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A REVIEW OF ASTRONOMY

EDITED BY

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2006 March 10th at $16^h\,00^m$
in the Geological Society Lecture Theatre, Burlington House

K. A. WHALER, *President*
in the Chair

The President. I'll begin with the usual string of announcements before we move to the scientific programme. New Fellows, or long-serving Fellows who have not previously taken the opportunity, are warmly invited to sign the membership book after the meeting, across in the Society's apartments in the less-formal atmosphere of the drinks party, where I will be very pleased to welcome them to the Society and invite them to 'sign the book'. I hope that all Fellows will take advantage of this opportunity at this or future meetings.

Some of you will know that Sir Patrick Moore went into hospital in Chichester earlier this week to have a pacemaker fitted. Although this is a routine operation, at his age and with his health, there was a slight degree of concern. I'm delighted to inform everyone that he is making a good recovery and should be home by the weekend.

We're very lucky today to have a whole series of talks by the 2005 Philip Leverhulme Prize-winners. One of them was given a couple of months ago in this season's programme and, you'll have seen from this meeting's programme, one of our speakers has unfortunately had to cancel at short notice due to illness, so we hope to be able to reschedule that. We do have permission from the Geological Society to overrun a bit, but I'm going to try and keep more or less to time, with the speakers talking for about 25 minutes and then five minutes or so for questions afterwards. I'd like to invite the first speaker, Katherine Blundell from Oxford, to give her talk on 'Astrophysical jets'.

Dr. Katherine Blundell. Jets are ubiquitous on many different size scales throughout the Universe. By far the longest-known are the powerful jets in quasars and other extragalactic objects such as radio galaxies. Although jet systems are known in many hundreds if not thousands of such objects, detailed studies of their individual evolution — specifically their temporal evolution — are beyond reach. This is because, with characteristic evolutionary timescales of $\sim 10^7$ years, we cannot make well-time-sampled observations of the evolution, or more correctly, the ageing of any individual object. Many studies of extragalactic jets thus focus on statistical analyses of samples of objects. In contrast, within the Galaxy, jets emanating from compact objects which are probably ~10^7 or 10^8 times less massive than those of quasars evolve on correspondingly more rapid timescales: the jets in these objects evolve on timescales of hours, days, and weeks, rather than millions of years. Time-resolved observations of the so-called 'nanoquasars' play an important rôle in illuminating our understanding of the relevant physical processes at play, which invariably inform our analysis of quasar jets.

A particularly paradigmatic example of a nanoquasar is SS 433, mainly famous as the first known relativistic jet source in the Galaxy. Red-shifting and blue-shifting optical lines, indicating velocities of $0 \cdot 26c$, were discovered by Margon *et al.* and Liebert *et al.* and interpreted as being from gas accelerated in oppositely-directed jets, which precess, much like the paddle of a kayaker in the rest frame of the kayak. My colleague Michael Bowler and I recently published the deepest radio image to date of this object, which reveals a historical record of two complete precession cycles of its jet axis. We analysed the details of this image, demonstrating rather compelling evidence that the distance between Earth and SS 433 is $5 \cdot 5 \pm 0 \cdot 2$ kpc. We further identified evidence for variations in the jet speed, about an average of $0 \cdot 26c$, purely from the details of the structure revealed in this radio image. Our findings led us to re-investigate the archive of optical spectroscopic data accumulated over the last 25 years. This optical investigation confirmed our findings from the radio study that there are variations in the jet speed of SS 433 and, moreover, revealed that these anti-correlate with variations in precession cone-angle. Discovery of this behaviour may herald a new era of investigation and learning about SS 433 and jet launch in general.

Quasars and radio galaxies are 'slow-motion' versions of nanoquasars. An important observational point about the jets in these objects is how faint they are, relative to the immense luminosity of the radio-emitting lobes they feed. There are good reasons to believe that this luminosity is due to synchrotron radiation from relativistic particles in the presence of magnetic fields, but it is not clear whether there are generally high number densities of relativistic particles and weak magnetic fields, or low number densities of relativistic particles and strong magnetic fields: for synchrotron radiation these quantities are degenerate. Combining radio imaging with X-ray imaging provides an important means of breaking this degeneracy. With my colleagues in Cambridge (Andy Fabian, Carolin Crawford & Mary Erlund) and Trieste (Annalisa Celotti), our comparative analysis of a giant radio galaxy reveals that X-ray emission (likely due to inverse Compton scattering off the cosmic microwave background, known as ICCMB) is preferentially found where the plasma in the lobe is oldest (*i.e.*, a long time has elapsed since that plasma was accelerated in the hotspot). ICCMB mandates the existence of relativistic particles whose Lorentz factors (gammas) are about 1000. The preferential association of ICCMB emission where the lobe plasma is oldest suggests that in the younger (more recently accelerated) plasma the Lorentz factors are rather higher than 1000, implying that what has become known as the minimum-gamma cutoff is in the region of, or somewhat greater than, 1000. If this interpretation is correct, then magnetic-field strengths are weaker than previously surmised and knowledge of the actual energy residing in the plasma reservoirs which constitute the lobes of radio-active galaxies is correspondingly much more securely known. Given increasing awareness of the influence of active galaxies on the Universe at large, this is an important step forwards.

The President. Thanks very much, Katherine. We have some time for questions. Donald?

Professor D. Lynden-Bell. Should you, or did you, allow for the time delay when you added Z_+ to Z_-?

Dr. Blundell. The light-travel-time effects are in fact very small and don't affect the measurement of the wavelengths of the simultaneously ejected redshifted and blueshifted jet bolides.

Dr. R. C. Smith. You argued, for the first radio galaxy you showed, that you had a displacement between the radio and the X-ray emission on the east. In the west lobe, they were coincident. Does that square with what you're talking about?

Dr. Blundell. It depends on what is the origin of the west X-rays. It's alleged that on the west side, that hotspot is gravitationally lensed, so whatever is going on there, the light is having a nasty accident on the way from it to us. I don't know whether I'm completely persuaded that it's gravitationally lensed, but it's a possibility. Another possibility is that you probably noticed the asymmetry in length between the west side and the east side. The east side is shooting away at high speed and is very long and thin, whereas the west side is much fatter. Now, it's actually the losses in the transverse direction that give you massive adiabatic expansion losses, so it may be that the losses that turn $\gamma = 10^4$ particles into $\gamma = 10^3$ particles happen much more quickly on the west side which just expands sideways, whereas the head of the east side shoots off without much transverse expansion.

Mr. M. Hepburn. Just on orders of magnitude, am I right in saying that your distant powerful radio galaxies have lobes that are separated by more than our Galaxy from the Andromeda galaxy?

Dr. Blundell. Yes, that's correct.

Mr. Hepburn. Secondly, you were giving a Lorentz factor of around 10^6 for the electrons concerned in the generation of the radio emission, which corresponds to what — about 1000 GeV per electron? Is that right? It seems quite amazingly energetic to me.

Dr. Blundell. Absolutely! It's in the right ball-park, certainly.

Dr. G. Q. G. Stanley. The 13·08-day period you saw on the Fourier transform — I guess that's very well defined, is it? Or not? Because that would indicate the processing speed and dissipation you'd get from material from the neighbouring star going though a disc and being ejected. It looks as if it must be a very fast transfer.

Dr. Blundell. It's very well defined. The 13·08-day period is (something like) the periodicity of the compact object orbiting the companion star.

Dr. Stanley. But that 13·08-day period is being picked up in the jet streams, which shows that it's a very fast processing time, from the material.

Dr. Blundell. Certainly, it's very quick. But it is a speed, of course, not a velocity, in which we're seeing that periodicity and it's not clear whether that corresponds to a speed along the instantaneous jet axis, or whether it's actually a Doppler effect that we're picking up because of the orbit of the compact object around the companion star. However, if you think the latter explanation is correct, you require an enormous mass indeed for the companion star, perhaps 100 solar masses.

The President. Right, I think we should finish there. Thanks, once again Katherine, for a really enjoyable talk. [Applause.]

Our second speaker in this session is Andrew Bunker from Exeter, who will be talking about 'Finding star-forming galaxies in the early Universe'.

Dr. A. Bunker. Tracing the star-formation history of the Universe is central to charting the evolution of the galaxies. However, the first attempts to measure the star-formation-rate density per unit comoving volume (see *MNRAS*, **283**, 1388, 1996 and *ApJ*, **460**, L1, 1996) were flawed. The data were restricted to the optical, where the instrumentation was most mature, which samples progressively shorter rest-frame wavelengths at the higher redshifts; the typical star-formation indicators used are Hα 6563Å at $z < 0 \cdot 5$, [OII] 3727Å out to $z \sim 1$, and the rest-frame ultraviolet (UV) continuum at redshifts beyond. Uncertain relative calibration between these indicators, and the increasing effect of dust reddening for the UV, means that the true evolution in the global star-formation rate is hard to disentangle. Ideally, one wants to use the same indicator over most of cosmic time. The Hα line in the optical is a relatively robust and well-calibrated star-formation measure, and less sensitive to dust than shorter-wavelength indicators (see, *e.g.*, *ARA&A*, **36**, 189, 1998). To trace this at redshifts beyond $z > 0 \cdot 6$ forces a move to the near-infrared (NIR), but until recently NIR spectroscopy has been restricted to single-object long-slit work, so building large samples of objects has proved prohibitively expensive in telescope time. Working with Ian Parry and colleagues in the instrumentation group at the Institute of Astronomy (IoA) in Cambridge, we have adapted the *CIRPASS* near-infrared spectrograph (see *SPIE*, **4008**, 1193, 2000) to take spectra of ~ 70 objects simultaneously, using a fibre-fed plug-plate. In 2003–2005, we became the first team to use multi-object-spectroscopy techniques on distant galaxies in the near-infrared, using *CIRPASS* on both the 4·2-m *William Herschel Telescope* in La Palma and the 3·9-m *Anglo-Australian Telescope*. The aim was to measure star-formation rates of galaxies half-way back in time to the Big Bang, using Hα redshifted to 1–1·6 μm, falling between OH sky lines in the *J* and *H* bands at $0 \cdot 7 < z < 1 \cdot 5$. Working with my IoA graduate student Michelle Doherty (now a postdoc at the European Southern Observatory in Garching), we studied a large sample of galaxies in the Hubble Deep Field (see *MNRAS*, **354**, L7, 2004 and astro-ph/0604584), showing that the global star-formation rate at $z \sim 1$ really was much greater than at the current epoch.

Moving now from $z \sim 1$–2 to even higher redshifts, working with another of my PhD students at IoA Cambridge, Elizabeth Stanway (now a postdoc at University of Wisconsin), and in collaboration with Richard McMahon, I developed a technique in 2002–2003 to isolate potentially very distant galaxies from the bulk of foreground sources in deep *Hubble Space Telescope* (*HST*) images, by using filters close in wavelength to search for a sharp drop in brightness caused by intervening gas in the Universe absorbing the blue light. This built on the 'Lyman-break technique' which had been successful at redshifts 3 and 4 (see, *e.g.*, *ApJ*, **462**, L17, 1996 and *ApJ*, **519**, 1, 1999), and we pushed this study to redshift 6 with our '*i*-band drop-out' method (see *MNRAS*, **342**, 439, 2003), to some of the most distant objects yet identified. It was important to confirm that these candidate distant objects really are at redshifts around 6, when the Universe was less than 10% of its current age, and we published the first proof of this (see *MNRAS*, **342**, L47, 2003): in collaboration with Richard Ellis we used the 10-m *Keck* telescope in Hawaii (the largest in the world) to take a spectrum of one of the galaxies we had identified in the *Hubble* images. We detected the Lyα emission line of hydrogen, redshifted by the expansion of the Universe all the way from the ultraviolet to the near-infrared. We were able to infer the rate at which stars were born in this galaxy. This was the first spectroscopic confirmation of the '*i*-band drop-out' technique, and the most distant galaxy with a robust redshift to be identified through *HST* imaging.

My team were the first to analyse and publish results from the Ultra Deep Field (released in 2004 March) — the most sensitive image ever taken, from 400 orbits with the *HST* — identifying more than 50 objects likely to be star-forming galaxies at some of the highest redshifts yet explored (*MNRAS*, **355**, 374, 2004). The Ultra Deep Field allowed us to explore galaxies of unprecedented faintness; this is important, as a long-standing question is how many under-luminous 'dwarf' galaxies there are in the early Universe. We measured the rate at which the Universe was forming stars at redshift around 6, and found that the ultraviolet ionizing light from the birth of the most massive stars was insufficient to re-ionize the Universe during this era. Re-ionization is a key event in the history of the Universe, but the evidence is conflicting — the cosmic-microwave-background experiment *WMAP* (exploring the echo of the Big Bang) indicates this happened early in history (at redshifts much higher than 6, possibly redshift 10 to 20), but studies of the most distant quasars suggest that the transition occurred close to redshift 6. Our results were highlighted by a NASA press conference at the Space Telescope Science Institute (Baltimore) in 2004 September.

We have recently built on this work using the new infrared *Spitzer Space Telescope* to study these objects at longer wavelengths, where the light from older stars is detectable (*Hubble* only examines the youngest stars). Working with my new graduate student, Laurence Eyles in Exeter, we found that some of these redshift-6 galaxies had large stellar masses (1/4 of $L_\star$ today) when the Universe was only 900 Myr old, and two galaxies had large Balmer spectral breaks, indicating most star formation occurred 200–400 Myr before and pushing the formation redshift back to around 10 (*MNRAS*, **364**, 443, 2005). This is a crucial result in the context of the re-ionization of the Universe — an epochal event which may have been caused by the intense ionizing radiation from early star formation. Future telescopes and instruments should be able to test this hypothesis directly through sensitive infrared spectroscopy; I am the UK representative for the *NIRSpec* instrument on the *James Webb Space Telescope* (the successor to *Hubble*, due for launch in 2014), which has this capability. We are already exploring the first billion years of history, and the next few years should be even more exciting as we push even further back in time, searching for the early stages of star formation in galaxies — 'first light' in the Universe.

The President. Our time's slipping away a bit, but I think if there are one or two quick questions or comments we shall take them.

Rev. G. Barber. Is there an age problem at $z = 6$ in terms of iron features in galaxies and quasars?

Dr. Bunker. Well, very nicely, you've enabled me to show the slides that I skipped over. [Laughter.]

The President. Not all of them?

Dr. Bunker. No, not all of them. [Laughter.] In fact, we made another discovery when we looked at these galaxies at longer wavelengths, at the rest-frame optical, using the *IRAC* camera on *Spitzer* sampling beyond 3·5 microns — above the Balmer/4000 Å break in the rest-frame. We found that some of our spectroscopically-confirmed $z \sim 6$ galaxies actually had Balmer decrements, a factor of two brightening in flux density (f_ν) going from the

HST–ACS and *NICMOS* and the ground-based *K*-band to the *Spitzer* data points above the Balmer break. This is essentially an age diagnostic; there are degeneracies in there, but it gives you a clue as to the age of the galaxies. We discovered that these have significant stellar masses and ages of a few 100 Myr. Now, you're probably thinking that a few hundred Myr is no big deal, we know of globular clusters today which are 13 Gyr old; but this galaxy at $z \approx 6$ is $<$ 1 Gyr after the Big Bang, so it's pushing the formation epoch of these galaxies out to a redshift of $z = 10$, so there's a lot left to play for.

Rev. Barber. In terms of iron features?

Dr. Bunker. Are you talking about the quasars?

Rev. Barber. Yes.

Dr. Bunker. That's another indirect piece of evidence that there was a lot of star formation early on.

The President. Is there one other quick question?

Professor M. Rowan-Robinson. You showed a star-formation history from your earlier work at redshift $z = 1$–2 and so on. Do you have direct estimates of the dust extinction for those galaxies, because if you have that star-formation history you would expect a higher extinction at redshift $z = 1$?

Dr. Bunker. For the low-redshift $z \sim 1$ work, we do correct for dust extinction. Now, that's somewhat uncertain because we use the Balmer decrement, and also the comparison of the rest-UV continuum with Hα. If you have totally obscured star formation, of course, it doesn't contribute. In fact, we are hoping to use the *FMOS* instrument on *Subaru*, which is the successor to *CIRPASS*, to work down the luminosity function and also, perhaps, to address the contributions of low-luminosity and highly-obscured galaxies to the star-formation budget.

The President. I really think we ought to move on in the interests of time, but thank you very much again, Andrew, for a very enjoyable talk. [Applause.]

Our next speaker is Rob Fender from Southampton. His talk is entitled 'The balance of power: how black holes accrete'.

Professor R. Fender. [No summary was received at the time of going to press. The speaker described how empirical patterns have been established of the coupling of accretion and ejection in neutron-star and black-hole X-ray binaries. The speaker summarized the general pattern of behaviour observed in accreting black-hole binaries, and described a model published by him and his collaborators which appears to explain an observed empirical relation between the X-ray luminosity and spectral hardness of such systems.

During periods of relative quiescence, which can last several decades, galactic black-hole X-ray binaries are faint, and are observed to exhibit hard X-ray spectra. The speaker showed how the 'hard' state is always accompanied by steady flat-spectrum radio emission, the probable signature of a self-absorbed steady jet. Jets are produced by nearly all X-ray binaries, in both neutron-star and black-hole systems, and they appear to carry away a large fraction of the available accretion power. Where there exists a simultaneous measurement of hard-X-ray emission and radio flux for quiescent systems, it can be shown that there appears to be a 'universal' correlation between the two. It can be inferred from this relation that at a sufficiently low X-ray luminosity, the jet power would be expected to dominate over the X-ray emission, and this indeed appears to be what is observed.

According to the model developed to explain the observed behaviour, when the X-ray emission of a system brightens, perhaps at the onset of some instability, the steady jet becomes more powerful, while the spectrum remains hard. Above some threshold luminosity, about 1% of the Eddington limit, the X-ray spectrum becomes softer, perhaps because the accretion disc moves in to the point at which it has a dramatic effect, for example, on the corona. It is possible that at this point a faster jet is ejected by the source and runs into the back of pre-existing ejected material, causing an internal shock which is optically thin. The jet switches off and no radio emission is seen while the source is in the soft state, the soft-X-ray emission being produced by a radiatively efficient accretion disc. After about a month, the source slowly lowers in X-ray luminosity and hardens again as the steady jet re-ignites and the source settles back down in quiescence.

The speaker examined the question of where the accretion power has gone when the source is in the quiescent state. He described how a sample of neutron-star X-ray binaries can be compared with a black-hole sample: advection of energy across the event horizon can occur in black-hole X-ray binaries, unlike in the neutron-star binaries. For neutron-star systems, and black-hole systems in the brightest states, the X-ray luminosity appears to be efficiently generated by accretion; but for the fainter black-hole systems, their luminosity falls well below that predicted from the accretion rate, consistent with the results of advective models. Interestingly, the black holes in low-luminosity active galactic nuclei appear to follow a very similar pattern. This is the strongest observational evidence to date that these black-hole systems radiate very inefficiently.

It was concluded that for black holes accreting at low rates, the power going out in the jet is always much more than the radiative output. The best (but uncertain) estimate of how much energy crosses the event horizon is that it is similar to that in the jet, with a very small amount of energy being emitted radiatively. The speaker considered that there have now been established clear patterns of disc–jet coupling for relativistic accreting systems: that is, clear empirical patterns have been found for neutron-star and black-hole binary systems; nearly all accreting systems produce jets most of the time, and these jets carry away about the same power for a given accretion rate. At low accretion rates, accretion on the black hole is radiatively inefficient, with about half the accretion power appearing in jets, and about half advected across the black-hole event horizon.]

