732 2).

In the age of instant communication, the rôle of conference proceedings is hazy. Is it to disseminate, to a wider audience, new ideas aired at (or springing from) the meeting? Probably not any longer: the pace of scientific development is so fast that, by the time the leisurely cogs of printing presses have spun, the novelty of any original work is severely diminished. What's more, the cutting-edge is often so sharply specialized that its relevance is almost impossible for the non-expert to judge. Instead, proceedings are now at their best when collating a cohesive selection of review articles, giving informal introductions to a series of topics. Interdisciplinary conferences, where delegates more clearly see the need to step back and present overviews of their field, offer an opportunity for assembling such a volume.

The 'SnowPac' workshop series, organized by the University of Utah, is designed to promote conversation between particle astrophysicists and theoretical, computational, and observational cosmologists. Despite the clear relationships between these four fields, the technical language spoken by researchers is surprisingly different. The SnowPac meetings can therefore legitimately be described as interdisciplinary.

The SnowPac 2009 volume is divided into four sections: on cosmic rays, neutrinos, dark matter, and cosmology, respectively. There is no doubt that it contains some useful review material. The first two sections give helpful insights into specific detector experiments, although the coverage is patchy (there's nothing in the first section, for instance, on the *Pierre Auger Observatory*). The cosmology section takes a somewhat different tack, describing successfully in non-technical language a few of the most exciting frontiers, with some engaging and accessible articles. Weakest is the 'dark matter' section: there is nothing particularly wrong with the articles, but they are relatively specialized and no semblance of a thread draws them together. Write-ups from some of the higher-profile speakers (Lisa Randall and Piero Madau spring out from the list posted on the conference website) are conspicuously absent.

Overall, this book contains some useful material but, through no fault of the contributors, its selection of topics is eclectic. Accordingly the volume never really transcends its curiously outmoded genre. A good textbook, supplemented by Google Scholar's excellent indexing of the arXiv, is a more reliable way to gain an up-to-the-minute overview of the covered fields. — ANDREW PONTZEN.

Classical Measurements in Curved Space-Times, by F. de Felice & D. Bini (Cambridge University Press), 2010. Pp. 309, 25·5 × 18 cm. Price £70/$125 (hardbound; IBSN 978 0 521 88930 8).

This book addresses the subtleties that arise when exploring classical measurements in curved space-time. A precise prescription for relating mathematical objects to physical measurements is a vital part of any physical theory, and such measurements are most elegantly and clearly expressed in curved space–time by using tensor calculus and modern geometrical concepts.

Although the early sections of the book contain a brief review of coordinate-free tensor calculus on manifolds, their brevity is such that they are only suitable for readers who have been exposed to coordinate-free methods and have already taken an advanced course in General Relativity. Some familiarity with exterior differential calculus is desirable, as the language of differential forms is occasionally employed, although it is used sparingly.

Emphasis is placed on the concepts of local and non-local measurements. The space–time curvature is negligible in local measurements, whereas the curvature has to be included if the measurement is non-local. For example, an observer may deduce the spatial velocity of a particle relative to his own rest frame by exchanging light signals with the particle, and the measurement is deemed local if the particle is sufficiently close to the observer. Formulae for local and non-local measurements of, for example, the frequency of a photon and the velocity of a particle are derived by using the world function.

The methods developed in the book are applied extensively to Schwarzschild, Kerr, and gravitational-wave space–times. In particular, the authors address the problem of space navigation; a number of interesting discussions are given about how an observer in a small spacecraft on a circular orbit in Schwarzschild or Kerr space–time may determine his orbital parameters by using measurements taken only inside the spacecraft. This leads to a section on determining the mass and specific angular momentum of a black hole by means of such measurements. Ray-tracing in Kerr space–time and General Relativistic corrections to light rays are also discussed, and the final chapter focusses on the behaviour of gyroscopes and extended bodies in Schwarzschild, Kerr, and gravitational-wave space–times.

In conclusion, this is a wide-reaching book that covers a lot of material in 309 pages and includes 100 exercises to stretch the most able students. However, due to its scope, it is fair to say that it is inappropriate for those who do not have, at the very least, an introduction to General Relativity under their belts and some familiarity with coordinate-free methods on manifolds. Nevertheless, I have no doubt that it will reward the diligent reader. — DAVID BURTON.

An Introduction to Planetary Atmospheres, by Agustín Sánchez-Lavega (CRC Press, Boca Raton, Florida), 2011. Pp. 587, 24 × 16 cm. Price £39·99 (hardbound; ISBN 978 1 4200 6732 3).

First, the reviewer has to come clean about the fact that his own book with nearly the same title appeared earlier this year. Hopefully this leads *Observatory* readers to expect an informed and inspired set of comments rather than sibling rivalry! In fact, the two books are fairly different, that currently under review being more than twice as long and considerably more advanced and research-orientated than the very basic introduction for undergraduates published in January by OUP.

Agustín Sánchez-Lavega is a professor in the Engineering School of the University of the Basque Country in Bilbao; this background shows in a satisfyingly mathematical treatment of most of the topics covered, which are organized by physical process rather than planet-by-planet. They range from orbital mechanics, origins, internal structure, and gravitational and magnetic fields, through radiative transfer in clear and cloudy atmospheres, spectroscopy, composition and chemistry, and finally to three long chapters on atmospheric circulation, covering global motions, waves, and local instabilities, respectively.

Atmospheric dynamics is the discipline that the author has employed to particularly good effect in his research on the analysis of results from planetary missions such as *Cassini* observations of Saturn and Titan and atmospheric remote sounding by *Venus Express*. A feeling for current research is displayed in every chapter, linking the Solar System bodies with atmospheres to aspects of Earth-climate science, and even reaching out to expectations for exoplanet atmospheres. The text is very clear and accurate, in excellent English, well illustrated and tabulated. It includes a large number of (mostly quite challenging) problems, thankfully with answers (although not worked solutions). It is a splendid book that comes highly recommended as a primer for students, and as a reference for researchers as well. — F. W. TAYLOR.