The President. Any questions or comments? I think everybody's stunned — you've convinced them all! Well, thanks very much again. That was another really excellent talk. [Applause.]

Our final speaker, since Steve Smartt is unable to be with us, is Steve Tobias from Leeds and, coming a bit closer to home, his talk is on 'Solar and stellar dynamos: turbulence, rotation and magnetic fields'.

Dr. S. M. Tobias. In this talk I shall address the important and controversial issue of the generation of magnetic fields in stars. I shall give particular emphasis to the observations of the solar magnetic field and to its explanation in terms of generation by a hydromagnetic dynamo.

The well-known eleven-year sunspot cycle is a manifestation of solar magnetic activity. Sunspots are cool dark patches on the solar surface which are associated with strong magnetic fields. They appear at mid-latitudes at the start of a cycle and migrate to the equator. Solar magnetic activity has been recorded since the early 17th Century following the invention of the telescope (although earlier observations were made by the ancient Greeks and Chinese). The activity record can, however, be extended back thousands of years using proxy data. These data (which include ^{10}Be records in ice-cores and ^{14}C records in tree rings) indicate that solar magnetic activity has 'switched off' for periods of up to 200 years in the past. One such period of reduced activity, called the Maunder minimum, occurred in the 17th Century, when astronomers would report excitedly on any sunspots which appeared. These periods of reduced activity are known as 'Grand Minima'. It is intriguing to note that the Maunder Minimum of solar magnetic activity coincided with the 'Little Ice Age' — a period when the global terrestrial temperature was reduced. Indeed the possible links between solar variability and climate is a field of active research.

Explaining the origin of the Sun's magnetic field is the fundamental problem of solar magnetohydrodynamics. This field underlies all solar magnetic phenomena such as solar flares, coronal mass ejections, and the solar wind, and is responsible for heating the solar corona to such high temperatures. These phenomena may all have important terrestrial effects, causing severe magnetic storms and major disruption to satellites, as well as having a possible impact on the terrestrial climate.

The Sun is just one star of many that exhibit magnetic activity. Observations of other stars with differing properties (spectral types, ages, and rotation rates) show that the Sun's behaviour is not unique (although there are some stars which do exhibit cyclic behaviour) and that other modes of magnetic variability are possible. These observations are crucial for calibrating any proposed theory for solar magnetic activity.

Solar (and stellar) magnetic activity is believed to be the result of a hydromagnetic dynamo. In an astrophysical setting, such as the highly ionized plasma in the Sun, the dynamo arises as the result of fluid flows in the plasma that may drive currents, and these in turn may generate magnetic fields. A successful dynamo is one where the flows are large enough (and sufficiently complicated) to overcome the action of ohmic dissipation, which leads to the decay of field. The dynamo problem is complicated; an adequate explanation requires the self-consistent solution of the equation for the generation of magnetic field (the induction equation) together with that for the evolution of the fluid velocity (the Navier-Stokes equation). Moreover, these equations need to be solved at extreme values of the control parameters — a parameter régime in which the flow is turbulent and the magnetic field is highly intermittent and complicated.

Although much progress has been made in advancing our understanding of the processes that lead to dynamo action, we are not yet at the stage where successful 'brute force' calculations of the relevant equations can lead to realistic dynamos. Much of our understanding relies on the theory of mean-field electrodynamics, which is a turbulence theory that describes the evolution of the large-scale magnetic fields whilst parametrizing the dynamics of the small scales in terms of transport coefficients. This approach has been very successful — almost too successful — with mean-field models being capable of reproducing many salient features of solar and stellar fields. However, this approach leaves many questions unanswered and, with many unconstrained parameters, requires careful examination. A great concern is that the formulation necessarily ignores the physics that underlies the small-scale-field evolution which leads to the generation of the mean fields. Though mathematically elegant, this is somewhat unsatisfying and I maintain that it is only by examining the underlying physics that we shall gain a complete understanding of the process that leads to magnetic-field generation in the Sun and other stars.

The President. Thanks, Steve. Are there any questions or comments on that?

Mr. H. Regnart. Can you tell us if there's any chance of the approximately 200-year period riding to our rescue at a time when our unquestioned good faith in the fossil-fuel industry is putting us in peril?

Dr. Tobias. I would love to be able to say 'yes', and I would love it if we went into a new Maunder minimum, that would be just great — I'd be rich and famous and go on radio chat-shows and things like that! [Laughter.] There's no sign of it as yet, but if a Maunder-type minimum did occur, it would certainly help; but I still think we should stop pumping greenhouse gases into the atmosphere.

Mr. Regnart. Oh yes, but temporally, are we due one?

Dr. Tobias. We are due one. In fact, we're overdue one, yes.

Mr. M. F. Osmaston. About twenty years ago, there was a paper in *Nature* discussing the variation in length of the sunspot cycle and its relationship with the conjunction of Jupiter and Saturn being on the same side of the Sun and displacing the effective centre of rotation. It always seemed to me rather sensible because what this actually means is that the angular velocity of the surface of the Sun is not the same, as the centre of rotation of the whole system is not the centre of the Sun anymore, and this might have an important effect on bringing, shall we say, the sunspots to the surface. Has there been any progress on this?

Dr. Tobias. I think I can say that there hasn't been very much progress on this. The differential rotation maps that I showed you are azimuthally averaged, so they wouldn't show any of the effects that you're talking about. I have to say I'm not aware of very much progress, but that's not really something I've looked into.

Mr. Osmaston. I think that the paper was not actually talking about the displacement but the rate of change of displacement being correlated with the sunspot interval.

Dr. Stanley. Was the Maunder Minimum an integral number of sunspot cycles, so it was still in phase before and after, or was there a phase change?

Dr. Tobias. The length of the solar cycle seemed to change slightly during the Maunder Minimum, but it was an integral number. It's very hard to infer the dates, but it does appear that there was an integral number and everything just carried on afterwards as before.

The President. Does the negative α value mean that you're generating a field that's opposing the one you started with, as a simple interpretation?

Dr. Tobias. In terms of the models, there is currently a conundrum as to why the sunspots go from mid-latitudes towards the equator, as opposed to towards the poles, and that's because α is of the wrong sign. You would find that if you used the opposite sign of α, you would actually get the correct migration of the sunspots, but having said that, I still don't think it's a good reason for using the theory.

The President. I think that's about the right point to stop. Thanks very much again, Steve. [Applause.]

I think we've been wonderfully privileged to have four excellent talks: all our speaker have done really well and are to be congratulated, not only on the awards, but on the way they have presented their material to us today. [Applause.]

This meeting will now close. I've already mentioned the meeting during the NAM when we will have a discussion primarily about PPARC, as well as some science presentations. The next A&G Ordinary Meeting of the Royal Astronomical Society is here on Friday, May 12.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2006 April 6th at $14^{h}\ 00^{m}$
in the Rattray Lecture Theatre, University of Leicester

K. A. Whaler, *President*
in the Chair

The President. Good afternoon, ladies and gentlemen. We've got a busy programme this afternoon, so while the last few people are settling down I'll just make a couple of brief announcements.

As most of you will have heard this morning, there is a slight change to the advertised programme, in that the talk that was scheduled for after tea is now going to be given before tea, after the medals are presented. We'll now be able to take tea at about $15^{h}45^{m}$ and return a little bit later for the PPARC discussion session, including the implications of the budget announcement. We think that makes a slightly more logical programme and still leaves plenty of time for discussion.

The first item on this very busy agenda is to invite Professor Joe Silk from Oxford to give the Darwin Lecture that he presented earlier in the year at one of the monthly meetings of the Royal Astronomical Society in London. To capture a larger audience here, I'd like to invite him to come forward again and give his George Darwin Lecture, 'The dark side of the Universe'. [Applause.]

Professor J. Silk. [A summary of this talk is expected to appear in a future issue of *Astronomy & Geophysics*.]

The President. Thanks very much, Joe. We're a little bit short of time, and as you've probably noticed, our speaker's voice is beginning to croak a little bit, but if there are one or two pressing questions we'll certainly squeeze them in. No? Well, I think you've convinced them all today! Let's thank Joe once again for a really stimulating talk. [Applause.]

We are departing from tradition in two ways this year: firstly, by presenting the majority of the Society's medals and awards at this meeting, since a number of the recipients are here today; and secondly, for those of you who have attended presentations in Burlington House, by giving the prize-winners an opportunity to say a few words. For that reason, I'm going to *précis* some of the citations to give the recipients a chance to respond; so if you're a recipient and don't recognize my garbled description of what you've done, I do apologise.

I'm going to start with the Herschel Medal, which will be presented to Professor Govind Swarup, former Director of the National Centre for Radio Astrophysics at the Tata Institute for Fundamental Research, and currently Honorary Scientist at NCRA. He was also a member of the team conducting the International Review of UK Physics and Astronomy, an exercise that took place late last year, and some of you may have met him when he was visiting your institution.

Professor Govind Swarup, the 'father' of Indian radio astronomy, who founded what is now the National Centre for Radio Astrophysics at Pune, has made a number of breakthrough discoveries in topics ranging from solar radio bursts to narrow bridges in the lobes of radio galaxies. He is currently observing galaxies in the very early Universe, using the highly redshifted 21-cm line of neutral hydrogen.

Govind Swarup's best-known achievements to date are two major telescopes. The Ootacamund (Ooty) radio telescope is a cylindrical paraboloid

530 m by 30 m. Such designs are normally most useful for transit and occultation studies but its ingenious location, elongated parallel to the Earth's axis on a hill with the same slope as the geographical latitude (11 degrees), allows it to track radio sources for up to 9·5 hours whilst only rotating about one axis. The construction of the *Ooty Radio Telescope* prefigured the 'Indian Rope Trick' design used for the *Giant Metrewave Radio Telescope* (*GMRT*) near Pune. This consists of thirty 45-metre dishes with reflecting surfaces made up of welded wire mesh, tensioned and adjusted using stainless-steel-rope trusses. This is not only intrinsically very economical but solves three of the greatest problems for large radio dishes — moving a heavy weight, compensating for deformations (less of a problem with light dishes anyway), and wind loading. He is also an important mover behind the ambitious international *Square Kilometre Array* project.

Professor Swarup has made many other contributions to science in general and to science education in India in particular. This has been recognised by his election as a Fellow of all the National Science Academies in India, of the Third World Academy of Sciences, and of the Royal Society of London. I'd like to invite him to come forward to receive his medal. [Applause.]

Professor G. Swarup. I am greatly honoured by this award that the Royal Astronomical Society has given to me. My primary motivation for conceiving and building these instruments has been for cosmological studies using observations of distant radio sources. This aspect is very difficult to highlight after Joe Silk's lecture. Back in 1963 there was a great controversy between the Big Bang and Steady State theories for the origin of the Universe. To test their predictions, we measured angular sizes of about 1000 radio sources by occultation, using the *Ooty Radio Telescope* that was specially designed for the purpose. We followed the Moon every day for ten years and we used to say we were the only professional lunatics in the world. [Laughter.]

The main motivation for conceiving the *GMRT* was to search for neutral-hydrogen clouds in protoclusters at high redshifts. We intended to find whether there is hot dark matter or cold dark matter in the early Universe, so we built the largest collecting area that we could afford with the available budget of 16 million dollars.

I would like to go back to the time when I was a student. Sir C. V. Raman, the Nobel Laureate, came and told us that when you collect a Nobel Prize, it's for 99 per cent perspiration and 1 per cent inspiration. So I said, "I come from a hot country, perspiration is not a problem". [Laughter.] I went to Australia in 1953–55 at the time radio-astronomy discoveries were being made and that gave me the inspiration. Thank you so much. [Applause.]

The President. Our second medal is the Chapman Medal, which will be presented to Professor Steven J. Schwartz of Imperial College London. Steve Schwartz is currently Professor of Space Physics at Imperial College. He is awarded the RAS Chapman Medal in recognition of his pioneering work in solar–terrestrial physics, and in particular associated with the Earth's bow shock. The bow shock is the prototype of all collisionless shocks throughout the Universe, and its study, possible from Earth-orbiting spacecraft, is critical in understanding processes such as particle heating and particle acceleration ubiquitous at all shocks.

Steve's seminal contributions have focussed on the so-called quasi-parallel shock, believed to be responsible for energetic-particle production. Twenty years ago it was believed such shocks were turbulent, but how they worked was not known. Using an extremely limited sample of spacecraft data, in two pioneering papers Steve developed a global picture of those shocks that is widely accepted today as the correct one. This involved replacing the idea of a quasi-steady shock with one where the shock was comprised of a patchwork of large-amplitude magnetic-field fluctuations that formed a three-dimensional ‘patchwork’ in space. The ‘short large-amplitude magnetic structures’, known as SLAMS, were predicted to be quite small scale (1000 km or so), to grow quickly (a few seconds), and to be responsible for significant plasma heating, and were likely to play a major rôle in the initiation of particle acceleration.

This early work was highly insightful and in the 1990s the numerical-modelling community took the hint, and showed that, at least theoretically, the SLAMS idea made sense. The true vindication of Steve’s work has come in the last few years when multi-spacecraft data from the *Cluster* mission have confirmed both the generic idea of SLAMS as being central to understanding the parallel shock, as well as many of the more detailed predictions about size and plasma heating he made. A good summary can be found in papers in the recently published *Space Science Review*, Volume **118**.

Steve has contributed important work to many other areas of space-plasma physics, especially concerning waves and instabilities in space plasmas that have revealed the complex physics at play. But it is especially appropriate that a medal named after a true pioneer of space-plasma physics is awarded for work that revealed the physics in probably the most important type of space-plasma structure, the collisionless shock.

I gather that Steve may not actually be here. [Laughter.] Well, let’s give him a round of applause. [Applause.]

The next medal is the Jackson-Gwilt Medal, presented to Dr. Keith Taylor of Caltech Optical Observatories, California Institute of Technology.

Keith Taylor, now at Caltech after a distinguished career at the Royal Greenwich Observatory and the Anglo-Australian Observatory, is awarded the Jackson-Gwilt Medal for the pivotal rôle he has played in developing world-class instrumental facilities for UK astronomers. The instruments he has built and commissioned have inspired a new generation of observers and have been responsible for major discoveries in optical astronomy.

In his early career, Keith developed and exploited *TAURUS*, the world’s first scanning Fabry-Perot imaging spectrograph, an improved version of which was chosen as a first-light instrument for the *William Herschel Telescope* (*WHT*). Keith then considered the concept of multi-object spectroscopy and worked with the late Charles Wynne to design the *Anglo-Australian Telescope* (*AAT*) *Low Dispersion Survey Spectrograph* (*LDSS*). He supervised the manufacture of this complex instrument and overcame technical challenges in faint-field acquisition, data reduction, and observing strategies. *LDSS* was the forerunner of a new generation of spectrographs now in use at all major observatories.

Undoubtedly Keith’s greatest achievement to date has been the ambitious *Two-degree Field* (*2dF*) project, which we heard a little about in the previous lecture. For more than eight years during his tenure at AAO, this venture was his responsibility as Project Scientist, and later also as Project Manager. He successfully oversaw the production and commissioning of the most complex wide-field corrector ever manufactured at the time, and supervised engineers in the assembly of a double-buffered, 400-fibre positioning system feeding two spectrographs. The instrument gave a new lease of life to one of the world’s most successful telescopes. As we have heard, *2dF* has garnered over 250 000 galaxy and 20 000 quasar redshifts.

With each of these projects, Keith's hallmark has been his unique ability to keep a keen eye on the scientific motivation whilst pursuing innovative ideas in optics, software, and engineering. He is admired and respected by technicians, observatory directors, and astronomers alike. His breadth of achievement is possibly unequalled by any other astronomical instrument builder.

I'd like to invite Keith to come forward and receive his medal. [Applause.]

Dr. K. Taylor. This is, of course, needless to say, a great honour, but more particularly, it's a greatly undeserved honour. [Laughter.] Really, I would like to give credit where it's due, because all I did was to conceive a few ideas and persuade people along the way that I had something of an idea, and then I was sort of shepherded and led along by superb engineers and superb support people. With *2dF* in mind, what's really striking from my perspective is the fact that *2dF* could only have been done at the *AAT* — and I mean the *AAT*, rather than the AAO; the *AAT* staff were so superb and dedicated in making this a reality, whereas I just spawned the idea. Actually bringing it to fruition was a terrific achievement for them. I hope they enjoyed the ride. Thank you very much. [Applause.]

The President. Now, some of you will know that we traditionally make an award for service to astronomy and that we also make an award for service to geophysics. Unusually, this year we are making a joint award for service to both astronomy and geophysics, to Dr. Brian Marsden of the Harvard–Smithsonian Center for Astrophysics. Brian has informed us that this is the 50th anniversary of his first attendance at an RAS meeting, which was one month after he was confirmed as what was then called a Junior Member.

Brian Marsden has spent most of his professional life employed at the Smithsonian Astrophysical Observatory in Cambridge, Massachusetts, although he was born in Cambridge, England. He received his undergraduate education at Oxford University (BA, 1959; MA, 1963) and his research training at Yale University (PhD, 1966). Starting with a dissertation on the orbits of the Galilean satellites of Jupiter, Brian has been an internationally recognized figure over the past four decades in fundamental research on the celestial mechanics and astrometry of small Solar System bodies, including minor planets, comets, and natural satellites.

Specific areas in which he has made vital contributions to the subject include the non-gravitational forces that disturb the theoretical 'Newtonian' motion of comets, the constitution of the Kreutz group of Sun-grazing comets (one sub-category is now universally termed the 'Marsden Group'), and procedures for deriving useful orbital information from minimal positional data. These have proven to be extremely useful for estimating the paths of the numerous trans-Neptunian objects discovered since 1992. His analysis of often-sparse observations has led to the recovery of several 'lost' comets and asteroids; for example, in a paper published more than a decade beforehand, he foresaw the return in 1992 of Comet Swift-Tuttle, which has the longest period of any comet ever successfully predicted. He is also co-editor of the standard *Catalogue of Cometary Orbits*, sixteen editions of which have been published since 1972, and has been Director of the International Astronomical Union's Minor Planet Center since 1978. The monthly *Minor Planet Circulars* and the more recent daily electronic updates disseminate positional and orbital information about comets, asteroids, and the satellites of the major planets to the worldwide astronomical community.

He has also made significant contributions to astronomy generally, particularly as Director of the International Astronomical Union's Central Bureau for Astronomical Telegrams from 1968 to 2000, and is now Director Emeritus. In this capacity he was responsible for the timely dissemination to the worldwide astronomical community of information about transient objects and events in all areas of astronomy and astrophysics. He has also served in numerous capacities within the IAU, being at different times Vice-President and President of several IAU commissions, a member of the IAU Special Nominating Committee, and a member and secretary of various IAU panels such as the Committee on Small-Body Nomenclature and the Working Group on Near-Earth Objects. Within the American Astronomical Society, he has held several substantial positions, such as Chairman of the Division on Dynamical Astronomy, and from 1987 to 2002 he was Associate Director for Planetary Sciences at the Harvard–Smithsonian Center for Astrophysics.

In 1974 the IAU named minor planet (1877) Marsden in his honour. Amongst his other awards we note the Merlin (1965) and Walter Goodacre (1979) Medals of the British Astronomical Association, the University of Arizona's George Van Biesbroeck Award for services to astronomy (1989), the Camus-Waitz Medal of the Societé Astronomique de France (1993), the American Astronomical Society Dirk Brouwer Award for research in dynamical astronomy (1996), and the Lacchini Prize of the Unione Astrofili Italiani (2001). In 2003 he was the first George Alcock Memorial Lecturer of the British Astronomical Association. In summary, there are surely few Fellows of the Royal Astronomical Society who have not benefitted in some vital way from the energetic and excellent work of Brian Marsden over the past forty years, nor heard of his name. We are delighted to present him with the Society's Medal for Service to Astronomy and Geophysics. [Applause.]

Dr. B. Marsden. Thank you very much; I do appreciate this. It was rather unexpected! Yes, it was 50 years ago this month that I attended my first meeting of the Royal Astronomical Society. I don't suppose there are too many people here now who were here then. [Laughter.] I suppose some of these things came my way: it was recommended I move to the USA in 1959 and that did get me into running the Central Bureau for Astronomical Telegrams because Fred Whipple had volunteered to take it over from Copenhagen, where it had been for many years. Then, with the Minor Planet Center, that sort of came my way too. I felt that in working with these organizations in service to astronomy, I could try and get a little bit of research out of it at the same time, like with the Sun-grazing groups and the trans-Neptunian objects. One thing that I did there was say that there would be quite a large number of objects that are in 2:3 resonance with Neptune — the Plutinos, Pluto being the first one — and this worked out really rather well. I was surprised that the President mentioned the IAU Nominating Committee [addressing Professor Swarup], because I think you were on the committee the same time I was, in about 1994! Thank you again for giving me this award. I do appreciate it. [Applause.]

The President. A few years ago, the Society instituted new awards. They are called the Fowler Awards, after the funding source, which are awarded specifically to people in the early stages of their career. The Fowler Award for Astronomy this year is presented to Dr. Serena Viti of University College London.

Serena obtained her PhD in 1997 after computing molecular-line opacities for use in modelling the atmospheres of cool stars. She then took up a postdoctoral position at UCL, switching topics to work on star-forming regions. Her science interests are broad and developing, and she already has approaching 50 refereed journal publications to her credit. She maintains a very active interest in cool stars, involving both observational and theoretical activities, and among other topics has worked on the time-dependent chemistry of hot cores, Herbig-Haro objects, water-vapour lines in sunspots, brown dwarfs, and astrochemistry in the high-redshift Universe.

Her success in research was marked by the award of a PPARC Advanced Fellowship in 2003, and she has lecturer status at UCL. Even at this early stage of her career she is playing a leadership rôle: whilst working at the CNR institute for space research in Rome, IFSI, she began an involvement with the forthcoming *Herschel* space mission, and she is currently the Team Coordinator of the *Herschel* Preparatory Science Chemical Modelling Group. She has sat on the Council of the RAS, and has conducted a number of outreach activities, most notably her RAS-supported 'Stars 'R' Us' exhibit at the Royal Society Summer Exhibition.

Can I invite Serena to come forward? [Applause.]

Dr. Serena Viti. I really just want to say thanks very much to the RAS for this very nice award. Thank you. [Applause.]

The President. The corresponding Award for Geophysics is presented to Dr. Clare Parnell of the University of St. Andrews.

Clare is an outstanding young solar physicist, one of the best of her generation, who has already made an enormous international impact on our fundamental understanding of the heating of the solar corona. This has involved a rare combination of analytical, numerical, and interpretive pieces of research, all of characteristic high quality. Clare completed her PhD thesis in 1994 and was immediately awarded a PPARC Fellowship. Following a year in the USA, she was awarded the RAS Sir Norman Lockyer Fellowship in 1998 and a PPARC Advanced Fellowship in 2002, before being appointed as a lecturer at St. Andrews University. She is an excellent rôle model for young women scientists, having had two periods of maternity leave and continuing to make major advances whilst looking after two children.

The enigma of coronal heating remains one of the major unsolved problems in solar and stellar physics. How are the three different parts of the corona (namely, X-ray bright points, coronal loops, and coronal holes) heated to at least a million degrees by comparison with the temperature of the solar surface of only 6000 K? The magnetic field is ultimately responsible, but the actual mechanism or mechanisms are a matter of great debate. The efforts of Dr. Parnell have in fact solved part of the coronal-heating problem and have clarified many aspects of it, so that it is expected that future space observations and theoretical studies should solve the remaining parts.

Clare has worked on many inter-related aspects of this problem, both observational and theoretical, but several are of particular note. Firstly, she developed a new model in her thesis, which explains all the main features of X-ray bright points as being due to the interaction of coronal magnetic fields, driven by the motion of their underlying photospheric footpoints. It is now widely accepted as the solution of part of the coronal-heating problem. X-ray bright points are tiny, hot, bright structures that appear all over the solar corona.

Secondly, she has made a major contribution to the development of 3D reconnection by categorizing the nature of 3D magnetic null points and the different ways that they can evolve. Such points are crucial locations in a complex magnetic field where reconnection and topology changes can readily occur.

Thirdly, she has investigated how nanoflares may be responsible for heating the corona by using, for the first time, a rigorous statistical analysis of the observations. In this ground-breaking paper she has laid the foundation for the next generation of observational and interpretive papers on nanoflares, by laying out just what one can and cannot deduce from the observations.

It is in view of these major contributions to solar physics that she is to be presented with this Fowler Award. [Applause.]

Dr. Clare Parnell. I'd just like to say thank you very much to the RAS for this award. I'm delighted and rather surprised; I'm not entirely sure what I've done to deserve it. I'd also like to thank both PPARC and the RAS, who have supported me financially, as you've heard, through a series of fellowships over the last decade. I hope they will continue to support me and my postdocs and PhD students over the next few decades! [Laughter.] Thank you. [Applause.]

The President. The next of our awards is the RAS Blackwell Prize for best thesis, and for that I'm going to hand over to the sponsor from Blackwell, Sue Corbett.

Mrs. Sue Corbett. I feel it has been one of the most important elements of collaboration between Blackwell Publishing and the Royal Astronomical Society that we have an opportunity to recognize outstanding young scholarship, and so it gives me very great pleasure to award the Royal Astronomical Society Blackwell Prize 2005 to Dr. Philip Livermore of the University of Leeds for his thesis, 'Magnetic stability analysis for the geodynamo'. [Applause.]

The President. Finally in this section, we also have a sponsor for the poster prizes here at the NAM, and that is Cambridge University Press. We must say a special thanks to our three judges, who had, I'm sure, a formidable task running around looking at all the posters: they are Gordon Bromage, Margaret Penston, and Richard Jameson. Before I announce the winners, maybe we could give them a round of applause. [Applause.]

The President. There are two commendations, a couple of runners-up, and one prizewinner. First, the commendations go to Cheryl Hurkett from the University of Leicester, 'Finding lines in *Swift* X-ray data: the Monte Carlo way', and Daniel Brown from Liverpool John Moores University, 'Horizon astronomy in the Ruhr area'. So can we give them a round of applause? [Applause.] The runners-up each receive a prize certificate and a £50 book token from Cambridge University Press. If they are in the audience, I'd invite them to come up receive that. They are Michelle Supper from Leicester, 'The soft-X-ray background of the Milky Way', and Vanessa McBride from Southampton, 'A study of the cyclotron-line features in two high-mass X-ray binaries'. [Applause.]

The winner is David Radburn-Smith from Durham, 'Modelling the Great Attractor'. He receives a £100 book token. [Applause.]

We're extremely grateful to Cambridge University Press for sponsoring that.

Just before we turn to the final talk of this session, prior to the tea break, it's my great pleasure to express the Society's thanks to Leicester University for organizing the conference, including sponsoring the welcoming reception. Names I'd like to mention in particular are Vice-Chancellor Professor R. Burgess and members of the local organizing committee: Professor Robert Warwick, who is the Chair, Dr. Paul O'Brien, Dr. Tim Roberts, Professor Mike Watson, Dr. Duncan Law-Green, Mr. Stuart Poulton, Dr. John Pye, Mr. Stuart Lyon, Dr. Richard West, Dr. Paul Dobbie, Dr. Simon Vaughan, Dr.

Gillian Butcher, and the conference secretaries, Mel Kidby and Pat Russell; so let's give them a round of applause. [Applause.]

The other organization that I must thank is our largest sponsor, PPARC. [Applause.]

A date for your diaries: the next National Astronomy Meeting will be held at the University of Central Lancashire, 2007 April 16–20.

Just before the tea break we have an extra talk, which some of you will have seen in the programme was originally scheduled for after tea, and that's going to be by Dr. Timothy Brown from Las Cumbres Observatory Global Telescope, 'Keeping astronomy in the dark around the clock'.

Dr. T. Brown. The Las Cumbres Observatory Global Telescope (LCOGT) is a new, privately-funded observatory that intends to construct and operate a world-wide network of 2-metre telescopes configured to study time-varying astronomical phenomena. Its goals are twofold: to organize collaborations and to make facilities available so that such studies may be undertaken, and to use astronomical research to inspire critical thinking and technical excellence among young people.

At present, the LCOGT facilities consist almost entirely of telescopes and organizations that are well known to astronomers in the UK. The telescopes are *Faulkes-North* (on the island of Maui) and *Faulkes-South* (at Mt. Stromlo Observatory in Australia). The operation of these telescopes is handled by the Astrophysical Research Institute at Liverpool John Moores University, and the educational programme is centred at Cardiff University. All of these elements were first conceived and built by the Dill Faulkes Educational Trust (DFET), and we newcomers at LCOGT are hugely grateful to DFET for providing such a solid foundation for continued growth. The growth itself will be enabled by Wayne Rosing, LCOGT's founder, funder, and technical director, and by his TABASGO Foundation, which is endowed at a level that should permit LCOGT to operate and renew itself for perhaps twenty years.

The projected capabilities of the LCOGT are driven by the requirements of time-domain astrophysics. Our aim is to provide two complete, longitudinally-distributed rings of research telescopes, one in the Northern Hemisphere, and one in the south. With such a network of similar telescopes, it will be possible to follow the time variations of single objects for days or weeks with a high duty cycle. We will also be able to assure that wherever and whenever a short-lived event occurs (at least if it is reasonably far from the Sun's position), LCOGT will have at least one telescope that is in the dark and able to observe it. Finally, we intend to operate the LCOGT network as a single distributed instrument, with the telescopes being run robotically and with the observing schedule chosen to be responsive both to overarching scientific goals and to conditions of the moment.

Many domains of astronomy are natural targets for a global network, ranging from the history and structure of our Solar System, as revealed by trans-Neptunian objects, to the history and structure of the Universe, as revealed by distant supernovae and gamma-ray bursts. In between, one might profitably study the photometric or radial-velocity signatures of extrasolar planets, or magnetic activity and rotation in stars of varying ages, or probe physical processes in stars *via* their normal-mode pulsations, or the distribution of matter near supermassive black holes *via* reverberation mapping. All of these studies are both exciting and possible for a network of the sort LCOGT aims to be. Likely all will be supported to some degree, though it is also a safe bet that for any given year, the bulk of observing resources will be dedicated to a small minority of these topics. Concentrated effort is often rewarded by progress, so LCOGT will attempt to concentrate its resources to whatever extent makes sense.

Besides the *Faulkes Telescopes*, LCOGT's resources now include ownership of Telescope Technologies Limited (TTL), which is designing the telescopes that will fill out the 2-m network. It also has headquarters, workshop facilities, and a staff in Santa Barbara, California. Presently the staff number about a dozen, including one scientist. When hiring is complete, the total Santa Barbara staff will be about double that size, including around four scientists; in addition, there will be a modest number of short-term scientific positions, including graduate students and postdoctoral fellows. This staff will maintain existing telescopes and install new ones, develop and deploy instruments appropriate to the observatory's scientific goals, and (almost always in collaboration with scientists outside LCOGT) do research, write papers, and pursue all of the absorbing tasks that make working in astronomy worthwhile.

In addition to the network of five to seven 2-metre telescopes dedicated mostly to research, LCOGT intends to field a worldwide network devoted to education and consisting of perhaps 30 telescopes with 0·4-metre aperture. Finally, an intermediate network is planned, consisting of about ten telescopes of roughly 1-metre aperture. These will be available for educational projects with needs that go beyond the reach of the smaller telescopes, and will also be usable as backups for the research telescopes in the case of bad weather or equipment failure. The goal to be achieved with all of this telescopic horsepower is to nurture original, productive, and community-wide research, supporting an inspiring and rigorous educational programme. The educational programme is, at this time, the most developed and experienced segment of LCOGT. Future educational activities will build on this solid base, with the aim of producing a future generation not of astronomers, but of technically capable, scientifically literate citizens who can think critically about the world around them. The existing programme is on its way to doing this *via* live astronomy sessions, astronomy workshops, teacher training, and clubs for science and astronomy. LCOGT's goal is to continue and augment these efforts, especially by providing universal access to plentiful telescope time. LCOGT also funds the former World Year of Physics Speakers Bureau, a clearinghouse for physics and astronomy lectures to college students, by way of a grant to the University of Texas at Brownsville.

Finally, it is worth reiterating the importance of collaborations and of outside use of LCOGT's facilities. If the observatory should attempt to restrict access to its own scientists, it would surely fail in its larger mission. Along with the *Faulkes Telescopes*, LCOGT acquired obligations to provide telescope time, especially to the *RoboNet* consortium. These obligations will all be met, and not merely because doing so is required by contract. If LCOGT is to succeed in advancing the practice of astronomy in the time domain, it must reach out and support excellent and appropriate science. It is doing this now by way of its collaboration with *RoboNet*; as the observatory grows and evolves, it will continue to do so. (I should like to acknowledge here the help and co-operation of my colleagues Stuart F. Taylor, Wayne Rosing, Rachel Mann, John Farrell, and Virginia Trimble.)

The President. Thanks very much, Tim. We do have a minute or two before tea, so if there's anybody who would like to ask a question or make a comment to Tim or the rest of the team, do feel free.

Mr. R. Chapman. Have you any indication of the timescale for the refurbishment of *Faulkes-South*?

Dr. Brown. Second light was three or four days ago — they actually got light through the telescope — after starting the repair cycle about a little over a month ago. Really, it's impossible to predict because all you need is a customs hold on one important thing and you're set back for weeks. But I hope it will be soon, and by that, I mean a month or two. I can't say for certain, but that's the target.

Professor T. J. Ponman. On the educational side I assume that Paul (Roche) is not covering all the schools in the world?

Dr. Brown. Something else I meant to say was that, if you really want to ask questions about the education, what you should do is ask Paul. [Laughter.] But I assume the answer is no.

Professor Ponman. So the serious question, I guess, is will there be a plan to have an international educational programme of the sort that's already under way in the UK?

Dr. Brown. I think there is such a plan and, if I understand Paul correctly, there are already efforts moving forward to do such a thing in Poland and Ireland. Particularly with the Polish, that raises the question of what to do about languages and that's one that is just going to have to be faced up to.

The President. Any more questions? You're all getting a bit shuffly anyway! Thanks very much again, Tim, for a really interesting talk. [Applause.] We will break now for tea. Please be back in half an hour to listen to representatives of PPARC and contribute to the discussion we're having afterwards.

[Tea]

The President. The plan is for Keith Mason to talk for about 20 minutes and then we'll follow that with a panel discussion and hopefully at the end we can formulate a community view. I would encourage anyone with a point of view to ask a question. The Standing Conference of Astronomy Professors (SCAP) met over the past few days to discuss some of these issues and I think it might be worth re-airing some of them. We need a representative view — not just from those in the profession now but for those whose careers might extend over the next 20 years or so.

Professor K. O. Mason. [The speaker started by saying that he suspected there was not going to be enough time to discuss everything. Clearly the biggest issue is the Treasury statement of a few weeks ago concerning the future of PPARC and CCLRC, and that topic would be discussed presently, but first the speaker said he would talk briefly about the PPARC investment strategy. The main issue, to his mind, is what is to be done about the response to the consultation period we are in with respect to the Research Councils (RC).

There is a fixed amount of spending for astronomy from the last spending review in 2004 until the next in at least 2007, and everything must be done within this allocation — exploit current facilities, pay for new facilities, carry out R&D for other projects downstream — and there is never enough money to do all this. The aim is to get as healthy a programme as we can within our limited resources. The review included a first systematic look at PPARC-wide past performance and future impact, and the strategic value of particular facilities and results for existing facilities were interleaved with priorities for planned new opportunities. There is no new money apart from that which was ring-fenced for the Aurora programme in 2004. The review was a robust and thorough process with extensive documentation on each facility — principal investigators either wrote or saw reports and the Science Committee went through all of them in great detail, and expertise was supplemented with someone from the STP community. The Chairmen of the four Advisory Panels on Solar System, Astronomy, Particle Physics, and Particle Astrophysics, were also involved. The criteria for new projects included how important they were, what their strategic priority was, how advanced was the development, what was the track record of those involved, where did the UK fit in, how likely it was to work, and how likely it was to remain within budget.

This process was also carried out for existing facilities, *i.e.*, how scientifically important they were for future PPARC programmes, whether better facilities are available worldwide, the size of the user base, to what extent the UK is the leader in this field, *etc.*, and all were scored with low, medium, or high priority. The outcome was that PPARC will be maintaining planned investment in current ground-based telescopes but, by going into ESO, we agreed to withdraw from them as soon as we could, which turns out to be at the end of the decade. The UK will complete the construction of *Scuba 2*, *VISTA*, *ALMA*, and participate in the next round of *VLT* and *Gemini* instrumentation, seek to participate in the *Dark Energy Survey*, deliver UK contributions to *Herschel* and *Planck* instrumentation and *JWST*, plan for the *Gaia* data centre and *LISA*, and provide R&D effort for *ELT*, *SKA*, and the next-generation X-ray observatory.

This is a pretty impressive programme but it must be stacked against things that cannot be done. In the Solar System area PPARC need to complete and exploit *Solar-B* and *STEREO,* which is launched this year, participate in Aurora, plan to participate in *Bepi-Columbo* and *Solar Orbiter*, and maintain exploitation of *EISCAT*. In the particle-astrophysics area we are involved in *Advanced LIGO*, operation and exploitation of the *Pierre Auger Observatory* in Argentina, and construction of the *CLOVER* microwave-background experiment. We are planning to invest in these in the next three years.

PPARC has been increasing grant levels by 5% per year since 2002 but this cannot be maintained because of the impact it is having on the rest of the programme, so grants will increase by only 1·25% per year over the next few years.

The projects that will be affected will include *Cluster* and *e-MERLIN,* which will not be supported to the level we would have wished, neither will the *Gaia* data centre, *Bepi-Columbo*, and the *Dark Energy Survey*; and the next-generation-telescope facilities will be constrained to a minimum level. The UK will withdraw early from *Veritas*, *SAMNET*, *CUTLASS*, and a collection of STP activities, ionosondes, *SPEAR*, and the *ISO* and *IUE* data centres. There is no current provision for the *UK Dark Matter* collaboration experiment beyond next year.

The Science Committee put these recommendations to Council and Council approved. So how will the changes be managed? There will be active discussions with affected projects and closures will be phased. For ionosondes this will take six months, whilst for *SPEAR* and *CUTLASS* this process will take two years. This is an investment plan based on fixed resources — if more is put into a given project then funding must be withdrawn from something else.