Atlas of the Galilean Satellites, by P. Schenk (Cambridge University Press), 2010. Pp. 394, 28 × 24·5 cm. Price £95/$165 (hardbound; ISBN 978 0 521 86835 8).

This is an extraordinary book. It self-destructs as you read it! It is published by Cambridge University Press and actually *printed* in Cambridge, where the Press must somehow have forgotten how to bind a book. This one, as you turn the pages, so they come loose, and when you have finished you have got a 'hard back' (and front) and a pile of 'loose-leaf' pages! — not at all satisfactory for a book costing £95*.

But now, about the content. In *that* respect the book is excellent. It has systematic pictorial coverage of the four Galilean satellites at 1-km resolution, plus a lot of pictures at higher resolutions, a few of them better than 10 metres. Surprisingly (to the reviewer) the global coverage owes quite a lot to the 1979 *Voyager* passages through the satellite system — a by-product of the need for those spacecraft to obtain 'gravity assistance' from Jupiter to help fling them towards Saturn at increased speed. There would have been no need to appeal to the *Voyager* images if the *Galileo* orbiter had worked as it was supposed to do. Unfortunately its main antenna, furled for five years (three of them awaiting launch by the *Space Shuttle* after it was delayed by the *Challenger* débâcle, and then *Galileo* was two years into its six-year journey to Jupiter before the antenna was intended to unfurl), refused to open. The planned transmission rate of 140 000 bits/s was ten thousand times greater than a secondary non-directional antenna was supposed to achieve, but some cleverness with the orbiter and upgrades on the ground allowed an ultimate data rate 'only' a thousand-fold less than intended. The images were also compressed by on-board software before transmission, and one has to say that it is amazing what was achieved in the face of the failure of the proper antenna — though the pictorial coverage would have been much more amazing yet had everything gone according to plan.

It seems surprising that the book has been compiled by a single author, who (among the ~10 000 people who worked on *Galileo* in the course of the 30-year history of the project) apparently has been largely responsible alone for everything that has needed to be done between the actual reception of the pictures from the spacecraft and their publication in the book under review. The author's preface includes the statement that "image selection, geometric control and registration, mosaic and map formatting, and all other aspects of map production are the sole responsibility of the author.". He has certainly had a big task on his hands!

*It now seems likely that the review copy was defective in an unrepresentative way. After this review was submitted, I found an opportunity anonymously to examine another copy of the book in a shop, and my examination did not destroy it. I then approached the publishers and asked a type of forked question that I have found useful when trying to obtain a replacement of a defective article — whether such books could be expected to come to pieces as soon as they were handled as a matter of course, or whether it was possible that the copy I received might have been defective. They assured me that the latter possibility must be the true case, and as a corollary they found themselves replacing the book. It is fair to report that the replacement copy is just like a normal book — it can be opened and read, and when one has finished looking at it it is still a book.

The book starts with a liberally illustrated introduction of about 50 pages, followed by atlases each running to 50–80 pages of the four satellites, and then appendices that include a glossary and gazetteer. It is as pleasing as it is unexpected to see that the naming of features on the satellites is Latin-based, exactly as in the case of the Moon. The atlases are systematically arranged, each starting with global views and then dealing with fifteen successive areas (two polar, four 90° sectors of each 'temperate zone', and five 72° sectors of the equatorial zone). Within that arrangement the images are systematically numbered in a hierarchical manner, but since there are great differences in both the interests of different areas and the availability and number of pictures in each, it is not as easy as it might be to find an image to which reference may be given on another page, and it would have been useful for page references to be given too — especially if the pages were all numbered, which many are not.

The pictures are accompanied by highly informative captions. It has to be said that they are in general marvellous pictures, which in their abundance (despite its being vastly less than was originally intended) give an extremely good impression of what the various satellites are like; and in contrast to the binding of my copy, the way that they have been printed is excellent — they are beautifully reproduced on high-quality paper throughout. Unobtrusive grids showing latitude and longitude have been added to the pictorial data. An impression that is brought home very forcibly by the atlases of the four Galilean satellites is how altogether-astonishingly *different* they all are from one another. As far as I know, before the era of visiting spacecraft there was no particular reason to suppose that there were great differences between them; if people had thought about them at all, they would probably have expected them all to look pretty much like the Moon. But even if one does not recall the still further differences of other satellites — for example, Titan with its smoggy atmosphere and hydrocarbon rain, Iapetus with its walnut shape and piebald surface, Miranda with its extraordinary construction and cliffs — one cannot but marvel at the diversity and individuality of the bodies and the ingenuity of the Creator in producing so many incredibly different variations on a single theme.

It is only to be expected that if one reads an American book one has to put up with American spelling — but why that should still apply when the book is published by CUP is not so clear. The recently adopted mis-spellings sulfur and sulfate particularly grate on the reviewer. Then there is the clearly mistaken American (and now CUP) habit to put punctuation *inside* inverted commas where it doesn't belong. For example, on pp. 293/4 there are references to an "island", which is all right, and to an "island," and "islands.", which are certainly *not*. Elsewhere, the satellites are called the "four sisters.", Io is called "dry,", and many other examples could be adduced. There are split infinitives and other irritations; a constant one, of which more than 30 examples were noticed, is the abuse of 'due' adverbially in place of 'owing'. There are occasional mistakes in wording, such as reference to the 'shear' number of craters, and 'lead' is repeatedly used as if it were the past participle 'led'. Another class of problems over language — not by any means confined, however, to the work presently under review — is the misappropriation of terms that specifically refer to the Earth as if they can serve for whatever celestial body may be under discussion. We are constantly finding the terms 'geology', 'geography', 'geophysics', and 'geologic' (the latter being American for 'geological') used of Jupiter or its satellites, as well as 'territory' and 'terrain'. The last is used particularly freely — in the caption on p. 160, for instance, it appears no fewer than nine times in eight lines.