The speaker then moved on to the recent Treasury statement and the possible creation of a Large Facilities Research Council (LFRC). It was part of the budget statement — hence all the secrecy involved with it — and it was uncomfortable for many people, but the good news is that it was the first time that the speaker could remember science being so high on the list of Government priorities. The general landscape for science is extremely good — the Government re-affirmed a ten-year investment strategy to increase the science base and the competitiveness of the UK. Their intentions include creating a new Large Facilities Research Council (LFRC) to replace CCLRC and that part of PPARC dealing with large facilities, and also possibly moving that part of PPARC concerned with grants to EPSRC.

The Government is anxious to invest in science and they see the UK as a world leader, playing a leading rôle in international facilities, but the creation of a single council would be seen as a major requirement. This should be set in context of the overall increased investment in science, but there is a worry: since PPARC started in 1994, total RC spending will have doubled by 2008 but the allocation to PPARC has not kept in step. This is a stark reminder that the community must make a case for some of the new science money to be diverted into astronomy. The Treasury has produced a proposal for consultation, which has a deadline of June 16. The speaker said that in his opinion the LFRC will happen; there appears to be so much momentum already built up behind it, but the details have not yet been thought through so there was still room for sensible input.

Should PPARC grants be moved to another research council? The speaker gave three examples of possible solutions to this question. Firstly, an extreme case in which all PPARC grants, including those for facilities and exploitation, are moved to EPSRC. This would still be the responsibility of LFRC, as will the production of facilities, and EPSRC will have to commission LFRC to do it. EPSRC works in a completely different way to PPARC and it is very hard to get continuity from one award to another. For astronomy, which needs continuity, a good mechanism does not currently exist. Would LFRC be able to operate successfully if it had no responsibility for strategy? This is a serious shortcoming of the scheme and it is not consistent with the vision the speaker set out for the creation of LFRC with all the facilities in one place. The current astronomical portfolio would be transplanted to EPSRC as a programme and funds for the large projects would be ring-fenced. EPSRC is not rich; although it is the largest RC it has other responsibilities.

The second possibility is that the grants that actually fund the construction of the equipment in the universities and the agencies would reside with LFRC, and the exploitation only would be handled by EPSRC. The main issue is, however, would the separation be of benefit in stabilizing the exploitation-grant fluctuation, because if a large project overruns then it's always the grants line that gets hit. Potentially if we removed that fluctuation then we would have a more stable grant situation. The most important thing is how to get a joined-up strategy between the spending on the facilities and the spending on the exploitation. It's very hard to see how you can get two independent councils, each of which has a body of a dozen people with scientists and lay members, to make consistent decisions. There are already problems with that in the current non-PPARC areas where it is done in this way. Basically we get less value for money, and the speaker envisaged an extreme situation where a new facility was built but nobody would have any money to exploit it. The other fact to bear in mind is that currently within PPARC we have the Science Committee, consisting of scientists drawn from the community, which actually makes the decisions based on scientific criteria of how much money is to be spent on new facilities and how much on exploitation, and the boundaries can be adjusted as necessary. With two councils there would be two programme managers in Swindon making that decision, and it is not clear that this would be desirable so far as the community is concerned.

Thirdly, EPSRC could be circumvented and a direct merger made between

PPARC and CCLRC to make a single RC. This would handle the whole PPARC grants portfolio. The main problem, the speaker noted, is that of inconsistency. The activities that CCLRC are concerned with are not that similar to PPARC activities and each case would need to be considered separately. What distinguishes the current PPARC portfolio from the current EPSRC portfolio is the strong and large international aspect. We have to go and compete in an international arena. The UK should not be paying for something which other countries are exploiting — this would clearly be an undesirable situation. There might, however, be a way in which to make a judgment on a case-by-case basis.

The speaker had a final word on the consultation process: soon there will be a more detailed set of questions put out to focus responses to this proposal, but they refer to a much broader range than just PPARC facilities. We need to be very careful on how we define what a large facility is. Are the *Dark Energy Survey*, *MERLIN*, or *2dF* large facilities? They are, compared with present CCLRC activities. The sort of generic things that the Government genuinely wants responses to are light sources, neutron sources, particle accelerators, CERN, space-based exploration, space-based astronomy, and ground-based telescopes. They are inviting comments about all of these aspects, not just those related to PPARC. The speaker put up a list of questions on the screen and ended his talk at that point.]

The President. I'm going to chair this discussion with a light touch. I shall ask each member of the panel to give a one-line introduction. At the end Peter Warry has agreed to summarize the discussion and give his opinion on how we can move forward.

Professor Monica Grady. I'm at the Open University. I'm a planetary scientist and on a number of PPARC committees.

Professor W. Gear. I'm from Cardiff and I recently joined the Science Committee. Before that I was chair of the Particle Astrophysics Advisory Panel.

Professor K. O. Mason. I guess you all know who I am. [Laughter.]

Mr. P. Warry. I'm chairman of PPARC.

Professor S. Miller. I'm from UCL. I'm a planetary scientist and chairman of the Solar System Advisory Panel.

Professor J. Hough. I'm from Glasgow. I'm on Council and will be taking over the Education and Training Committee in October.

Professor R. Wade. I am Deputy Chief Executive of PPARC and also Director of Programmes.

Professor R. Davies. I'm from Oxford and I'm an extragalactic astronomer. I am on the RAS Council and have just joined the PPARC Council, although I have not yet attended a meeting.

Professor M. A. Barstow. Can I ask you about the radio and space-plasma area? It was endorsed very strongly in the recent international report and yet it seems to have taken the largest hit in the evaluation by PPARC. Could you comment on this?

Professor Mason. There is nothing in the current PPARC programme that is regarded as anything other than high priority; however, the space-plasma area came across as weakest in the programmatic review. We did have a relevant expert from the field on the panel and we did not eliminate it entirely. It's an issue for that particular community and you should consider for yourselves why you came across so weakly.

Professor Miller. I'm glad that you raised this, Martin. It is obviously of concern to the Solar System Advisory Panel as we are responsible for putting the case for solar-and-terrestrial-physics (STP) activities. We should look carefully as to how the closures and the two-year time scales are going to affect programmes and delivery. I've always found STP the most difficult to sell because you are looking at the upper atmosphere and magnetospheric interactions, which require a lot of detailed investigation. It is difficult to put across the really big picture going on here. On the other hand, a lot of the work being done in the STP area is being transferred to the Solar System area, in particular, planetary plasmas, upper-atmosphere interactions, and so on, but the understanding of planets is going to be seriously affected by our reduced ability to carry out STP. We need to look at this as an astronomy panel and feed back advice to the Science Committee. There are questions of other international commitments to be explored, maybe with a slightly altered strategy.

Professor K. A. Pounds. When PPARC was set up in 1994, I was assured by David Phillips and William Waldegrave that the Government fully recognized that its predominantly 'blue-skies-research' agenda could not be subject to the same criteria (as other Councils) of 'relevance and usefulness'. I wonder if that protection written into the PPARC Mission Statement is going to disappear under the new arrangements. My other concern is that I cannot see how the LFRC is going to be effectively science-driven if it is to be providing facilities for every part of UK research. In the current CCLRC delivery plan, its top agenda items are the provision of synchrotrons, lasers, and neutrons. When satellites and telescopes — and presumably related instrumentation — are added, where is the LFRC to get its balanced science advice from? As to the alternative proposal, where grants are funded by EPSRC, I have a big problem with the separation of investment from exploitation. That seems a recipe for inefficiency and waste of resources.

Mr. Warry. Dealing with your first point, I think you said, "Has PPARC lost its protection from being irrelevant and not useful?" [Laughter.] Everyone will have seen that the Government is putting more and more focus on economic impact. At the moment I am chairing a group on behalf of the Office of Science & Technology (OST) looking at the economic impact that the RCs are having. This is one of the three key inputs which the OST is putting to the Treasury for the next comprehensive spending review, and we would contend that you should not look at economic impact over a one- or two-year timescale. Indeed, right across the sciences the economic impact is over ten or twenty years, but the reality is that it is harder for PPARC to convince ministers that our science is relevant and more useful than something like medicine or biology, or, indeed, the activities that EPSRC deals with over a much shorter timescale, and we are going to have to work harder to keep its position in the pecking order.

Professor Mason. This comes back to the point I made about the current lack of investment in the PPARC area in the last decade, compared to RCs in general. We all feel that there is a strong point to make and we need to make it. As to the other part of the question, LFRC will be a new RC and so one of the key issues will be to ensure that it is set up with the right kind of structures that allow it to be science-led. If you look at the overall vision of what the RC does, it has to be science-led if it is to fulfil its aims to compete internationally. This is something that I think we do incredibly well in PPARC, and in many ways this is how it should be done. If we are serious about making a success of this new council then we have to ensure that that part of the PPARC culture goes along.

Professor Wade. The main thing is that one can betray an expectation about what is going to happen by listing the current facilities. I suppose that the new council will be responsible for ground-based telescopes and that sort of facility but it will also be responsible for facilities in particle physics. An interesting counter to what you are saying is that potentially one of the big new developments that will take place in the UK will be a neutrino factory, so you can imagine the situation when the new merged RC is responsible for building such a facility on UK soil and the Government might be made to recognize the benefit of this. I think the issue of how you make that science-driven across such a wide range of sciences is a tricky question and one which we'll have to work out. In my mind it comes down to extending the philosophy which was developed within PPARC, which is that you have to focus on the scientific questions that you are trying to answer and not what facilities you would need to build. You need to ask what science needs to be done — sometimes the answer is to build a telescope, at other times you may need an accelerator.

Professor Davies. Could I just make one comment about this? If you look at a section in the paper, *The Case for Change*, the first two paragraphs deal with better engagement with business by the LFRC and making strategic decisions over a wider range of topics. There is no doubt that the agenda you have identified is exactly the one that is intended, and if this is to go ahead we have to learn to use it for our own purposes.

Professor Ponman. What worries me is that, given that list of facilities, it seems an inevitable consequence of arguing the need for the science driver for the LFRC, that you really have to have the users of the synchrotron sources, *etc.*, in there as well. Otherwise you have an uncomfortable situation where one part of the facility is science-driven and the other part is not. In that sense you have to bring more people on board and then persuade them that this is a good idea.

Professor Wade. Perhaps I can just answer that. If you look at the way in which CCLRC is developing the second target station for *ISIS* you'll find that they work with a science-driven advisory structure. You really have to have the users involved, whether they are users of telescopes or neutron sources. You need to determine what will the applications of a facility be.

Professor Ponman. Are you actually in the process of consulting with these communities?

Professor Hough. There is a little difficulty in the instrument community, for example. Part of the time we are producing good facilities and the rest of the time doing small-lab research. It is not clear to me that, rather than thinking about putting part of PPARC money into EPSRC, we should be pulling nuclear physics from EPSRC and trying to move it to a totally different structure.

Professor Pounds. What if you can only afford to buy a neutron-beam machine or a telescope?

Professor Wade. The answer is that you can either be on the inside making that decision or on the outside watching it being made by politicians. I would argue that we would be better off having our scientific committee inside making those decisions. That is the essence of a science-driven strategy.

A Fellow. Are things clearer now with the impact of quality on costing? In particular, are there going to be fewer grants across the board?

Professor Mason. The answer to that is that I do not yet know. We will get a much better indication once we've completed a much bigger exercise, which we are currently beginning, and in a few months we'll know what universities will actually propose. The only way that the RCs can determine the full economic costs is by modelling based on previous applications. It did not formally matter previously if you were to put down that you were going to spend 1% of your time on a project or 100%, to first order. It is not clear that the model on which the data was based had any great prominence. It is recognized that there are huge errors in that model so the reason why there is an uncertainty still is that we don't actually know what the universities will propose. That is not an issue for the RCs, but the universities. If the bill for the full economic costing is higher than we had anticipated, and there is no more money until the next spending review since it's a zero-sum game, the only way to deal with that is to have less grants.

Mr. Warry. Could I also mention that PPARC actually has a higher allocation of grants than the other RCs. I suspect that it will not be enough, but we have secured a position that is better than it ought to have been.

Dr. J. J. Eldridge. I'm a young astronomer and when you complete your research degree you aim to get a PPARC Fellowship or Advanced Fellowship. However, the success rate for applications has gone down from 14% in 2001/02 to 8% in 2004/5 as the number of applicants has doubled. This is probably due to the growing population of students, and unless all those extra applicants are submitting poorer proposals, it is getting more difficult to get these fellowships. This is also increasing the stress levels of applicants. Should you not increase the number of fellowships to follow the growing number of applicants?

Professor Wade. I'm afraid that you might not like the answer to this question. What you are assuming is that the reason we are increasing the number of PhD students is that we can train more people to become astronomers or particle physicists. That is not the reason — we believe that skills acquired in astronomy or particle physics are a very valuable commodity for the broader community. In a sense that is why the Government invests in training people in our field. I think it is fantastic that a number of those people *can* move in and make a career out of astronomy or particle physics, but to increase the number of career opportunities for those people is not a sustainable position. You have to accept that, although the reason that you are doing a PhD in astronomy is because you harbour an ambition to be a professional astronomer, it may not be a realizable option.

Mrs. Nancy Z. Marsden. What is the point of studying in astronomy if you can't become an astronomer?

Mr. Warry. People with the sort of PhDs in astronomy and particle physics — and many other areas — which we produce are highly prized individuals. We all learn skills that we hope might take us to a professional career in astronomy. We can't all do that but the skills are still valuable.

Professor Wade. The alternative is to train fewer PhD students.

Professor Miller. I know exactly where you are coming from. Back in the 1960–1970s when I was doing my studies as a young postdoc, I expected to go straight into an academic career. The big expansion in the universities in the 1960s had filled all the posts with young people who were not going to retire for a long time so I was frozen out. I think that nowadays you cannot expect to have a single-track career. I went away and became a journalist for six years before coming back into science. I would not have missed the training in physical chemistry; I thoroughly enjoyed it but I had to do something else for a while.

Professor Hough. The whole scientific community benefits if the PhDs go into industry and government. It is very noticeable that in both France and Germany the Government offices dealing with scientific disciplines are actually staffed by people who know a great deal about those disciplines.

Professor A. Lawrence. I wanted to ask whether anybody would comment on a certain reading of history, which relates to Trevor Ponman's question, on what happens with all those biologists, chemists, and to some extent, physicists, who use these other facilities, and where their science advice goes, and where their money goes. There's a certain reading of the history of *Diamond*, where the biologists in particular took a deep breath and said "nothing to do with us", and it was therefore top-sliced, which meant that effectively PPARC paid disproportionately for it. Whether you think that sounds as reasonably correct — but even if it isn't — how do we stop that happening? I think Trevor's right; it doesn't make any sense unless we understand the strategy for a very large range of other science, and what it means for the LFRC, and how decisions are taken about where money is spent and why.

Professor Mason. I think the only answer to that question is you have to keep control, and you have to have an active dialogue with the interested parties. There is an increasingly good dialogue between the RCs, and if we're supposed to learn from history so we don't repeat our mistakes, the only way is to keep control, know what you're doing, have a strategy in place so you can see when things are going wrong, and engage the interested parties in a proper dialogue.

Professor Lawrence. One can't disagree with any of that, but will a LFRC improve or exacerbate that problem?

Professor Mason. It depends on how you set it up, and that's the key — it could be an abject disaster if it's set up the wrong way, or the best thing since sliced bread if it is done right, and there is a continuum in between. What we have to do is make sure it is on the 'sliced bread' side, rather than the 'burnt toast' side.

Professor P. A. Charles. Keith, are you or any of the panel able to say anything about how these proposals are being received by the other RCs, in particular CCLRC and EPSRC?

Professor Mason. I can certainly tell you what the CCLRC response is. CCLRC has not been in existence for that long and it has had quite a difficult genesis — it is not like the other RCs, and it has had to find its rôle. I think it is increasingly doing that very successfully under John Wood, and the CCLRC of today is very different from the CCLRC of yesterday. I hope also that the LFRC will have a future, because we need to keep going on that vector which makes it more and more effective, more competitive. So CCLRC welcome this opportunity to continue the good work that they've already started.

I don't know what EPSRC thinks. Clearly this is disruptive — it will be disruptive for them as well. If we were to put the PPARC grants portfolio into EPSRC, it might still be called EPSRC, but it wouldn't look like the current EPSRC. It would be a gigantic animal, and that would create its own problems. NERC have issues — they have large facilities too — and I personally think that you really do need to keep the strategy and facility in the same pot. NERC operates ships, *etc.*, which are clearly large, expensive facilities, and it makes no sense to have the strategy for those ships anywhere other than NERC. This proposal has set tremors through the whole system and it is not clear where the chips will fall yet.

Mr. Warry. It's not just us who have been affected — there is a very significant proposal about medical research, which has caught them by surprise. It will take some time.

Professor Gear. There's been a lot of emphasis on the model that Keith has been pushing — we all agree on the importance of keeping strategy with delivery and there has been a lot of emphasis on the proposal to move the strategy of PPARC entirely into the LFRC. As I think Andy Lawrence has indicated, this might put astronomy in a somewhat privileged position compared to aspects of biological sciences or solid-state physics, for example. For me, there hasn't been enough discussion of the route whereby, if the astronomy strategy is in EPSRC, there is control over its delivery from within the LFRC, which is the model that exists in biology and currently exists in EPSRC. Just to repeat something I have said twice now to some of the same people here — CCLRC has more employees than there are astronomers in this country, so no matter what the remit of the Chief Executive of the LFRC, he would have 2000 people sitting there that he has to pay and feed, and there is a distinct chance that they would become a higher priority than, for example, operating a small telescope on a very distant, small island in the middle of the Pacific. I know that Keith has objected to this viewpoint, and his solution to that is to move the strategy into that Council, but there is a distinct danger in doing that.

Professor Mason. The other part of my answer is that the landscape has changed — this would have been an issue, say, 10 or 15 years ago; I think it is much less of an issue now. Also, the model that is in the Treasury statement is that the current RAL and Daresbury sites become science and innovation parks, and that's deliberate, to push the knowledge-transfer aspects, to push the knowledge out into industry. It's clear that such a park, properly invested in, will attract huge amounts of inward investment into those sites. I don't know if Richard wants to add to that?

Professor Wade. Well, I think that your view, Walter, is very much a sort of stone-age view [laughter] of the way that RCs work, and the way that the British economy works. I think you could look at numerous examples of where you might have expected this to work in a certain way in the 1970s, but it doesn't work that way in the 2000s.

Professor M. Rowan-Robinson. I wanted to comment on how the astronomy community should respond to this document. We've focussed very strongly on this LFRC, but that is only a small part of the document. As Keith said at the beginning, it is actually a very positive document as far as science is concerned, because science is seen as being at the centre of the economy, so that is something we should welcome. There is also a big section — a much bigger section than there is on the facilities — on education, on improving applied sciences, on increasing the uptake of science, technology, engineering, and mathematics (STEM) subjects in schools. That is something we should emphasize very strongly — that astronomy and space science are very important motivators for pupils towards STEM subjects. We should respond wholeheartedly to that part of the document, and say that we definitely want to participate in it and help the Government achieve its goals, and that we think we're very well placed to do that. I accept that we have worked hard to improve our performance in knowledge transfer and links with industry and so on, but we're not going to score that many points on that score-sheet; but at least on the education side, we have a very strong case.

On the crunch issue of the facilities and where grants go, it seems to me that this LFRC is not particularly needed by astronomy, but we are told that it's going to happen anyway, so we do have to try and make it as much like PPARC as we can, and perhaps just merging the whole of PPARC with it is the best way to achieve that.

Mr. Warry. It is very clear that our science has much greater pull in schools than almost any other area of science, and that is something that ministers value, and I hope will continue to value. It is also interesting that in the wider world of knowledge transfer, the work that PPARC has done is actually much appreciated.

Professor Rowan-Robinson. It wasn't really a question — just a comment.

Professor Davies. Just a piece of information here: I don't know how many people here have read this paper, but the part on education calls for 25% of science teachers to have a physics degree by 2014. It also announces a new commitment that for pupils achieving level 6 at Key Stage 3 there will be an entitlement to do three science GCSEs. So these are extremely positive things, and are also relevant to the question of what people with PhDs in astronomy do for careers; that is an area where we don't do very well as a community — we don't send very many people into physics teaching. That's something as a community that we have to address, and I think if we can show we're adopting this agenda enthusiastically, then that would be extremely positive for us because, as Peter said, it's recognizing that our science is strong in this area — we bring young people into science.

A Student. Just a quick reply to that — I can't speak for everybody, obviously, but I don't think I have met very many astronomy PhD students who study for an astronomy PhD with the idea of becoming a teacher, or doing something that isn't astronomy. If they want to go and work in the City, they are quite capable of doing that straight from a degree. In fact, I was at a recent presentation by Accentua, who said they made no real distinction between someone with a PhD and someone without. They both go in at the same level in the graduate programme, they both get paid exactly the same, so really there is no benefit in doing a PhD if that is what you want to do. People who want to do an astronomy PhD probably want a career in astronomy.

Professor Wade. I can't resist answering that, because this is something that comes up every time we have one of these meetings. I will defend the fact that we're increasing the number of people we are training in astronomy, and if I have anything to do with it, we'll continue to increase the number of people we train in PhDs in astronomy, because I continue to believe that it is a good training, and we should not back away from it. I'd reflect the question back to you — would you rather we decrease the number of PhD studentships available in astronomy?

The Student. No, I wouldn't disagree with what you are saying; what I'd rather you do is provide, as the gentleman down here asked earlier, a greater opportunity for people to continue their career in astronomy so they can become professional astronomers.

Professor Mason. Even if we increase the number of fellowships, you'd then have to ask what happens at the end of the fellowship. I'd just like to take issue with you though on the fact that PhD students are not interested in teaching. My experience is absolutely the opposite of that. I think that there are huge numbers of excellent researchers out there who are also good at transmitting that information to a wider society. They like to do it, and I think we should be working to provide opportunities to do it, because it's a valuable resource and we can't afford to ignore it.

Dr. N. A. Walton. Keith's early slides showed that total research funding has doubled over the last five years but that the research funding of astronomy has not doubled over the last five years. Therefore it seems to me that this is a great opportunity to get a bigger share of the increased pot of money. The question is, have you a good process for marshalling feedback to the consultation process such that the community speaks with a clear, focussed voice? I think the opportunities are there for benefits for astronomy.

Mr. Warry. Thank you very much — that is what I wanted to say myself. There is an opportunity here, and there is a threat. There is a significant opportunity if we can take the positives out of it and marshal the arguments well, if we can make sure that we get a greater share of capital, and that it's science-driven. You all have a very important part to play in that, because it would be very helpful if we can get some positive messages coming back. I think you'll have heard our concern about the separation of funding and exploitation of facilities, and the risks that that can cause; if we can make sure from the responses we give that the funding and the facilities must be kept together, then that would be very helpful. I believe that is all to play for. How do we do that? I know that SCAP will be putting forward a view, I'm sure that the RAS will be putting forward a view — if that's a positive and a united view, that would be constructive and extremely helpful.

Professor Grady. Thank you, Kathy! Kathy just said I can't be here and stay silent all the time! [Laughter.] I would like to make what is really a very trivial comment. We've been talking about an LFRC, and an LFRC has to mean something, but its title doesn't mean anything about science, about strategy, it doesn't talk about the questions that those facilities are going to answer. I think really if we want to take things forward — this is where the trivial bit comes in — we need a new name for this RC, and once you have a name for it (and a logo), and preferably a name that doesn't spell something else backwards [laughter], then we can go forwards in looking at those questions and defining how the RC works. I have a suggestion, and my suggestion is to name it the 'Physical Processes and Applications Research Council'. [Laughter.]

The President. I am conscious that we are running out of time. Peter, if you could say a few words just to wrap things up, that would be really helpful.

Mr. Warry. First of all can I thank everybody for contributing to this; it has been very helpful to me and to Keith. I would like to reiterate some of the things Keith said in his presentation, first of all in terms of the programmatic review. There have been some very tough choices that we have had to make. Almost everything we do is first rate, and choosing between first-rate projects is very difficult, but the sum of money is fixed and we can't do much about that, other than — and this is a crucial point for everybody — that we need to inject a greater awareness amongst ministers, amongst the public, and in the media, of how important the PPARC science is ultimately to the country as a whole.

We've had a debate about large facilities, and to my mind the things that have come across are that a new council must be science led — we can't afford to let this be facilities led. How do we do that? Well, it's a whole lot easier to do that from the inside than from the outside, and this is the message I would take. I have genuinely been worrying about the name; I don't want to have 'facilities' in that name, because if it is, then it's about facilities, and that's not what we want — so thank you, Monica, though I'm not quite sure your proposal will be entirely acceptable [laughter].

I would keenly reiterate the point about trying to keep the funding and the facilities together; we do not want to separate them. If we separate them, it does not seem to work, and the experience in EPSRC in an area like nuclear physics, for example, shows that it has not been entirely satisfactory. I think the PPARC model is the one we want, and if things are going to change we want to get that culture into the new RC.

I would like to comment on students, just because this has come up several times. It is a real world out there, and people should not do PhDs without doing their research, and part of that research should be finding out that the probability of being able to continue your studies professionally is sadly not higher than 30%. That does not mean you are wasting your time: it is still an excellent education, and I disagree with the speaker who said it wasn't valued — my experience is that people do value that education and employers value it.

The President. We will continue this discussion, and I encourage you all to contribute to the RAS's discussion *via* the web forum, or by contacting a member of Council. We do want you to have input and participate in this process. The RAS Council discussion will take place on May 12. I'd like to thank the panellists and Keith for coming along today, and to remind you that the next monthly A&G meeting of the RAS will take place in London on May 12.

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 190: HD 109484, HD 110376, HD 119334, AND HD 120531

By R. F. Griffin
Cambridge Observatories

The stars discussed here are all 8^{m}–9^{m} single-lined spectroscopic binaries whose nature was discovered in the course of a survey of radial velocities of late-type stars in the field of the North Galactic Pole. HD 110376 is a main-sequence star with a type near K2 V; the others are all giants, although the luminosity of HD 120531 seems rather on the low side. The four stars have orbital periods of about 14, 3½, 1¾, and 13 years, respectively. The orbits of HD 119334 and HD 120531 have eccentricities as high as 0·8; the other two have only modest eccentricities.

Introduction

Certain previous papers in this series (most recently no. 184) have provided orbits for some of the ~125 spectroscopic binaries identified in the course of the Cambridge survey[1] of the radial velocities of all the late-type *HD* stars in the field of the North Galactic Pole (hereinafter NGP) ($b > 75°$). Here are four more. HD 109484 and HD 110376 are in Coma, while the other two stars are at the following margin of the NGP field, in Boötes and Canes Venatici, respectively.

Rather surprisingly in view of their magnitudes, all four stars feature in the *Hipparcos* catalogue, so their parallaxes are accurately measured. In the case of the single dwarf star treated here, HD 110376, the parallax leads to an accurate luminosity; in the other cases it is not incomparably greater than its standard error, so although it indicates that the stars concerned are giants it does not provide very accurate absolute magnitudes for them. The parallaxes and the derived absolute magnitudes are listed in Table I together with the photometry from *Hipparcos*[2], from Yoss & Griffin[1], and from such other sources as have been located in the literature.

TABLE I

Magnitudes, parallaxes, and absolute magnitudes for the four stars

Star	*V*	*(B−V)*	*(U−B)*	*π*	M_V	*Ref.*
	m	*m*	*m*	″.001	*m*	
HD 109484	8·43			2·68 ± 1·24	+0·6 ± 1·0	2
	8·46	0·99			+1·6	1
	8·42	0·97	0·74			3
HD 110376	9·07	0·940		28·84 ± 1·42	+6·57 ± 0·11	2
	8·99	1·06	0·95		+6·5	1
	9·13					4
HD 119334				3·51 ± 1·81	+1·7 ± 1·3	2
	8·93	0·98			1·4	1
	8·92	0·97	0·77			3
HD 120531	7·97	0·950		6·44 ± 0·92	+2·0 ± 0·3	2
	7·98	0·92			+2·8	1

In the course of the NGP survey programme, radial-velocity observations of the stars were begun with the original photoelectric spectrometer in Cambridge in the early 1970s, except in the case of HD 119334 whose first measurement was not until 1980. It took quite a long time to discover the binary natures of the objects and thereupon to transfer them to the spectroscopic-binary programme and to start observing them systematically; that happened for all four stars in the 1980s. The two long-period stars, HD 109484 and HD 120531, have each been watched for considerably more than a full cycle; the latter was specially carefully monitored during the periastron/nodal passage in its very eccentric orbit. HD 110376 has been seen round seven cycles and HD 119334 round ten since systematic measurements began. At different times and places, six spectrometers have been used; Table II shows the number of observations attributable to each of them for each of the stars.

TABLE II

Sources of radial-velocity measurements of the four stars

Source	*HD 109484*	*HD 110376*	*HD 119334*	*HD 120531*	*Totals*
Cambridge (old)	25	35	5	7	72
Palomar	1	–	–	–	1
DAO	2	4	2	2	10
OHP	30	26	24	23	103
ESO	1	1	1	1	4
Cambridge (new)	31	29	40	34	134
Totals	90	95	72	67	324

HD 109484

HD 109484 is on the fringes of the Coma Cluster, and carries the designation Melotte 111 no. 191 in Trumpler's listing[5] of stars in that vicinity, although there is no suggestion that it is actually a *member* of the cluster. Trumpler gave the spectral type as gK0. Melotte's contribution was to record the Coma Cluster as no. 111 in his *Catalogue of Star Clusters shown on the Franklin-Adams Plates*[6], in which he placed it in his Class IV ('Coarse Clusters'). It features also in the astrometric catalogues of Meyermann[7], who numbered it 269 and unaccountably listed its spectral type as A1, and of Abad & Vicente[8], in which it is no. 2256.

Woolley *et al.*[9] classified HD 109484 as K0 III from a 66-Å mm^{-1} spectrogram taken with the 74-inch Kottamia telescope, and gave the radial velocity measured from it as $-33 \cdot 0 \pm 4 \cdot 5$ km s^{-1} (complete with date). Bartevičius & Lazauskaitė[10] used Vilnius-type photometry[11] to classify a lot of stars that had been listed as metal-deficient in an unpublished catalogue; the type that they gave for HD 109484 (their no. 362) was "MD?-G9 III, Ba", where the MD stands for metal-deficient, but they also derived from the photometry a logarithmic abundance [Fe/H] of only $-0 \cdot 15$ — so not very metal-deficient after all — and an absolute magnitude of $+1 \cdot 75$. Yoss & Griffin's[1] *DDO*-style photometry[12] led to a type of G9 III and an [Fe/H] of $-0 \cdot 05$. The radial velocity that is given in the table mentioned by Famaey *et al.*[13] is a γ-velocity derived by those authors from the present writer's OHP *Coravel* observations that they found on the relevant data base in Geneva and that appear as part of the data in Table III below.

On 2000 June 10·95 a *fairly* nearby star, BD $+25^\circ$ 2529 (about 11′ south-following HD 109484) was observed, probably inadvertently, with the Cambridge *Coravel*; it gave a radial velocity of $+16 \cdot 5$ km s^{-1}. Deliberate re-measurement on 2006 March 2·14 gave $+17 \cdot 5$.

HD 110376

HD 110376 is the only dwarf star among the objects treated in this paper. Table I shows the excellent agreement between the absolute magnitudes derived from the *Hipparcos* parallax ($+6 \cdot 57$) and from the *DDO* photometry of Yoss & Griffin[1] ($+6 \cdot 5$). The same photometry yielded an 'mk' type of K3 V; the *HD* type[14] is K2. *Hipparcos* noticed the orbital motion of HD 110376, which could be expected to be quite large enough, and of a favourable period, for it to have determined an orbit, but it derived only an 'acceleration solution' which is none too instructive, at least to the uninitiated. Table I shows that each of the three sources of photometry of the star is remarkably discordant with each of the others, the strong *prima facie* implication being that the star is variable. Such a conclusion is, however, directly at variance with the one that could be drawn from the constancy of the *Hipparcos* 'epoch photometry'. The contradiction cannot be resolved here in the absence of further data: either there must be substantial error in two of the photometric sources reported in Table I, or the star must exhibit *intermittent* variability. That could easily be attributed to intermittent spottedness on a K dwarf, but is not an attractive way of explaining a variation in which the star is redder when it is brighter, as the magnitudes in Table I indicate.

HD 119334

HD 119334 is near the south-following margin of the NGP field as defined for the purposes of the survey[1] (a radius of 15° around the Galactic Pole), and thereby manages to be in Boötes, a constellation not normally associated with the Galactic Pole. The star's *Hipparcos* parallax has an unusually large uncertainty, possibly owing to the unrecognized orbital motion, of which nearly two cycles were completed during the satellite's observing lifetime. The parallax shows only that the star is of the general nature of a giant, and that is confirmed by spectroscopic and photometric data. Yoss, Neese & Hartkopf[15] used *DDO*-style photometry to derive a photometric type of G9 III, an absolute magnitude of +0·8, and an [Fe/H] of −0·12. They listed a V magnitude of 8·76, but noted that they were quoting it from an imprecise source; they also gave a z distance of 383 kpc — it must be supposed that an error was made in the units in the heading of the relevant column of their table. They listed one radial-velocity measurement, complete with date; it was made with the spectrometer at the coudé focus of the Victoria 48-inch reflector and is duly recorded in Table V. The entry for HD 119334 in the NGP survey[1] of Yoss & Griffin includes the measured magnitudes listed in Table I above, the type of G9 III, $M_V = +1^{m}\cdot 4$, [Fe/H] = −0·12, and z = 313 pc.

The star appears also in a paper by Soubiran, Bienayme & Siebert[16] on the "Vertical distribution of Galactic disk stars", which had the same purpose as reference 1 but involved observations of a much smaller number of stars; many of them are in common with ref. 1, which is, however, nowhere mentioned in the paper. In particular, Soubiran *et al.* observed HD 119334 and HD 120531 among the stars that also feature in the present paper. They made only one observation of each star, so they did not discover their binary natures; although for each object they reported their one radial velocity, it cannot be included in this paper because they did not give the date. For HD 119334 they obtained an M_V of $+0^{m}\cdot 8$ (they actually gave it to three decimal places of a magnitude) and an [Fe/H] of −0·13. *Simbad* additionally records the presence of HD 119334 in a 1986 paper on interstellar polarization, which identifies stars by their SAO numbers; HD 119334 (SAO 82953) is not there, but SAO 82853 *is*, so there has probably been a mistake in *Simbad*.

HD 120531

HD 120531 has a parallax that is large enough and well enough determined to show that the star is rather less luminous ($M_V \sim +2^{m}\cdot0 \pm 0^{m}\cdot3$) than would be expected for a normal giant; its luminosity class could be put at III–IV. Its somewhat subdued luminosity was also attested in the NGP survey[1], in which it was assigned a photometric type of K0 IV and an M_V of $+2^{m}\cdot8$, with [Fe/H] = −0·22. Soubiran *et al.*[16] found $M_V \sim 2\cdot6$ and [Fe/H] as low as −0·45. The star appears also in the table referred to by Famaey *et al.*[13], but as in the case of HD 109484 the radial velocity given there is a γ-velocity derived by those authors from such of the radial velocities that feature in this paper as were to be found in their data base in Geneva; they had been made with the Geneva Observatory's *Coravel*s at Haute-Provence and ESO, and were used with the present writer's permission.

TABLE III

Radial-velocity observations of HD 109484

Except as noted, the sources of the observations are as follows:
1967–1991 — original Cambridge spectrometer (weighted ¼ in orbital solution);
1992–1998 — Haute-Provence Coravel (wt. ¼); 1999–2006 — Cambridge Coravel

Date (UT)	MJD	Velocity km s⁻¹	Phase	(O−C) km s⁻¹
1967 Mar. 30·90*	39579·90	−33·0	0·444	−12·8
1973 Feb. 24·15	41737·15	−28·5	0·860	+0·4
1982 Mar. 6·06	45034·06	−21·1	1·496	−0·5
1984 Apr. 24·93	45814·93	−22·7	1·647	−0·2
1985 Feb. 24·04	46120·04	−24·6	1·706	−0·9
1986 Jan. 26·16	46456·16	−23·1	1·771	+2·4
Apr. 11·05†	531·05	−25·0	·785	+0·9
May 5·92	555·92	−26·0	·790	+0·1
26·90	576·90	−26·2	·794	0·0
28·97	578·97	−26·3	·794	0·0
Nov. 24·55‡	758·55	−27·6	·829	0·0
1987 Jan. 6·19	46801·19	−27·8	1·837	+0·1
Feb. 1·13	827·13	−30·1	·842	−2·0
Mar. 1·14†	855·14	−28·1	·848	+0·3
Apr. 27·92	912·92	−28·7	·859	+0·2
May 31·93	946·93	−29·3	·865	−0·1
Dec. 22·22	47151·22	−30·7	·905	+0·4
1988 Jan. 8·21	47168·21	−31·0	1·908	+0·2
Feb. 1·43§	192·43	−31·4	·913	0·0
Mar. 11·06†	231·06	−31·9	·920	−0·1
May 26·93	307·93	−31·8	·935	+0·6
June 12·92	324·92	−32·3	·938	+0·2
Nov. 5·21†	470·21	−33·5	·966	−0·4
1989 Feb. 11·13	47568·13	−32·6	1·985	+0·3
Mar. 18·03	603·03	−31·9	·992	+0·8
Apr. 28·94†	644·94	−32·0	2·000	+0·3
May 29·90	675·90	−31·5	·006	+0·5
June 19·93	696·93	−30·5	·010	+1·2