Various minor mistakes were noticed in the course of a careful (and it must be said enjoyable) reading of the book and comparison of the captions with the pictures. For example, the reference to Plate Jc3 on p. 33 should be to Plate Jg3. On p. 144, the worm-like crater is at 13°·8 south, not 15°·5. On p. 192, a spot to which our attention is drawn is at 103° west, not 183°. Page 200 says southwest where it means southeast. One cannot follow the caption of Plate Je9.5.1, which refers to three features by their coordinates, because no coordinates are labelled on the picture. On pp. 229 and 231 two completely different images are both called Plate Je9.4; the second one has no caption. On pp. 246 and 247 the captions direct attention to features at coordinates far outside the boundaries of the respective pictures. The "oddly shaped bright streak" referred to in the caption to Plate Ji3.2.1c is not to be seen in the corresponding image but in part 1a on the facing page. The scale of longitudes on Plate Ji12.1 is altogether haywire: the labels 125, 130, 135, 140, and 150 should really run from 120 by increments of 10 to 160. But those and other minor errors really detract little from a splendid piece of work which certainly brings into sharp focus the natures of the four extraordinary satellites. It is just a pity that after one reading my book needs to be rebound properly. — R. F. GRIFFIN.

Saturn: Exploring the Mystery of the Ringed Planet, by N. Mortillaro (Firefly, Richmond Hill, Ontario), 2010. Pp. 95, 26 × 26 cm. Price £19·95 (hardbound; ISBN 978 1 56407 649 9).

This slim, colourful book sets out to examine the planet Saturn, with emphasis on the results of the *Cassini–Huygens* mission. Mortillaro is an author of children's books and the writing style suggests that this book was written for a young readership, so this review has been written with that in mind. The key feature is a selection of images of Saturn and its satellites taken by the *Cassini* orbiter, the *Huygens* lander, other Saturn missions, and the *Hubble Space Telescope*. Many of these images are well reproduced with only a few appearing dark.

The book is divided into chapters but the bulk of the text forms captions to many of the images; those images that did not have captions would have benefitted from such for clarity. It starts with an introduction to the planet and the satellites, but surprisingly does not provide an overview of the rings, which is left until much later in the book.

Some simplification of concepts is to be expected when writing for young readers. However, this should not be at the expense of accuracy, such as the statement that 'Saturn's internal heat sets the planet on fire'. A clearer description of some concepts, such as why Saturn's rings vary in appearance, would also have been of benefit. The book includes a brief glossary of terms, but this is far from complete as terms such as 'morphology', 'CCD', and 'thermal segregation' are used in the text but are not explained. And, alas, the text contains a number of errors, particularly relating to the objectives of some of the various interplanetary spacecraft. For example, it is erroneously stated that *Pioneer 10* came after *Galileo*, and the date of Neil Armstrong setting foot on the Moon is incorrectly given as "July 22 1968".

I hope that this book was produced to encourage young readers to take an interest in the planets and astronomy. Achievement of this perceived objective is helped by the selection of illustrations, which should delight both young and old readers alike. However, improvements to some parts of the text and the removal of errors would have made this a better book. Further, achievement of this objective is not helped by the statement on page 20, which says that

"Astronomy is a hobby that requires … superb mathematical skills …". This is certainly not the case and may put a young reader off what is a fascinating hobby, and indeed a fascinating planet. — MIKE FOULKES.

Cometography — A Catalog of Comets, Volume 5: 1960–1982, by Garry W. Kronk & Maik Mayer (Cambridge University Press), 2010. Pp. 820, 26 × 18 cm. Price £130/$250 (hardbound; ISBN 978 0 521 87226 3).

This superb multi-volume catalogue of cometary observations goes from strength to strength. Volume 1 started with the Babylonian observations of the comets of 675 BC. In Volume 5, the one under review, we are in the modern era when visual and photographic observations are being intercompared. Also, with the boom in amateur and professional astronomy, the number of observations per year, and the number of discovered comets per year, has increased very substantially, so much so that the two authors have had to do some judicious 'weeding' to reduce the number of pages per comet, concentrating on the findings of only the most experienced cometary observers.

The period 1960 to 1982 includes Comet Kohoutek (C/1973 E1) the "Comet of the Century", which turned out to be a disappointing damp squib, as well as the bright long-period comets Ikeya–Seki (C/1965 S1), Bennett (C/1969 Y1), and West (C/1975 V1), and the challenging large-telescope hunt for the returning Comet Halley in mid-1982, a time when the comet was at about 24th magnitude and 11 AU from the Sun, out beyond the orbit of Saturn. Towards the end of the 1960–1982 period we saw the first use of the charge-coupled device, a detector that has led to a considerable improvement in the accuracy of cometary-brightness estimates. It was also a time when the asymmetry of many cometary light-curves with respect to perihelion passage led to a more sanguine interpretation of cometary absolute magnitudes and activity indices.

This catalogue is an immensely useful chronological collection of cometary discovery details, celestial and Solar System positions, brightness variability, tail development, and orbital parameters. It is superbly referenced and is an obvious first port of call for anyone researching cometary observations, populations, and evolution. — DAVID W. HUGHES.

The 50 Most Extreme Places in Our Solar System, by D. Baker & T. Ratcliff (Harvard University Press, London), 2010. Pp. 290, 21 × 18·5 cm. Price £19·95/$27·95/€25·20 (hardbound; ISBN 978 0 674 04998 7).

The book aims to illustrate the great range of environments and landscapes that can be found in the Solar System and succeeds extremely well in that task. It is distinctly a coffee-table book, not a direct teaching text, nor one to read from beginning to end, as it covers so many different facts about the Solar System *via* extremes such as the longest-lived storm and the hardest rain; and as such I found it impossible to read as a continuous book since one's mind keeps thinking about the various topics covered. The book is full of excellent illustrations and pictures of Solar System objects, and *via* these images puts many things into perspective — for example, the size of the Martian volcano Olympus Mons, where the illustration (p. 4 of the book) very clearly shows how Olympus Mons dwarfs Mauna Kea and Mount Everest. Some of the images would make good illustrations for undergraduate-teaching courses.