TABLE III (*continued*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
1990 Jan. 31·10†	47922·10	−28·0	2·054	+0·3
Feb. 12·31¶	934·31	−28·2	·056	−0·1
Mar. 26·97	976·97	−27·7	·064	−0·3
Apr. 30·88	48011·88	−27·0	·071	−0·1
May 26·95	037·95	−27·4	·076	−0·9
Dec. 27·22	252·22	−23·6	·117	+0·2
1991 Jan. 29·12†	48285·12	−23·9	2·124	−0·4
June 10·94	417·94	−21·7	·149	+0·7
1992 Jan. 16·11	48637·11	−20·9	2·192	+0·3
Feb. 28·38§	680·38	−21·8	·200	−0·8
Apr. 23·91	735·91	−20·5	·211	+0·3
June 25·88	798·88	−19·2	·223	+1·4
Aug. 15·82	849·82	−20·0	·233	+0·4
Dec. 20·26	976·26	−21·0	·257	−0·8
1993 Feb. 14·12	49032·12	−20·2	2·268	−0·1
Mar. 23·12	069·12	−19·5	·275	+0·5
July 7·92	175·92	−19·8	·296	+0·1
Dec. 29·20	350·20	−19·9	·329	−0·1
1994 Jan. 8·16	49360·16	−19·0	2·331	+0·8
Feb. 18·12	401·12	−18·5	·339	+1·3
May 3·01	475·01	−20·3	·353	−0·5
Aug. 2·85	566·85	−19·8	·371	+0·1
Dec. 14·22	700·22	−20·2	·397	−0·3
1995 Jan. 3·18	49720·18	−20·1	2·401	−0·1
June 2·96	870·96	−20·5	·430	−0·4
Nov. 7·17	50028·17	−21·6	·460	−1·3
1996 Mar. 30·98	50172·98	−20·8	2·488	−0·3
Nov. 21·27‖	408·27	−21·4	·533	−0·5
Dec. 15·25	432·25	−20·9	·538	+0·1
1997 Mar. 29·07‖	50536·07	−21·1	2·558	+0·1
Apr. 17·94‖	555·94	−21·4	·562	−0·1
May 13·02‖	581·02	−21·2	·567	+0·1
July 20·90	649·90	−21·4	·580	+0·1
Dec. 25·18	807·18	−23·0	·610	−1·1
1998 May 1·99	50934·99	−21·9	2·635	+0·4
July 27·87	51021·87	−21·6	·652	+1·0
1999 Dec. 29·20	51541·20	−25·0	2·752	−0·1
2000 Feb. 16·08	51590·08	−25·0	2·761	+0·2
Apr. 7·00	641·00	−25·3	·771	+0·2
June 10·95	705·95	−26·3	·784	−0·4
Nov. 20·26	868·26	−27·2	·815	−0·2
2001 Jan. 7·22	51916·22	−27·5	2·824	−0·1
Mar. 2·15	970·15	−27·2	·835	+0·6
May 12·93	52041·93	−28·1	·849	+0·3
Dec. 30·20	273·20	−30·9	·893	−0·4
2002 Feb. 14·10	52319·10	−31·4	2·902	−0·5
Mar. 10·07	343·07	−31·4	·907	−0·2
29·01	362·01	−31·6	·910	−0·3

TABLE III (*concluded*)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O – C) km s⁻¹*
2002 Apr. 19·99	52383·99	−31·4	2·915	+0·1
May 16·98	410·98	−31·4	·920	+0·4
2003 Feb. 15·13	52685·13	−33·1	2·973	0·0
Mar. 16·07	714·07	−33·2	·978	−0·2
May 7·89	766·89	−33·0	·988	−0·2
2004 Jan. 17·22	53021·22	−29·9	3·038	−0·2
Mar. 30·07	094·07	−28·5	·052	0·0
May 4·96	129·96	−28·1	·058	−0·2
June 16·93	172·93	−26·7	·067	+0·5
Dec. 27·25	366·25	−24·5	·104	0·0
2005 Mar. 23·12	53452·12	−24·0	3·121	−0·4
May 7·94	497·94	−22·9	·129	+0·3
June 6·91	527·91	−23·3	·135	−0·4
2006 Jan. 29·20	53764·20	−21·2	3·181	+0·2
Mar. 23·04	817·04	−21·1	·191	+0·1

*Observation published by Woolley *et al.*[9]; wt. 0.
†Observed with Haute-Provence *Coravel*; wt. ¼.
‡Observed with Palomar 200-inch telescope; wt. ¼.
§Observed with DAO 48-inch telescope; wt. ¼.
¶Observed with ESO *Coravel*; wt. ¼.
‖Observed with Cambridge *Coravel*; wt. 1.

TABLE IV

Radial-velocity observations of HD 110376

Except as noted, the sources of the observations are as follows:
1971–1990 — original Cambridge spectrometer (weighted 1/10 in orbital solution);
1991–1996 — Haute-Provence Coravel (wt. ¼); 1997–2006 — Cambridge Coravel

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O – C) km s⁻¹*
1971 Feb 21·05	41003·05	−6·3	0·165	−1·1
1980 Jan. 2·16	44240·16	−12·7	2·703	+0·1
May 6·00	365·00	−13·1	·800	−0·4
1981 May 4·99	44728·99	−3·8	3·086	+1·6
1982 Mar. 2·11	45030·11	−8·3	3·322	−0·9
May 5·01	094·01	−6·7	·372	+1·6
1983 Feb. 4·53*	45369·53	−11·7	3·588	0·0
Mar. 7·09	400·09	−12·3	·612	−0·3
Apr. 15·92	439·92	−12·3	·643	0·0
May 9·93	463·93	−12·0	·662	+0·5
June 15·91	500·91	−12·7	·691	0·0
Dec. 11·22	679·22	−13·8	·831	−1·4

TABLE IV (*continued*)

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O – C)* *km s*$^{-1}$
1984 Jan. 9·16	45708·16	–12·2	3·853	–0·2
Apr. 13·97	803·97	–10·6	·929	–0·6
May 11·91	831·91	–8·6	·950	+0·7
Dec. 21·26	46055·26	–5·4	4·126	–0·3
1985 Jan. 24·16	46089·16	–5·6	4·152	–0·5
Feb. 17·45*	113·45	–4·7	·171	+0·5
May 31·92	216·92	–6·8	·252	–0·6
1986 Jan. 25·13	46455·13	–10·0	4·439	–0·6
Feb. 27·11	488·11	–10·4	·465	–0·5
Mar. 7·05	496·05	–11·6	·471	–1·6
Apr. 10·07†	530·07	–9·1	·498	+1·3
May 12·96	562·96	–9·5	·524	+1·3
18·91	568·91	–10·8	·528	+0·1
Dec. 12·23	776·23	–11·0	·691	+1·7
1987 Jan. 7·24	46802·24	–11·8	4·711	+1·0
Feb. 1·15	827·15	–13·1	·731	–0·2
Mar. 1·15†	855·15	–12·9	·753	0·0
Apr. 27·96	912·96	–12·6	·798	+0·1
May 31·95	946·95	–11·9	·825	+0·5
Dec. 10·27	47139·27	–8·5	·975	–0·1
1988 Jan. 8·24	47168·24	–6·7	4·998	+0·9
Feb. 1·44*	192·44	–7·2	5·017	–0·2
Mar. 11·07†	231·07	–5·6	·047	+0·6
Apr. 12·93	263·93	–5·7	·073	–0·1
May 26·94	307·94	–4·6	·108	+0·6
Nov. 5·20†	470·20	–6·9	·235	–1·0
1989 Feb. 11·15	47568·15	–7·3	5·312	–0·1
Mar. 25·08†	610·08	–7·7	·345	+0·1
Apr. 28·95†	644·95	–8·6	·372	–0·3
May 26·93	672·93	–7·7	·394	+1·0
1990 Jan. 31·11†	47922·11	–11·8	5·589	–0·1
Feb. 12·32‡	934·32	–12·5	·599	–0·6
Mar. 27·00	977·00	–13·5	·632	–1·3
Apr. 30·91	48011·91	–13·0	·660	–0·5
1991 Jan. 29·13	48285·13	–11·6	5·874	0·0
Feb. 6·14	293·14	–12·0	·880	–0·6
1992 Jan. 16·12	48637·12	–5·1	6·150	0·0
Feb. 28·41*	680·41	–5·1	·184	+0·2
Apr. 23·94	735·94	–5·5	·227	+0·3
June 25·89	798·89	–6·4	·277	+0·2
Dec. 20·25	976·25	–9·1	·416	–0·1
1993 Feb. 15·10	49033·10	–10·1	6·460	–0·3
Mar. 24·97	070·97	–9·6	·490	+0·7
July 8·90	176·90	–12·1	·573	–0·6
Dec. 30·21	351·21	–12·9	·710	–0·1

TABLE IV (*concluded*)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O – C) km s⁻¹*
1994 Feb. 18·12	49401·12	–13·6	6·749	–0·7
May 3·05	475·05	–12·9	·807	–0·3
Aug. 3·85	567·85	–11·9	·879	–0·4
Dec. 14·22	700·22	–8·3	·983	–0·2
1995 Jan. 5·17	49722·17	–7·9	7·000	–0·4
June 2·97	870·97	–5·6	·117	–0·4
1996 Mar. 30·99	50172·99	–9·1	7·354	–1·2
1997 Mar. 31·95	50538·95	–12·7	7·641	–0·4
Apr. 17·98	555·98	–12·2	·654	+0·2
May 13·02	581·02	–12·5	·674	+0·1
July 18·89†	647·89	–13·7	·726	–0·8
1998 July 8·87†	51002·87	–7·4	8·004	0·0
2000 Jan. 9·18	51552·18	–9·4	8·435	0·0
Apr. 7·00	641·00	–10·3	·505	+0·2
May 29·95	693·95	–11·5	·546	–0·3
June 19·96	714·96	–11·6	·563	–0·2
2001 Jan. 7·23	51916·23	–12·6	8·721	+0·3
Feb. 27·12	967·12	–12·4	·760	+0·5
Dec. 30·22	52273·22	–7·8	9·000	–0·3
2002 Feb. 21·01	52326·01	–6·3	9·042	0·0
Mar. 10·08	343·08	–6·3	·055	–0·3
28·95	361·95	–5·2	·070	+0·5
Apr. 20·00	384·00	–5·4	·087	0·0
May 16·99	410·99	–5·3	·108	–0·1
2003 Feb. 18·09	52688·09	–7·6	9·326	–0·2
Mar. 17·03	715·03	–7·4	·347	+0·4
Apr. 16·00	745·00	–7·8	·370	+0·4
May 14·96	773·96	–8·7	·393	–0·1
2004 Jan. 17·24	53021·24	–11·7	9·587	0·0
Mar. 31·03	095·03	–12·3	·645	+0·1
May 22·95	147·95	–12·8	·686	–0·1
Dec. 27·26	366·26	–11·7	·857	+0·2
2005 Mar. 23·13	53452·13	–10·3	9·925	–0·1
Apr. 21·97	481·97	–9·1	·948	+0·3
May 14·99	504·99	–8·7	·966	0·0
2006 Feb. 16·15	53782·15	–5·5	10·183	–0·2
Mar. 23·05	817·05	–5·5	·211	+0·1
Apr. 11·05	836·05	–5·9	·226	–0·1

*Observed with DAO 48-inch telescope; wt. ¼.
†Observed with Haute-Provence *Coravel*; wt. ¼.
‡Observed with ESO *Coravel*; wt. ¼.

TABLE V

Radial-velocity observations of HD 119334

Except as noted, the sources of the observations are as follows:
1980–1996 — Haute-Provence Coravel (wt. ¼); 1997–2006 — Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O − C)* *km s*$^{-1}$
1980 May 17·97*	44376·97	−19·5	0·494	+0·4
1986 Mar. 17·48†	46506·48	−23·2	3·779	−0·5
May 26·98*	576·98	−25·3	·888	−0·7
1988 Feb. 1·54‡	47192·54	−23·7	4·838	−0·1
Mar. 13·16	233·16	−24·6	·900	+0·3
1989 Mar. 27·12	47612·12	−20·0	5·485	−0·2
Apr. 29·09	645·09	−19·5	·536	+0·7
June 1·98*	678·98	−19·6	·588	+1·1
1990 Jan. 27·17	47918·17	−26·2	5·957	+0·5
Feb. 14·40§	936·40	−26·7	·985	−0·6
Apr. 5·04*	986·04	−14·3	6·062	−0·8
1991 Jan. 30·15	48286·15	−20·1	6·525	+0·1
May 23·01*	399·01	−20·9	·699	+0·9
1992 Feb. 27·51‡	48679·51	−15·7	7·131	−0·1
Apr. 24·08	736·08	−16·5	·219	+0·6
June 25·99	798·99	−18·6	·316	−0·4
1992 Dec. 19·24	975·24	−21·0	·588	−0·3
1993 Feb. 15·21	49033·21	−22·6	7·677	−1·0
Mar. 19·16	065·16	−21·8	·726	+0·3
July 7·97	175·97	−24·7	·897	+0·1
1994 Jan. 3·17	49355·17	−16·1	8·174	+0·3
Feb. 21·16	404·16	−18·0	·249	−0·5
May 2·08	474·08	−18·8	·357	−0·2
Aug. 2·92	566·92	−20·5	·500	−0·6
1995 Jan. 4·27	49721·27	−22·5	8·738	−0·3
June 3·04	871·04	−27·6	·970	−0·6
Dec. 27·22	50078·22	−18·9	9·289	−1·0
1996 Mar. 31·11	50173·11	−18·9	9·435	+0·5
1997 Mar. 1·15	50508·15	−26·6	9·952	−0·1
6·19	513·19	−27·4	·960	−0·6
27·06	534·06	−23·7	·992	+0·2
29·13	536·13	−22·1	·995	0·0
Apr. 1·12	539·12	−19·1	10·000	−0·1
3·94	541·94	−16·1	·004	+0·1
8·16	546·16	−13·4	·011	0·0
11·12	549·12	−12·4	·016	+0·1
14·98	552·98	−11·9	·021	+0·2

TABLE V (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
1997 Apr. 16·09	50554·09	−11·7	10·023	+0·3
18·09	556·09	−12·1	·026	−0·1
25·06¶	563·06	−12·6	·037	−0·2
28·08¶	566·08	−12·0	·042	+0·6
May 1·04	569·04	−12·6	·046	+0·2
July 19·94¶	648·94	−16·1	·170	+0·2
1998 May 4·09¶	50937·09	−21·2	10·614	−0·3
July 7·95¶	51001·95	−22·0	·714	−0·1
2000 Jan. 9·27	51552·27	−21·0	11·563	−0·5
Apr. 6·07	640·07	−22·1	·698	−0·3
May 13·97	677·97	−22·0	·757	+0·4
Aug. 5·89	761·89	−24·7	·886	−0·1
2001 Feb. 17·19	51957·19	−16·8	12·188	−0·2
May 12·03	52041·03	−18·2	·317	0·0
Dec. 20·24	263·24	−21·4	·660	0·0
2002 Mar. 1·18	52334·18	−22·4	12·769	+0·2
Apr. 4·12	368·12	−23·4	·822	−0·1
May 2·06	396·06	−24·2	·865	−0·1
June 22·97	447·97	−25·9	·945	+0·4
July 10·94	465·94	−26·8	·972	+0·2
26·89	481·89	−21·4	·997	−0·2
Aug. 6·87	492·87	−13·0	13·014	−0·3
2003 Mar. 16·14	52714·14	−18·5	13·355	+0·1
Apr. 19·06	748·06	−19·1	·408	0·0
May 16·01	775·01	−19·4	·449	+0·1
June 19·01	809·01	−20·2	·502	−0·3
July 15·91	835·91	−19·9	·543	+0·4
2004 Apr. 23·08	53118·08	−26·6	13·978	+0·3
May 7·05	132·05	−19·2	14·000	−0·1
June 21·99	177·99	−14·0	14·071	−0·2
July 9·91	195·91	−15·0	·098	−0·3
26·89	212·89	−15·3	·125	+0·1
2005 Jan. 9·25	53379·25	−18·3	14·381	+0·6
June 27·98	548·98	−21·0	·643	+0·2
2006 Feb. 16·16	53782·16	−17·0	15·003	+0·2
Mar. 1·16	795·16	−12·3	·023	−0·3

*Observed with original Cambridge spectrometer; wt. 1/10.
†Observation published by Yoss *et al.*[15]; wt. 0.
‡Observed with DAO 48-inch telescope; wt. 1/4.
§Observed with ESO *Coravel*; wt. 1/4.
¶Observed with Haute-Provence *Coravel*; wt. 1/4.

TABLE VI

Radial-velocity observations of HD 120531

Except as noted, the sources of the observations are as follows:
1973–1998 — Haute-Provence Coravel (wt. ½); 1999–2006 — Cambridge Coravel

Date (UT)	*MJD*	*Velocity* km s^{-1}	*Phase*	*(O – C)* km s^{-1}
1973 Apr. 25·06*	41797·06	+24·4	0·896	+0·5
1987 Mar. 26·08*	46880·08	20·9	1·973	−0·2
1989 Mar. 27·13	47612·13	27·0	2·128	−0·3
Apr. 29·09	645·09	27·9	·135	+0·6
June 1·98*	678·98	27·8	·143	+0·5
1990 Jan. 27·18	47918·18	26·7	2·193	−0·4
Feb. 14·40†	936·40	27·5	·197	+0·4
Apr. 5·05*	986·05	27·6	·208	+0·6
May 26·98*	48037·98	28·1	·219	+1·1
Dec. 27·28*	252·28	26·9	·264	0·0
1991 Jan. 30·16	48286·16	26·6	2·271	−0·3
May 22·99*	398·99	27·1	·295	+0·3
Dec. 19·25	609·25	26·2	·340	−0·5
1992 Jan. 18·25	48639·25	26·9	2·346	+0·3
Feb. 27·53‡	679·53	27·6	·355	+1·0
Apr. 24·11	736·11	26·8	·367	+0·2
June 25·99	798·99	26·2	·380	−0·3
Dec. 19·24	975·24	26·3	·417	−0·1
1993 Feb. 15·22	49033·22	26·5	2·430	+0·1
Mar. 19·17	065·17	26·4	·436	0·0
July 6·92	174·92	26·0	·460	−0·3
1994 Jan. 2·16	49354·16	25·0	2·498	−1·2
8·20	360·20	26·2	·499	0·0
May 2·09	474·09	25·8	·523	−0·3
Aug. 3·90	567·90	25·4	·543	−0·7
1995 Jan. 8·25	49725·25	25·6	2·576	−0·3
June 5·02	873·02	25·6	·608	−0·2
Dec. 27·23	50078·23	25·8	·651	+0·1
1996 Mar. 31·11	50173·11	25·2	2·671	−0·4
1997 Mar. 29·14§	50536·14	25·5	2·748	+0·3
July 19·96	648·96	24·9	·772	−0·2
1998 May 3·05	50936·05	24·4	2·833	−0·2
July 11·93	51005·93	24·6	·848	+0·1
1999 Dec. 27·24	51539·24	22·3	2·961	+0·4
2000 Feb. 3·60‡	51577·60	20·7	2·969	−0·7
Apr. 6·09	640·09	20·0	·982	−0·2
21·06	655·06	20·0	·986	+0·2
24·08	658·08	19·4	·986	−0·3
30·03	664·03	19·5	·987	−0·1
May 7·96	671·96	19·6	·989	+0·1
13·99	677·99	19·7	·990	+0·3
25·95	689·95	+19·5	·993	+0·2

TABLE VI (*concluded*)

Date (UT)			*MJD*	*Velocity* $km\ s^{-1}$	*Phase*	*(O−C)* $km\ s^{-1}$
2000	June	5·02	51700·02	+19·3	2·995	−0·1
		17·97	712·97	19·7	·998	−0·2
	July	16·96	741·96	22·0	3·004	−0·2
	Aug.	1·90	757·90	24·1	·007	+0·6
		2·89	758·89	23·8	·008	+0·2
		11·88	767·88	24·4	·010	+0·2
		29·83	785·83	24·8	·013	−0·4
	Sept.	20·79	807·79	26·4	·018	+0·4
		23·78	810·78	25·7	·019	−0·4
	Dec.	2·26	880·26	26·9	·033	−0·2
2001	Jan.	11·28	51920·28	27·1	3·042	−0·1
	Feb.	17·20	957·20	27·3	·050	0·0
	May	5·05	52034·05	27·5	·066	+0·1
	July	27·92	117·92	27·3	·084	−0·1
	Aug.	20·85	141·85	27·5	·089	+0·1
	Dec.	20·26	263·26	26·9	·115	−0·4
2002	Feb.	24·17	52329·17	27·8	3·129	+0·5
	Apr.	24·06	388·06	27·2	·141	−0·1
	Sept.	2·84	519·84	27·0	·169	−0·2
2003	May	10·02	52769·02	27·1	3·222	+0·1
2004	Mar.	31·11	53095·11	27·3	3·291	+0·5
	July	6·94	192·94	27·0	·312	+0·3
2005	Jan.	9·24	53379·24	27·0	3·351	+0·4
	July	18·92	569·92	26·5	·392	0·0
2006	Mar.	23·16	53817·16	+26·2	3·444	−0·2

*Observed with original Cambridge spectrometer; wt. ¼.
†Observed with ESO *Coravel*; wt. ¼.
‡Observed with DAO 48-inch telescope; wt. ¼.
§Observed with Cambridge *Coravel*; wt. 1.