The book covers, in some detail and — almost without effort — through its various sections, the physics, geology, geochemistry, and planetary science of the Solar System, although the various planets and moons are scattered throughout the book. The explanations are excellent and show how the same physics, geology, *etc.*, work throughout the Solar System; and there is a good section on the possibility of life elsewhere. It also contains some very simple experiments that can be carried out at home or in the classroom to illustrate some of the topics covered: for example, demonstration of plate tectonics using milk and powdered chocolate mix. Unfortunately I haven't had the time to try the various suggestions to see how well they work.

The book is up to date and deals well with recent discoveries and speculation about the various phenomena and environments; it also nicely covers the counter arguments to some of the postulated theories and interpretations. It aims, very much like Brian Cox's recent BBC TV series, to show the wonders found within the Solar System, comparing them where it can with the Earth. Given the book's title, there are some 'glib' chapter and section titles and text, 'stretches' of definition of terms, and also one or two awful puns buried in the text. It is, however, an excellent and fun book to read and will captivate anyone who has an interest in the Solar System, what has been discovered, and just how different, and in some cases how similar, environments and conditions are elsewhere. — Mark Sims.

The Rainbow Sky, by Tony Buick (Springer, Heidelberg), 2010. Pp. 377, 23·5 × 15·5 cm. Price £22·99/$34·95/€34·95 (paperback; ISBN 978 1 4419 1052 3).

Colour is a quality which is vital to both art and science. It is deceptively simple at first but becomes increasingly complex when examined more closely. Person-to-person differences in colour vision can be assessed by simple experiments, but there is no reason to believe that two people with the same test scores perceive objects the same. The response of the combined human eye and brain is not linear but roughly logarithmic. Moreover, the human response to colour is much greater in daylight than at the low levels typical in astronomy. In most books with colour images the aim is to produce an image on paper which appears identical to the object itself. In astronomical books the colours are often exaggerated to bring out scientific points or simply to impress the public.

The first chapter gives a clear description of these aspects, including the little-known fact that colour is appreciated differently between observing directly and observing between one's legs when turned in the opposite direction. This section includes a description of the relation between colour and 'effective temperature' — more correctly 'colour temperature'. To reproduce a colour on the printed page or a computer screen, one must convert each colour to its *r,g,b* values which are given here in both decimal and hexadecimal formats. Here, and in later chapters, the reader is directed to salient pages on the web which take the subject further.

Colour photography of spectra poses particularly difficult problems whether the detector is conventional colour film or a CCD chip. This problem is evident in Figures 3.3 and 5.10 which are identical but with different captions. The colours are unnaturally divided by clean boundaries instead of blending into each other. This is probably caused by the particular selection of dyes in the photographic emulsion and the overlapping of different grating orders.

The following chapter on 'Deep sky objects' contains many colour pictures. The extragalactic objects are mostly taken with the *Hubble Space Telescope* and the Galactic ones by members of the Orpington Astronomical Society. Strangely the Sun and solar eclipses are included in this chapter and not in the following one on 'Solar System colours'. The Solar System is well covered but the reader is left in doubt as to whether Uranus or Neptune is the planet with its rotation axis in the plane of the ecliptic. The caption to Fig 7.32 identifies the same comet as both Hale–Bopp and Holmes. To the human eye the Moon is nearly colourless but digital images can be manipulated so that the differences in colour are enormously exaggerated. It is well within the capabilities of amateur equipment to take and manipulate digital images; the results provide valuable information about minerals in the Moon's surface.

Natural colours in the Earth's atmosphere have fascinated mankind throughout the ages. Coloured subjects range from the fog bow, which is usually formed within a few feet of the observer, to the aurora 50 miles up. Rainbows, sun dogs and pillars, and haloes are all caused by scattering in the Earth's atmosphere. Shadows are clearly seen in the form of crepuscular (and anti-crepuscular) rays which can be formed by the Moon as well as the Sun. Observers on high mountains at sunset can see the shadow of the mountain on the cloud deck below and there is a reverse effect from ground level. Low clouds may be fascinating to the meteorologist and may produce beautiful sunsets but are merely a nuisance to astronomers. However, no one can deny the beauty of polar stratospheric clouds (nacreous clouds) and noctilucent clouds. The 'opposition effect', which makes the full Moon so bright, also occurs on Earth. There is a striking image here of the shadow of a balloon surrounded by a halo, taken by a passenger in the balloon.

Since the advent of the space age we have become used to seeing satellites cross the sky during twilight and *Iridium* flares even by day. At the same time mankind profligately wastes light and obliterates the beauties of the night sky in so doing.

On one occasion the author accidentally kicked his camera tripod while photographing star trails. This produced a Lissajou figure rather than the hoped-for result. He went on to repeat the experiment under more controlled conditions and then built a dedicated instrument which moved his telescope in small circles. Several of the results are reproduced here. The whole can be summed up by the title of one of the last chapters: 'Star colours for fun'. — DEREK JONES.

Cosmic Challenge: The Ultimate Observing List for Amateurs, by P. S. Harrington (Cambridge University Press), 2010. Pp. 469, 25 × 19·5 cm. Price £27·50/$45 (hardbound; ISBN 978 0 521 89936 9).

Phil Harrington has written a number of books on equipment and observing and he is perhaps best known for his work on binocular astronomy. This new book from CUP for visual observers covers a series of challenges that have been organized into sections for the naked eye, binoculars, and for small, medium, large, and monster telescopes. Each section is similar in layout in that there is a description of the object along with a drawing (in many cases) and a chart showing where the object is in the sky, if required. The choice of objects in each section is very eclectic varying from features on the Moon and the moons of the outer planets to galaxy clusters and quasars. The section for each object includes a visual description along with some information on the object itself. The choice of objects, as the author describes, is very much a personal one, and it will include many familiar objects as well as ones you may not have heard of. There is also a section on what kinds of equipment to use and how it should be configured to meet the challenges. How you see the challenges may well depend on your location and level of skill as to whether or not you see the binocular challenges as more suitable to a small telescope, *etc*. So how well does the book succeed? Well, I think this is an excellent addition to the collection of deep-sky books and should be on every amateur's bookshelf. I found many of my favourite objects in the lists along with some other new ones that I will have a go at when I next have a clear sky in that observing season. An appendix lists the objects and the best observing season for them so you can easily decide when is the best observing time for each object. — OWEN BRAZELL.

Stars Above, Earth Below, by T. Nordgren (Springer, Heidelberg), 2010. Pp. 300, 24 × 17 cm. Price £26·99/$29·95/€29·95 (paperback; ISBN 978 0 4419 1648 8).

This book is an introduction to astronomy through the lens of the most spectacular landscapes in the United States. It is likely to resonate strongly with relative newcomers to astronomy who pick it up on a tour through any one of the 58 US National Parks, and it will be a great companion to the vast dark skies of those parks — feeding imaginations primed by spectacular starry nights. It will also appeal to readers with a wider interest in wild landscapes.

The author draws on four years in these environments as a public astronomer with the US National Parks Service. This distinctive approach to relatively familiar astronomical terrain is signposted nicely by its eleven chapter headings in which each chapter is linked to an astronomical theme such as environments on other planets. For example, Chapter 4, 'World of ice and fire', uses an understanding of volcanoes in Yellowstone National Park to explore volcanoes elsewhere in our Solar System. In Chapter 6, Glacier and all glacial National Parks are used to discuss climate change on Venus, Earth, and Mars. The author also uses astronomical events in specific parks to explore ideas about the Universe, such as a description of a solar eclipse he witnessed in Grand Teton National Park as an introduction to black holes and gravity. Lastly, there is the cultural dimension: Chapter 8, 'Our cosmic connection', explores the archaeoastronomy of Chaco Culture National Historical Park.

In its approach, the book is an excellent example of the growing global trend of developing partnerships between astronomers and environmental managers. The Dark Skies Awareness Cornerstone Project of the International Year of Astronomy 2009 provided an umbrella for many such collaborations. In the UK, the Forestry Commission Scotland successfully established the Galloway Dark Sky Park, having been a partner in the Dark Sky Scotland events programme since 2006. During 2009 many UK astronomical societies strengthened their links with the managers of local dark-sky places.

The book includes a good number of eye-catching images and illustrations to draw the reader into its themes. As a general introduction to astronomy the style is individual and idiosyncratic. The approach weaves together, for example, personal anecdotes, philosophical observations, and practical seasonal star-charts. A shorter, glossy version would make a fantastic coffee-table book, perhaps with even greater impact in some respects. — DAN HILLIER.

The Universe and Beyond, 5th Edition, by T. Dickinson (Firefly, Richmond Hill, Ontario), 2010. Pp. 204, 26·5 × 26·5 cm, Price £19·95 (paperback; ISBN 978 156407 748 9).

Writing a large-format, popular-level account of astronomy is no easy undertaking, and the bargain bins are full of the failures. Frequently, authors concentrate all their effort on making the book visually pleasing, whilst neglecting the minor detail of providing scientific content in an accessible manner that reflects the enthusiasm and excitement of the research world.

Given that *The Universe and Beyond* has now reached 200 000 sales, Dickinson is clearly striking an acceptable balance between the two.

It's undeniably an appealing book, with almost every page boasting an eye-catching NASA/*HST*-sourced colour image. The other images consist of a mixture derived from large ground-based observatories, the work of 'astroartists', and some amateur 'astroimagers'. The accompanying text is confidently written, with a scattering of personal reflections, anecdotes, and reminiscences that serve to enliven proceedings and avoid it all becoming soulless. This approach works well most of the time, with Dickinson's time at a planetarium probably explaining his ability to strike a happy balance between 'wow' and 'how', while still delivering some nice turns of phrase and helpful analogies. The chapter on the planets is especially well illustrated and contains considerable detailed information. Unfortunately, the Moon is, by comparison, rather poorly served, with just a page or so of text and three pictures — two the size of postage stamps. It would have been nice, perhaps, to have seen one of the *Lunar Reconnaissance Orbiter* images showing the tracks laid down by *Apollo 14* during the astronauts' time on the surface.

Another worthwhile change would be the removal of some of the full-page artists' impressions or, even, the whole of the rather dull Chapter 11, 'Reflections on astronomical illustration', in favour of an index. More importantly, most of the images contained within the final chapter, 'Cosmic update', should have been integrated into the text at the appropriate point, rather than being bundled up and appended at the end of the book — it's not what you hope for from a "fully revised edition".

Inevitably, there are a number of points in the text that could be argued with, but only a few typos. For example, Dickinson implies that the discovery of black-hole candidates had to await the advent of orbiting X-ray satellites, and yet Cygnus-X1 appears in a 1967 *Astronomical Journal* paper by Ouelette listing the 17 discrete X-ray sources then known, all discovered by sounding rockets. This sort of thing should have been picked up by the editor, but there was little that would seriously derail understanding.

All that said, I quite liked the book and found it an interesting introduction for someone wanting a quick and colourful summary of the state of knowledge. It would certainly go down well with someone new to astronomy as a hobby. — Grant Privett.

Comets and How to Observe Them, by R. Schmude (Springer, Heidelberg), 2010. Pp. 254, 23·5 × 15·5 cm. Price £31·99/$34·95/€34·95 (paperback; ISBN 978 1 4419 5789 4).