Radial velocities and orbits

All the radial velocities available for the four stars are listed in chronological order in the respective Tables III–VI. They were all obtained by the author, apart from one published measurement[9] for HD 109484 and one[15] for HD 119334. As usual, the Haute-Provence and ESO velocities have been adjusted by +0·8 km s^{-1} from the values derived in Geneva on the post-2000 zero-point[17], in an effort to maintain the zero-point that has been used fairly consistently in this series of papers and whose basis is to be traced to an early investigation[18] made with the original photoelectric spectrometer. Again following what has become somewhat of a standard practice in these papers, the velocities obtained with the Cambridge *Coravel* over the last several years have been subjected to an empirical adjustment to bring them into systematic accord with the Haute-Provence data. It has become increasingly clear that, whereas no change is normally needed for the later K stars, negative adjustments are warranted for stars bluer than about early K, and that they increase numerically towards earlier types until they reach values that may approach -1 km s^{-1} for Am and early F stars. The stars treated in the present paper, all of which have much the same colour index, offer a particularly good opportunity to evaluate the offset at that colour, since they have all been observed many times with both the Haute-Provence and the Cambridge *Coravel*s, and in two cases there is also a large contribution from the original spectrometer itself. Independent assessments of the best offsets for each of the four stars result in values of 0·0, $-0{\cdot}3$, $-0{\cdot}2$, and $-0{\cdot}2$ km s^{-1}, respectively; the values are to be seen as being in quite good accord with one another, since the standard errors of their determinations are about 0·1 km s^{-1}. A mean of $-0{\cdot}2$ km s^{-1} has been adopted, and has been applied uniformly to all the measurements contributed to this paper by the Cambridge *Coravel*.

The formal purpose of attributing different weights to different sources of measurements is to equalize the weighted variances. There is no purpose in doing that in any but a broad-brush fashion, since a glance at tables of *F* ratios shows that with data sets of the sizes typically featuring in this series of papers (20–30 measurements per series, *cf.* Table I) differences of about a factor of 2 in the variances are necessary to be significant at the 5% level. Minor series of data can easily give very small apparent variances that do not truly characterize their sources, just through statistical accident in the absence of any large residuals. It is in any case dangerous to attribute too much weight to a minor data set, not only because its apparent variance may be grossly optimistic but also because it is liable to be very non-uniformly distributed: in the orbital application, a small number of data falling within a restricted interval of phase and having an ill-determined zero-point offset could produce appreciably erroneous values of the orbital elements, especially of e and ω.

In a case like the present, where several stars are considered, it is useful to take an overview of the whole *ensemble* when assessing weighting. More generally, when the same sources are used repeatedly, useful experience builds up concerning their relative reliabilities. For each of the four stars treated here, it is found that the residuals from the Cambridge *Coravel* are much smaller than those of the other principal sources. It should perhaps be remarked that there is no implicit claim that the Cambridge *Coravel* is a better instrument than the Haute-Provence one, of which it is in fact in many respects a copy, thanks to the gift by Dr. M. Mayor (the presiding genius behind the Haute-Provence *Coravel*[19]) of the design. The improved performance is probably attributable to increased integration times at Cambridge, since observing time on the home site is not at such a premium as it is on expeditions, and also to the fact that reductions at Cambridge are under the writer's own control. To the extent that they can be assessed, the residuals given by the instruments other than the Cambridge one tend to be comparable with one another, save that the original spectrometer usually (and not surprisingly) proves to be less accurate than the others. The upshot of investigations of the residuals given by the orbital solutions considered here is that, with the Cambridge *Coravel* taken as the unit of weight, the other sources could in general be attributed a weight of one quarter. A few exceptions have been made to that generalization, as follows. For HD 110376 and HD 119334, velocities from the original spectrometer have been weighted $^1/_{10}$ in place of $^1/_4$, and for HD 120531 the Haute-Provence data have been weighted $^1/_2$ instead of $^1/_4$. The orbital elements that follow for the four stars from the tabulated velocities and with the weightings just described are set out in Table VII, and the corresponding velocity curves are shown in Figs. 1–4.

TABLE VII

Orbital elements for the four stars

Element	*HD 109484*	*HD 110376*	*HD 119334*	*HD 120531*
P (days)	5182 ± 13	1275·5 ± 1·7	648·26 ± 0·05	4717 ± 31
T (MJD)	52827 ± 18	50997 ± 17	51835·57 ± 0·20	51723·0 ± 2·9
γ (km s^{-1})	−24·12 ± 0·06	−9·28 ± 0·04	−19·85 ± 0·04	+25·71 ± 0·06
K (km s^{-1})	6·63 ± 0·08	3·89 ± 0·06	7·50 ± 0·06	4·06 ± 0·07
e	0·401 ± 0·009	0·187 ± 0·015	0·800 ± 0·003	0·830 ± 0·006
ω (degrees)	208·0 ± 1·7	292 ± 5	273·2 ± 0·8	225·9 ± 1·9
$a_1 \sin i$ (Gm)	433 ± 6	67·1 ± 1·1	40·1 ± 0·5	146·6 ± 3·5
$f(m)$ ($M_\odot$)	0·121 ± 0·005	0·0074 ± 0·0004	0·00614 ± 0·00021	0·0057 ± 0·0004
R.m.s. residual (wt. 1) (km s^{-1})	0·32	0·26	0·24	0·28

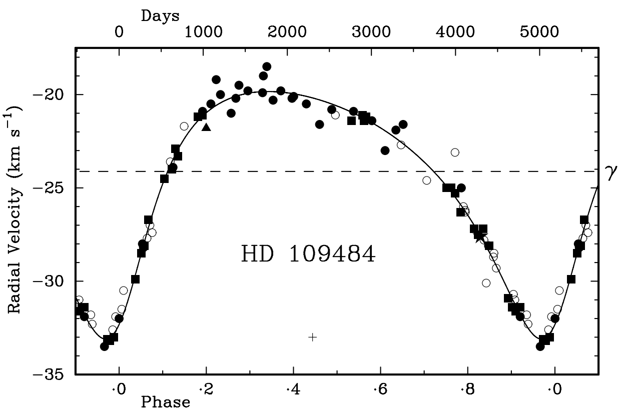

FIG. 1

The observed radial velocities of HD 109484 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. Filled squares represent radial velocities measured with the Cambridge *Coravel*, which received unit weight in the solution of the orbit. Other sources, which all received weight 1/4, are the original radial-velocity spectrometer at Cambridge (open circles), the Haute-Provence and ESO *Coravel*s (filled circles), the spectrometer at the Victoria 48-inch coudé (filled triangles), and the one at the Palomar 200-inch coudé, whose single observation is represented by a star symbol partly hidden at phase ·83. The one published velocity, obtained[9] from a 66-Å mm^{-1} photographic spectrogram at Kottamia, is indicated by the plus symbol and has been rejected.

Discussion

It has not been possible to detect a secondary dip in the *Coravel* traces of any of the stars. Except possibly in the case of HD 109484 that is not at all surprising, since the other three stars have very small mass functions, and two of them also have such small radial-velocity amplitudes that any secondary dip would be closely blended with the primary one and thereby masked. The mass function of HD 109484, a giant star, is as much as 0·12 $M_\odot$, and if the mass of the observed object is taken as 2 or 3 $M_\odot$ then that of the secondary has to be, as a minimum, 1·0–1·3 $M_\odot$. The companion is probably, therefore, of the nature of an F- or solar-type star. It could well be two or three magnitudes

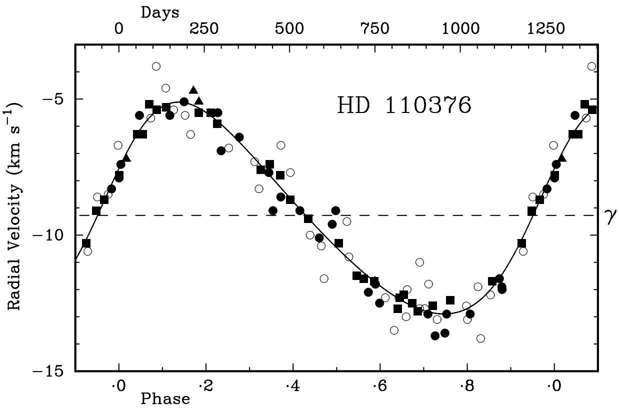

FIG. 2

As Fig. 1, but for HD 110376. In this case the 'original Cambridge' measurements were weighted only 1/10, the weighting of the other sources remaining as before, but there are no Palomar and no published measures to be plotted.

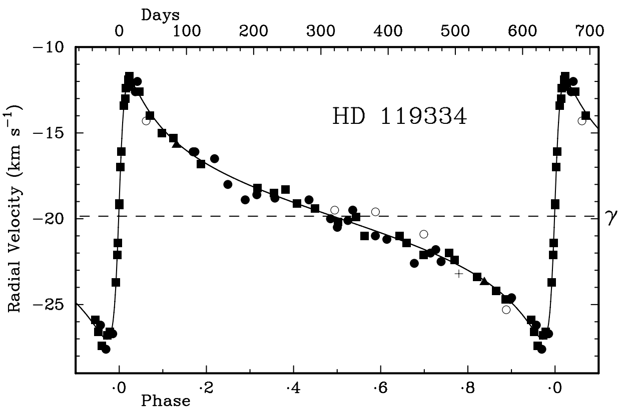

FIG. 3

As Fig. 1, but for HD 119334. The weightings of the different sources are the same as noted for HD 110376 in the caption to Fig. 2. There is one published velocity[15], obtained at the Victoria coudé and plotted with a plus symbol; it was not utilized in the solution of the orbit.

fainter than the primary, and the dip to be expected on radial-velocity traces would be proportionately smaller still, since the dips given by stars of such types in isolation are intrinsically only about half as strong as those given by late-type giants, and in some cases are also smeared out by rapid rotation. There is of course always the possibility that an unseen component in a binary or multiple system is itself a binary. A specific but unsuccessful effort to detect a second dip was made on 1993 February 14 with the Haute-Provence *Coravel*, when the system was close to a node of the orbit.

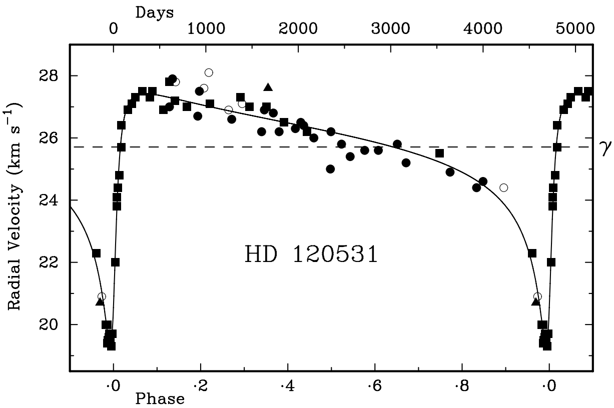

FIG. 4

As Fig. 1, but for HD 120531. In this case the Cambridge *Coravel* velocities were given unit weight as usual, but OHP measures were weighted 1/2 and all the others (including the 'original Cambridge' ones) 1/4.

HD 109484 has much the largest value of $a_1 \sin i$ of the four stars investigated here; it amounts to nearly 3 AU, so, since the star that has been observed is certainly a great deal brighter than its companion, the photocentric motion must be nearly as much as that, even if $\sin i \sim 1$. It follows that the astrometric orbit must have a major axis that is something like three times the parallax — about 8 ± 4 milliseconds of arc. That would be more than large enough to have been detected by *Hipparcos*, were it not for the fact that the satellite was only in operation for about one-fifth of a 14-year orbital cycle. As matters stand, the effect of orbital motion is probably visible in the discrepancy between the *Tycho 2* proper motion in right ascension, which represents an average over something like a century of astrometry, and the *Hipparcos* value, which is in effect a 'snapshot' determination. The values are $-10{\cdot}0 \pm 1{\cdot}1$ and $-4{\cdot}55 \pm 1{\cdot}40$ arc milliseconds, respectively, so they differ by three times their joint standard errors.

The $a_1 \sin i$ values for the other stars, expressed in AU, are about 0·45, 0·27, and 1·0, and should be reflected by astrometric motions of something like those factors times their respective parallaxes and so amount to about 13, 1, and 6 milliseconds, respectively. *Hipparcos* observations spanned 88% of an orbital cycle of HD 110376, so it is perhaps a bit surprising that they yielded only an 'acceleration solution' and not an actual orbit. Clearly the astrometric results on that system could be improved if the satellite measurements were re-discussed now in the light of the spectroscopic orbit presented here. HD 120531 was near apastron in its 13-year orbit during the *Hipparcos* mission, so it is not surprising that its ~6-millisecond orbit was overlooked.

The high eccentricities of the orbits of HD 119334 and HD 120531 call for notice: although eccentricities of 0·8 and above have now been found for 15 of the 250-odd stars whose orbits have been given in this series of papers, that proportion is clearly a lot smaller than would be expected if orbital eccentricities were randomly distributed between 0 and 1. It was fortunate that the Cambridge *Coravel* entered routine operation just in time to document the nodal and periastron passages of HD 120531 in 2000, although observing opportunities were scarce during the earlier part of the approach towards the node. By coincidence the two isolated initial observations of that star, made in 1973 and 1987, fall in the vicinity of that phase. The Cambridge *Coravel* had been operational briefly from time to time in late 1996 and early 1997, and made a substantial contribution to the documentation of the periastron passage of HD 119334 in 1997 April/May; owing to the more moderate period of that star in comparison with HD 120531, however, the observation of one particular periastron passage was not so critical.

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SPURIOUS ECCENTRICITIES OF EARLY-TYPE BINARIES

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The orbits of the early-type eclipsing binaries UW CMa, V453 Sco, V861 Sco, and V448 Cyg are often considered as eccentric, according to the appearance of their radial-velocity curves. Photometric evidence, however, strongly supports circular orbits. Deviations from circular spectroscopic orbits in the first three cases are nearly identical, and an explanation is offered here as being due to the combined effects of mass exchange and stellar wind in these hot, close binaries. However, there are no other cases among systems of spectral type B1 or earlier for which this so-called 'Barr effect' has been unambiguously proven, with the only possible exception of HR 8281 (HD 206267). Several new high-dispersion *CAT–CES* spectra of

UW CMa are presented to illustrate the strong influence of circumbinary matter on the line profiles as a suggested reason for the fictitious eccentricities. A more precise period for V453 Sco is given.

Introduction

It has long been suspected that the apparent eccentricity of a spectroscopic binary orbit can be spurious[1,2]. A typical case already mentioned by Struve and studied in detail by Batten[3] is U Cep (B7 V + G8 III–IV); SX Cas[4] is another example. In these two cases the spectroscopic eccentricity is non-zero, and the longitude of periastron ω is in the first quarter. In Alan Batten's book, warnings were given to be aware of this problem. If a binary is eclipsing and if the longitude of periastron ω differs from 90° or 270°, the phase of the secondary minimum is a clear indication of whether the spectroscopic eccentricity is true or not. It is therefore rather surprising that in several recent papers an orbit is referred to as eccentric, even if the light curves are symmetric. Four eclipsing binaries of early spectral type with non-zero spectroscopic eccentricity are listed in Table I. Their periods lie in the range 4 to 12 days. Circumbinary matter (CBM) is commonly expected to be present in these interacting systems, and photometrists always believed in circular orbits (see, *e.g.*, refs. 5–7 in the case of UW CMa). We suggest that the apparent spectroscopic eccentricity is due to additional absorption in spectral lines caused by the combined effects of gas streams and stellar wind in such luminous, mass-exchanging binaries.

Binaries with eccentric orbits should reveal evidence of apsidal rotation. For the range of periods mentioned and the early-type massive systems, the period of the apsidal rotation must be ~100–200 years or even shorter. The apsidal advance should therefore be observable in cases with longer coverage of radial-velocity data, and the reality of non-zero eccentricity can be judged also for non-eclipsing binaries.

TABLE I

Parameters of binaries with spurious eccentricities

Name	*HD*	*Sp. type*	*Period (days)*	*Eccentricity**	*Periastron longitude**	*Source ref.*
UW CMa	57060	O7 e	4·393	0·101	57°·8	9
V453 Sco	163181	BN0·5 Iae	12·00	0·102	65°·4	18
V861 Sco†	152667	B0 Ia	7·85	0·163	35°·9	21
V448 Cyg	190967	B1 Ib-II	6·52	0·039 ± 0·013	34°·5	26

* spectroscopic parameter
† *e* and ω according to the 'eccentric' solution, which had better residuals than the 'circular' solution

UW CMa

This binary has been subject of many spectroscopic studies — *e.g.*, by Stickland[8,9] and by Bagnuolo *et al.*[10], all based on *IUE* spectra. Stickland[8] discussed the possibility of a circular orbit, although in his more recent paper[9], as well as in a study by Hutchings[11], the eccentric orbit was accepted without reservation.

There are several papers devoted to photometry of this binary. Photoelectric photometry was obtained by Eaton[6], Herczeg *et al.*[7], and van Genderen *et al.*[12]; analysis of these data was also carried out by Leung & Schneider[13]. A good and more recent light curve is provided by means of *Hipparcos* photometry; it consists of 222 measurements and is displayed in Fig. 1. The ephemeris used to phase the data is that of Herczeg *et al.*[7]:

$$\text{HJD (Prim. Min.)} = 2440877{\cdot}563 + 4{\cdot}39336\,E.$$

Some *Hipparcos* data can be used to define several times of minima, and values calculated by the Kwee–van Woerden method are listed in Table II.