This is a curious book — curious in the sense that it is difficult to see how many of its faults arose. The author has written an earlier book in the series and has a physics degree, yet there are elementary errors in the text, which is often clumsy. Frequently literature is quoted in the form "An astronomer reported..." instead of giving the named reference listed in the bibliography. To avoid discrimination the form "he or she" is frequently used, yet this just emphasizes that discrimination may lie below the surface. Some of the apparent errors may just be clumsy wording, for example "Consequently, Earth's Polar Regions are usually very cold and, hence little sunlight reaches them" and "Resolution ... affects the focus". The series editor is a respected Fellow of the Royal Astronomical Society, yet there are several technical errors. For example, temperatures are given as °K rather than K. Springer is a well-established company, yet there are a number of production faults. For instance, many of the pictures on even-numbered pages suffer from white streaks, and in the review copy pages 77 and 78 were missing. A connection between all these may be the US ranking in its quality of science and mathematics education (48th globally) as the book was written and produced in the USA.

I have a particular gripe with the conservative usage of comet nomenclature. For around a decade the IAU *Catalogue of Cometary Orbits* has given guidance on the modern comet nomenclature, yet many authors (including Schmude), journals (including *A&G*), and organizations (including NASA) still use the old designations for periodic comets. For example, the comet observed by the *EPOXI* spacecraft should be reported as "103P/Hartley" and not, as frequently appears, "Comet Hartley 2".

For the reader who can overcome such obstacles the book is full of useful and helpful material. The content is well organized, and covers much technical background about comets at a scientific level, including equations. The author is an experienced observer and has included lots of tips on how to make and improve observations, such as resting your elbows on your car roof when observing with larger-aperture binoculars. If its failings are corrected in a reprint it will become a really worthwhile book. — JONATHAN SHANKLIN.

Astronomers Anonymous; Getting Help with the Puzzles and Pitfalls of Practical Astronomy, by S. Ringwood (Springer, New York), 2010. Pp. 237, 23·5 × 15·5 cm. Price £26·99/$29·95/€29·95 (paperback; ISBN 978 1 4419 5816 7).

"Don't judge a book by its cover" has always been a good adage. But what does one make of this one, featuring a (ghastly) photograph of the author, posed to resemble Edvard Munch's *Skrik* ('The Scream')? The blurb promises much: "... questions and problems practical and amateur astronomers face ... His screamingly funny comments will keep you laughing out loud ... so be careful of reading this book in public!"

The format consists of purported letters written to an imaginary 'astronomical agony aunt', each accompanied by a 'humorous' response, and a serious short essay, tenuously linked with the subject. The ostensibly original letters are puerile in their humour and content, on the level of "My new telescope can't see through clouds — why not?". And are supposed letters from a drunk, an alien — a nonsensical jumble of letters — or a call-girl, funny? The 'replies' are, in the main, asinine, and (although it may be just me) none even raised a smile or an internal twitch of appreciation.

The short, more-or-less-factual, passages are generally acceptable, but even there, unfortunately, they encompass a number of errors. How many times, for example, must we reiterate that, contrary to the statement on p. 146, dark adaption occurs, not because of the (essentially instantaneous) expansion of the pupil, but through the progressive enhancement of rhodopsin in the retina? The whole text is marred — not necessarily by the use of American spelling ('whiskey' for 'whisky', *etc.*), although this sometimes jars with the otherwise British accent — but with numerous examples of poor spelling and editing errors.

In recent years it has been distressing to see the name of Springer, once a highly respected, family-based scientific publisher (now owned by a conglomerate, of course) issuing so many books of dubious quality. That it should be associated with such drivel as this is a disaster. Frankly, after reading through this book (twice, searching for some redeeming features), I feel I should be the one imitating 'The Scream'. As for it assisting anyone in 'Getting help with the Puzzles and Pitfalls of Practical Astronomy' — now, that *is* a laugh! — STORM DUNLOP.

OTHER BOOKS RECEIVED

Advanced Mechanics and General Relativity, by J. Franklin (Cambridge University Press), 2010. Pp. 367, 25 × 18 cm. Price £45/$75 (hardbound; ISBN 978 0 521 76245 8).

This book starts with classical mechanics in its Lagrangian and Hamiltonian forms, and gradually introduces much of the apparatus of tensor calculus and relativity. It is aimed at advanced undergraduates with background knowledge of classical mechanics and electricity and magnetism.

The Pinch Technique and its Applications to Non-Abelian Gauge Theories, by J. M. Cornwall, J. Papavassiliou & D. Binosi (Cambridge University Press), 2010. Pp. 286, 25·5 × 18 cm. Price £70/$115 (hardbound; ISBN 978 0 521 43752 3).

Aimed at graduate students and researchers in elementary-particle physics, this Cambridge Monograph shows how Green's functions can be constructed through the pinch technique, with applications in quantum chromodynamics, electroweak theory, and other areas of particle physics.

THESIS ABSTRACTS

DYNAMICAL ASPECTS OF JOVIAN IRREGULAR SATELLITES

By Tobias Cornelius Hinse

This thesis concerns the mapping of chaotic resonances and the long-term dynamics of Jovian irregular satellites. In order to obtain a detailed dynamical picture of the phase-space structure occupied by the observed satellites, we applied the numerical MEGNO (Mean Exponential Growth factor of Nearby Orbits) technique to detect quantitatively chaotic resonant dynamics. By following numerically an unprecedented ensemble of test satellites we successfully identified the location of orbital resonances and their occupation in phase space. We carried out a complete mapping of the chaotic topology of satellite phase space in the form of high-resolution MEGNO maps. In order to associate orbital resonances with their respective dynamical effects we considered solar and Saturnian perturbations separately. In the restricted three-body (Jupiter–satellite–Sun) problem we show that *the phase space occupied by retrograde Jovian irregular satellites is dominated by numerous solar high-order mean-motion resonances*. These resonances are characterized by showing dynamical properties associated with chaos. The MEGNO technique also allowed us to detect the location of the secular resonance $\varpi - \varpi_{\odot} \approx 0$ when including Saturn's perturbing effects. Furthermore, the orbits of the satellites Carpo (prograde) and S/2003 J02 (retrograde) are found to be close to chaotic regions. Using single-orbit integrations we obtained numerical evidence that S/2003 J02 possibly exhibits long-term stable (or 'sticky') chaos.

The location of solar-mean-motion resonances curiously coincides with satellite members of the retrograde Pasiphae family exhibiting a significant orbital dispersion in (a, e) and (a, I) space. *Based on this result, we considered the hypothesis that long-term orbital diffusion is driven by solar high-order chaotic-mean-motion resonances*. Assuming that retrograde satellite families originated from a single collisional break-up event, the process of chaotic diffusion by mean-motion resonances could provide an effective transport mechanism in phase space, possibly solving a long-lasting conundrum as pointed out by Nesvorny *et al.* (*Icarus*, **157**, 155, 2002; *AJ*, **127**, 1768, 2004). Using Gauss' equations we calculated the observed velocity dispersion of the retrograde Jovian satellite families to be of order $\delta V \approx 320$ m s^{-1}. This is significantly larger than expected from the kinematics of a collisional break-up event. Numerical hydrocode simulations and laboratory impact experiments suggest a typical velocity dispersion on the order of a few tens of m s^{-1}.

To test our hypothesis we carried out long-term numerical orbit integrations using accurate adaptive time step and fast symplectic algorithms. For each retrograde satellite family we adopted an isotropic-ejection model to generate initial conditions of test particles representing the initial state of a post-collisional fragmentation cloud. The particles in each fragmentation cloud were centred around the most massive (largest) satellite and we integrated numerically the system over 4–5 Gyr considering only solar perturbations. As a result we were able *to demonstrate insignificant chaotic orbital diffusion in proper element space of retrograde satellites by solar high-order mean-motion resonances*.

In another attempt to identify the underlying dynamical mechanism capable of increasing the velocity dispersion of satellite fragments produced by a collisional-break-up event, we studied the effects of long-term perturbations by Saturn. We find that long-term chaotic diffusion in eccentricity and inclination is strongly associated with secular perturbations involving exchange of angular momentum between the satellite orbit and Saturn. Our results could partially reproduce the observed distribution of retrograde-orbital mean elements. This finding supports our initial assumption of a collisional-break-up event for all three retrograde-satellite families.

Finally, due to chaotic diffusion of the proper inclination we demonstrate numerically the possibility of contamination of the Carme family with fragment members originating from the Pasiphae family. Observational support from photometric surveys indicates the existence of colour differences among Carme members, suggesting contamination of the Carme family, assuming a homogeneous progenitor satellite. We propose further photometric follow-up observations in order to test and further constrain these ideas.

In summary, we have provided the first detailed mapping of Jovian irregular-satellite phase space using MEGNO; we have investigated a dynamical explanation for the relatively large velocity dispersion of the identified families of Jovian irregular satellites, and have shown that their origin is consistent with formation in a primordial break-up event. The resulting fragments then experienced a subsequent dynamical diffusion of orbital elements primarily driven by secular perturbations of Saturn. — *Queens University of Belfast/Armagh Observatory; accepted 2010 December.*

Characterizing Ultra-Luminous Infrared Galaxies in the Early Universe

By Caitlin Casey

Ultra-luminous infrared galaxies (ULIRGs) exhibit the most extreme star-formation rates (SFR) in the Universe. At early epochs ($z > 1$), ULIRG activity contributes significantly to the build-up of stellar mass through intense star-forming bursts (with $\tau < 100$ Myr, SFR > 500 $M_{\odot}$ yr^{-1}). Since the observed properties of these starbursts are short-lived and extreme, they are thought to be triggered by the collision of gas-rich disc galaxies and serve as a fundamental transition phase to luminous active galactic nuclei. While ULIRGs are likely to be responsible for the formation of massive elliptical galaxies in the local Universe, much about the population is still unknown due to limitations in far-infrared (FIR) observations, strong selection biasses, and sample inhomogeneity. Submillimetre galaxies (SMGs, a subset of $z > 1$ ULIRGs) put powerful constraints on galaxy-evolution theories and the environments of heavy star formation, but their selection at 850 μm is susceptible to strong temperature biassing. This implies that a significant fraction of high-z ULIRGs have yet to be discovered.

This PhD thesis aims to reduce the effect of selection bias on the understanding of high-z ULIRGs by characterizing a population of submillimetre-faint star-forming radio galaxies (SFRGs). SFRGs have similarly high star-formation rates as SMGs, but I present evidence in this thesis that SFRGs are overall less bolometrically luminous, contain a slightly higher fraction of emission from active galactic nuclei (AGN), and that there is a lack of hotter-dust (> 60 K) SFRGs at the highest bolometric luminosities (> $10^{13} L_{\odot}$). In Chapter 2 I present a population of SFRGs which are 70-μm-luminous, showing that they are star-formation dominated and have hotter dust temperatures than SMGs of similar luminosities and redshifts. Chapter 3 presents high-resolution *MERLIN* radio imaging of two $z \sim 2$ SFRGs which were originally thought to be star-formation dominated, but the *MERLIN* radio imaging combined with a multiwavelength study shows that the systems are actually evolved, low-luminosity AGN. This highlights the usefulness of *MERLIN* radio observations in breaking degeneracies and characterizing galaxies. Chapter 4 presents neutral-carbon observations of a set of $z \sim 4$ SMGs; neutral carbon provides an independent measurement of a galaxy's star-forming gas reservoir. This serves as a pilot study for future *Atacama Large Millimeter Array* (*ALMA*) observations of large samples of high-z ULIRGs. Chapter 5 describes a CO survey of SFRGs which probed their molecular gas, leading to a characterization of the population. SFRGs are equally efficient at forming stars as SMGs but are somewhat scaled down in luminosity. Chapter 6 presents a redshift survey and FIR characterization of 250-μm-luminous galaxies. The spectral-energy distributions fitted measure dust temperature, luminosity, and emissivity independent of the FIR/radio correlation. I find a dearth of hot-dust hyperluminous infrared galaxies (HyLIRGs) in the sample, which supports results from Chapter 5, arguing that the most luminous HyLIRGs are dominated by cold, diffuse dust brought on by major mergers. Chapter 7 presents mid-infrared spectra of SFRGs, presenting the first quantitative measurement of their AGN content, which appears to be slightly increased relative to SMGs of similar luminosities. Chapter 8 summarizes the goals of this work and outlines pertinent projects which will serve as the natural extension of this work for the next several years. — *University of Cambridge; accepted 2010 August*.

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The Origin of Hot Dust Around Sun-Like Stars

By Mark Booth

Within our Solar System lie two belts of planetesimals, the asteroid belt and the Kuiper Belt. Collisions between these planetesimals produces the dust that forms our debris disc. Many other stars have been observed to be surrounded by debris discs much more massive than our own. Most of these discs can be explained by steady-state collisional evolution. However, some systems have more hot dust present than we would expect given the age of the system. In this thesis we explore some potential explanations for this high level of hot dust.

One potential explanation is that the dust is the result of a transient event similar to the Late Heavy Bombardment (LHB) in our own Solar System. We investigate the IR emission from the Kuiper Belt during the history of the Solar System. We show that the Solar System would have been amongst the brightest debris discs before the LHB at both 24 and 70 μm. We find a significant increase in 24-μm emission during the LHB, which rapidly drops off and becomes undetectable within 30 Myr, whereas the 70-μm emission remains detectable until 360 Myr after the LHB. Comparison with the statistics of debris-disc evolution shows that such depletion events must be rare, occurring around less than 12% of Sun-like stars.

The latest models for the formation of the asteroids suggest that large planetesimals could have formed quickly. We follow the collisional evolution of the early stages of the asteroid belt including dynamical depletion from embedded embryos and show what this would have looked like in terms of emission. Under these assumptions we find that the asteroid belt would be observable for ~ 100 Myr at 24 μm. We find that some systems showing an excess in 24-μm emission at young ages could be harbouring massive asteroid belts.

Debris discs are generally modelled by assuming circular rings of planetesimals. However, for systems with a large amount of hot dust, it may be possible to explain the observations by a disc of eccentric planetesimals. By increasing the eccentricity we can increase the mass remaining at late times and so, for the highest eccentricities, we can increase the emission from the system. With a highly eccentric disc, the dust is distributed over a wider range of distances and so the cold emission is also increased. This makes it difficult to explain the HD 69830 system with this model due to the lack of 70-μm emission seen in this system, but it may be useful for explaining other situations in which highly eccentric populations of planetesimals may exist. — *University of Cambridge; accepted 2010 August*.

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Characterization and Mitigation of Radiation Damage on the *Gaia* Astrometric Field

By Scott Brown

In 2012 November, the European Space Agency (ESA) is planning to launch *Gaia*, a mission designed to measure with microarcsecond accuracy the astrometric properties of over a billion stars. Microarcsecond astrometry requires extremely accurate positional measurements of individual stellar transits on the focal plane, which can be disrupted by radiation-induced charge-transfer inefficiency (CTI). *Gaia* will suffer radiation damage, impacting on the science performance, which has led to a series of radiation campaigns (RCs) being carried out by industry to investigate these issues. The goal of this thesis is to assess rigorously these campaigns and facilitate how to deal with CTI in the data processing.

I begin in Chapter 1 by giving an overview of astrometry and photometry, introducing the concept of stellar parallax, and establishing why observing from space is paramount for performing global, absolute astrometry. As demonstrated by *Hipparcos*, the concept is sound. After reviewing the *Gaia* payload and discussing how astrometric and photometric parameters are determined in practice, we introduce the issue of radiation-induced CTI and how it may be dealt with.

The on-board mitigating strategies are investigated in detail in Chapter 2. Here I analyse the effects of radiation damage as a function of magnitude with and without a diffuse optical background, charge injection, and the use of gates, and also discover a number of calibration issues. Some of these issues are expected to be removed during flight testing, others will have to be dealt with as part of the data processing, *e.g.*, CCD stitches and the charge-injection tail.

In Chapter 3 I turn to look at the physical properties of a *Gaia* CCD. Using data from RC2, the density of traps (*i.e.*, damaged sites) is probed in each pixel and, for the first time, the full-well capacity of the supplementary buried channel is measured, a part of every *Gaia* pixel that constrains the passage of faint signals away from the bulk of traps throughout the rest of the pixel.

The Data Processing and Analysis Consortium (DPAC) is currently adopting a forward-modelling approach to calibrate radiation damage in the data processing. This incorporates a charge-distortion model (CDM), which is investigated in Chapter 4. I find that although the CDM performs well there are a number of degeneracies in the model parameters, which may be probed further by better experimental data and a more realistic model. Another way of assessing the performance of a CDM is explored in Chapter 5. Using a Monte Carlo approach I test how well the CDM can extract accurate image parameters. It is found that the CDM must be highly robust to achieve a moderate degree of accuracy and that the fitting is limited by assigning finite window sizes to the image shapes. Finally, in Chapter 6, I summarize our findings on the campaign analyses, the on-board mitigating strategies, and on how well we are currently able to handle radiation damage in the data processing. — *University of Cambridge; accepted 2010 December*.

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Here and There

VIA A WORM HOLE, PERHAPS?

A new galaxy spotted by the Hubble Space Telescope is 13·1 billion light years away and would take the space shuttle, at 17,600 mph, 1·35 million years to reach it, said astronomers. — *Daily Telegraph*, 2010 October 21, p. 14.

TEN KILOMETRES TO THE MILE?

… Miranda, a patchwork quilt of a moon, bizarre beyond belief. Three thousand kilometres across, … — *Astronomy Now*, 2011 January, p. 73.

IT MAKES A DAY OUT

The Saturday opening for the Library is new for 2010, and is arranged for every third Friday of the Month, … — *FAS Newsletter* 95, Winter 2010.