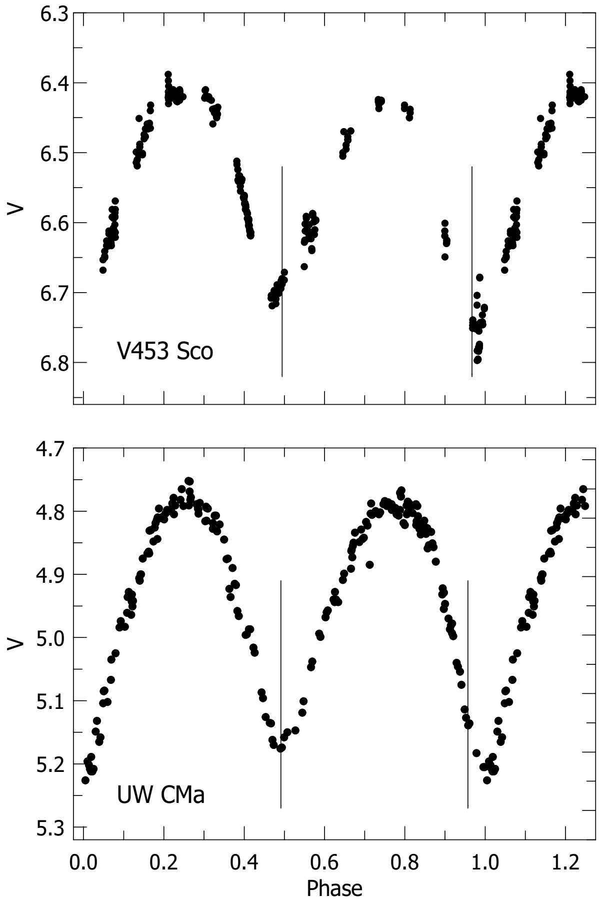

FIG. 1

Light curves of UW CMa according to *Hipparcos* and of V453 Sco according to Woodward & Koch. The vertical bars indicate the positions of minima as they would occur according to the spectroscopic orbits.

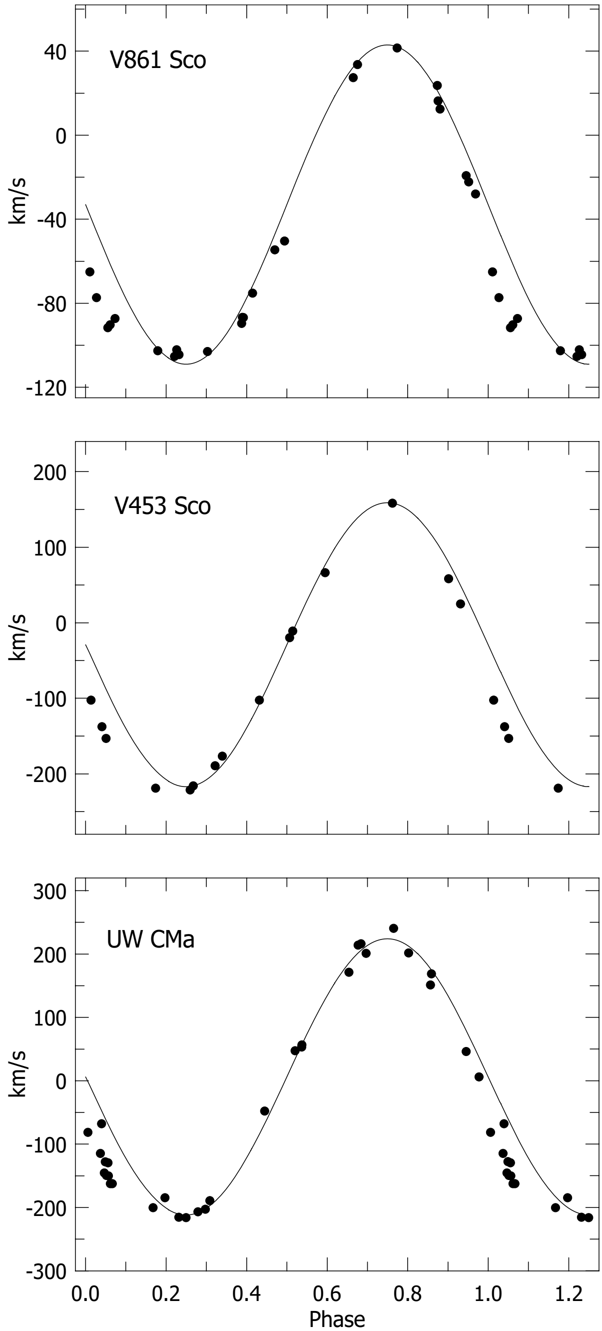

FIG. 2
Radial-velocity curves of UW CMa, V453 Sco, and V861 Sco; for parameters of the sine curves see text.

TABLE II

Times of Hipparcos minima of UW CMa

HJD −2 400 000	*Epoch*	*O−C* (*days*)
48649·454	1769	+0·037
48651·616	1769·5	+0·002
48653·789	1770	−0·011

Rather small values of O–C confirm the validity of this ephemeris; also van Genderen *et al.*[12] noted its correctness. The light curve is distorted and exhibits temporal changes — the shapes of minima differ in various epochs; nevertheless the curve does not bear indications of a non-zero eccentricity. Herczeg *et al.*[7] did not detect any shifts of the secondary minima from phase 0·5; also the phase of the secondary minimum in Table II is close to 0·5. In Fig. 1, phases of conjunctions as predicted by the spectroscopic orbit by Stickland[9] are marked. Clearly, this orbit is unacceptable on photometric grounds.

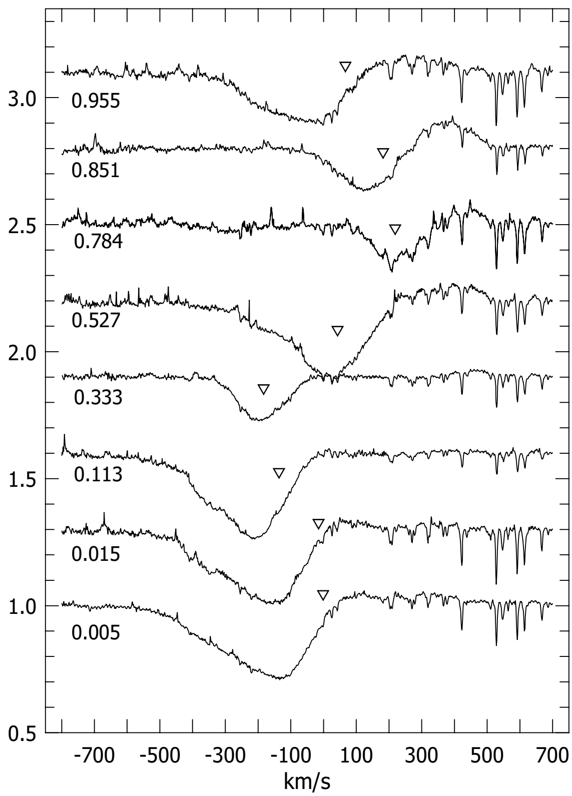

FIG. 3

CAT–CES spectra of UW CMa. Labels denote photometric phases; expected circular velocities are marked.

TABLE III

Journal of UW CMa spectra

HJD −2 400 000	*Phase*	*HJD* −2 400 000	*Phase*
45709·607	0·851	45713·706	0·784
45710·761	0·113	45714·460	0·955
45711·727	0·333	45714·677	0·005
45712·578	0·527	45714·724	0·015

The radial-velocity data — again following Stickland[9] — are shown in Fig. 2. The sine curve drawn according to the photometric ephemeris with an amplitude $K_1 = 218$ km s^{-1} and $V_\gamma = 6$ km s^{-1} fits the measured velocities very well, with the exception of the interval around the primary minimum: between phases 0·9 and 0·2, the measured velocities are more negative than the assumed circular orbital velocity, with a difference of up to about 80 km s^{-1}.

In spite of the brightness of UW CMa, no high-resolution study has been attempted yet. We present here eight so-far-unpublished *RETICON* spectra taken in 1984 in one echelle order of the *CAT–CES* spectrograph (ESO, La Silla). The spectral range covers the region around the He I λ5876 line at a resolving power of 50 000 with very high S/N (see Table III and Fig. 3). Exposure times were between 30 and 90 minutes. Although the phase coverage of these spectra is sparse they provide some useful information about the presence and properties of CBM in this system; see below.

Another strong argument against any non-circularity is the absence of apsidal rotation. If $e > 0$, then the period of this rotation — assuming reasonable approximate parameters of the binary components — can be estimated to be shorter than 100 years. But during 70 years of observations, no systematic change of periastron longitude has been observed. Though a slow change of ω might not be measurable accurately due to the error involved in the longitude determination, it can hardly be larger than about 20° per century, *i.e.*, the resulting period would be unacceptably long (Monet[14] was talking of an "infinite period for possible apsidal motion").

V453 Sco

V453 Sco is a similar case to that of UW CMa. For this binary only five times of minima have been published — see Table IV; another epoch, obtained from the ASAS–3 catalogue (Pojmański[15]), is also given. Due to the brightness of the star, many ASAS measurements differ strongly from the expected light curve, and even after deleting the obvious cases, some might disturb the curve. To calculate the normal minimum of epoch 944·0 in Table IV only data taken after JD 2453400 were used. This sample of times of minimum defines quite well a somewhat longer period than considered in other papers:

$$\text{HJD (Prim. Min.)} = 2442218{\cdot}73(3) + 12{\cdot}00608(3)\ E.$$

Recently a spectroscopic study of the binary was published by Josephs *et al.*[18]. The velocities listed there are displayed in Fig. 2 together with the sine curve phased according to our ephemeris. The radial-velocity curve is very similar to that of UW CMa, with a best solution for $K = 190$ km s^{-1} and

TABLE IV

Times of minima of V453 Sco

HJD −2 400 000	*Error* (*days*)	*Epoch*	*O−C* (*days*)	*Source ref.*
28357·65	0·05	−1154·5	−0·061	16
28429·79	0·07	−1148·5	+0·043	17
42212·64	0·04	−0·5	−0·087	19
42218·74	0·02	0·0	+0·010	19
48509·97	0·03	524·0	+0·054	*Hipparcos*
53552·42	0·01	944·0	−0·050	*ASAS–3*

$V_\gamma = -27$ km s^{-1}. In this case the negative residuals of velocities from a circular orbit reach ~ 60 km s^{-1} at phases between −0·1 and +0·2.

Photoelectric photometry of V453 Sco was published by Woodward & Koch[19] and Madore[20]. There are only 42 measurements contained in the *Hipparcos* catalogue. The phases of minima as expected according to the spectroscopic ephemeris are shown in Fig. 1 with the 'yellow' band photometry by Woodward & Koch. In spite of asymmetries of the curve, the 'spectroscopic' phases of minima are clearly unacceptable. Already Woodward & Koch have noted that "the displacement of the minimum near phase 0·5 is in the sense opposite to that predicted by the spectrographic eccentricity and longitude of periastron". Josephs *et al.* discussed the period of apsidal rotation. As in the case of UW CMa, for V453 Sco the 'observed period' is too long, namely a factor of six larger than the theoretical period, which again casts strong doubts on the assumption of a non-zero eccentricity.

V861 Sco

In this case Stickland & Howarth[21], using *IUE* data, got an eccentric orbit as all previous investigators did. But as judged from the light curve they suggested that the real orbit might actually be circular, and a spurious eccentricity might be caused by tidal distortion and eclipses. Hutchings noted[11] that in the case of short-term radial-velocity variations the observations could also be compatible with zero eccentricity. However, careful work by Lloyd[22] excluded any short-term variability. In Fig. 2 the *IUE* velocities according to Stickland & Howarth are plotted. The similarity with UW CMa and V453 Sco is obvious. The parameters of the sine curve are $K_1 = 76$ km s^{-1}, $V_\gamma = -33$ km s^{-1}.

For this binary, only times of minima by Cousins & Lagerwey[23] and *Hipparcos* were published: HJD 2433823·234 and 2448507·300, respectively. An estimation using ASAS–3 (accompanied by the same problems as in the case of V453 Sco) is HJD 2452706·13, which means that the ephemeris by Howarth[24]

$$\text{HJD (Prim. Min.)} = 2433815{\cdot}386 + 7{\cdot}84825\, E$$

is still valid.

The *B* light curve by Cousins & Lagerwey was solved by Howarth[24]. He assumed that the primary component fills its Roche lobe — certainly an acceptable assumption, since the deformation of the primary component must be significant. The possible range of binary parameters is large. According to Howarth the best solution is obtained for $T_{pri} = 25000$ K, $q = 1/3$. Then $T_{sec} = 15200$, $i = 77 \cdot 3$, $r_{sec} = 0 \cdot 155$. Howarth suggested that the secondary is of type B2 V (corresponding to its radius and mass). Then there is a discrepancy for the temperature, since T_{eff} of a B2 V type star should be ≈ 22000 K; perhaps the observed low temperature is due to a CBM envelope. There are more recent photometric data: *uvby* and Hβ measurements by Bunk & Haefner[25] (only a graphical representation of normal points was published) and by *Hipparcos* (unfortunately only 47 measurements). No apparent changes of the light curve are noticeable. Note that the shape of the light curve is again very similar to those of UW CMa and V453 Sco (regarding the steeper ingress to, than egress from, the minimum).

V448 Cyg

The binary V448 Cyg might be another comparable case. Spectroscopy was discussed in a classic paper by Petrie[26] — based, of course, on photographic spectra. The eccentricity was found to be $0 \cdot 038 \pm 0 \cdot 013$, *i.e.*, any deviation from zero being at least uncertain. Eccentricity is not noted in the more recent paper by Harries *et al.*[27] because their spectroscopic material was not appropriate to determine it. The light curve is quite symmetric according to *Hipparcos* data. Older terrestrial photometry is rather noisy (Hartigan & Binzel[28], Kumsiashvili & Kochiashvili[29]), but does not reveal any evidence of eccentricity, in spite of a clear variability of the light curve on a longer time scale. An attempt to solve the light curve was recently published by Kumsiashvili *et al.*[30], with hot spots needed to improve the fit. However, it is expected that hot spots would raise the luminosity at the far-UV end of the *IUE* spectra, which is not observed.

Other common characteristics

Although the solution of light curves in our four cases is difficult due to the light-curve asymmetries and variability, it appears that, except for V448 Cyg, the primary components are always hotter and more massive and fill their Roche lobes (the loser being the star eclipsed at phase zero). In V448 Cyg it is the cooler, more massive component which fills the lobe (and therefore the velocities of the well-observable lines are positive after the primary eclipse). All light curves are remarkably similar, not only the shape (with steeper ingresses to minima than egresses), but also the amplitudes of the curves of V453 Sco and UW CMa are nearly identical. Therefore it can be suspected that the ratios of radii, temperatures, and luminosities also have to be very similar.

The four systems investigated reveal several other common characteristics: the secondary components are nearly undetectable in the spectra, or their radial velocities behave in an unexpected way (V448 Cyg). Also the spectra as well as the light curves exhibit pronounced variability independent of phase — obviously due to the presence of circumbinary matter. The suggestion that the eccentricities in all these cases are spurious is also supported by the values of periastron longitude derived by the spectroscopists — ω is always found in the first quadrant. Therefore, it is not possible to agree with Vanbeveren *et al.*[31,32] who used V453 Sco and V448 Cyg as a proof that "the binaries after mass exchange can have eccentric orbits". This hypothesis is — with high probability — not true, at least for early-type binaries with periods shorter than about 20 days. Apparently, orbital circularization by tidal interaction and mass transfer is quite an effective process.

Howarth[33] has investigated the distribution of the longitude of periastron for spectroscopic binaries. He found a bias with a preferred ω of ~100° for systems with periods < 3 days, a phenomenon described as the 'Barr effect'. The usual explanation is a systematic distortion of spectral-line profiles by gas streams in those close binaries, which causes a deformation of the radial-velocity curves such that their shape can be fitted under the assumption of a spurious eccentricity and values of ω within a certain range. However, the results by Howarth — that the Barr effect is present only among binaries with periods shorter than 3 days — seem not to apply to the earliest-type stars. In the new catalogue of spectroscopic binaries, SB9[34], there are only a few such short-period binaries with non-circular orbits, and in all cases where the eccentricity is well established apsidal rotation is present (CW Cep, Y Cyg, V478 Cyg); *i.e.*, among the early-type binaries with periods < 3 days the Barr effect is not observed.

Mason *et al.*[35] examined a sample of 30 binaries of spectral type O, mostly with longer periods (median value 6·1 days), for evidence of the Barr effect and found that statistically the most probable value of ω is 72°. However, in such a small sample, three or four binaries might be responsible for the excess of longitudes around that value. There are several binaries with known apsidal rotation (Mayer *et al.*, in preparation), which by chance have their periastron longitudes in the pertinent interval at present: *e.g.*, HD 152218, 152219, and 152248 have longitudes 81°, 92°, and 77°, respectively.

Besides the four cases discussed here there might exist other binaries which show the Barr effect, but probably not among eclipsing systems. A search of the SB9 catalogue revealed several non-eclipsing candidates. The accuracy of e and ω is often low, and we found only HR 8281 (HD 206267; type O6·5, period 3·710 days) as a promising candidate. For this star Stickland[36] obtained $e = 0{\cdot}119 \pm 0{\cdot}013$, $\omega = 13° \pm 6°$. To prove (or exclude) the reality of the eccentricity, it would be necessary to calculate the rate of the apsidal advance. Radial velocities already cover an interval 80 years long; but Stickland found that the system is triple and the old velocities are useless due to blending with the third-star lines. The case is therefore an inconclusive one. Other related binaries are AO Cas and V640 Mon (HD 47129, Plaskett's star). The secondary velocities of V640 Mon behave in the same way as those of V448 Cyg. RY Sct is a similar system, too. However, in these latter cases, the orbits are commonly considered as circular.

Presence of circumbinary matter

The classical Barr effect, as it occurs among Algol-type binaries, was described, *e.g.*, by Kríz & Harmanec[37]. Referring to their Fig. 1, some remarks based on the high-resolution *CAT–CES* spectra of UW CMa can be made: (*i*) There is nearly no trace of a secondary component of He I λ5876; only at phase 0·784 a shallow depression at λ5870·3 (−270 km s^{-1}) is discernible, which is perhaps the expected secondary-line position. This finding agrees with notes by Stickland[9], who detected possible secondary lines in the phase interval 0·6–0·9. (*ii*) In spectra taken at phases 0·35–0·05, emission with positive velocity up to 400–500 km s^{-1} is present. This is usually ascribed to an optically thin stellar wind producing the common P-Cygni-type line profiles. In hot interacting binaries the situation is much more complicated. There we have to deal with colliding stellar winds originating from both components, eclipsing of parts of the emitting envelope by the companion star, and presence of streams and rings of matter between and around the stars; emission can also be connected with gas streams leaving the system through the L_2 point. (*iii*) When the line positions expected in the case of a circular orbit (Fig. 2) are compared with the observed profiles, it is clear that most of the profiles are deformed by the mentioned emission on the red wings and by additional absorption on the blue side. Only the profile at phase 0·784 agrees with an undisturbed circular velocity. The blue absorption is very pronounced at phases 0·955–0·113. Clearly, the 'radial velocity' measured at these phases is affected by non-photospheric absorption. The effect of the presence of CBM on the He I λ5876 line is strong; more lines would be needed to establish the probable photospheric profile. By subtraction one could then hope to construct the contribution of CBM to the line profiles and thus extract information about its structure and dynamics.

The stars discussed here are much hotter than common Algols and possess strong stellar winds. There are certainly considerable complications due to the combined effects of streams of matter and stellar winds, especially in the collision zones. Recent progress has been made on the theoretical side by the development of numerical modelling techniques for the calculation of the gas dynamics of mass transfer in close binaries, *e.g.*, by the work of Bisikalo and others (see Bisikalo[38] and references therein) or MHD techniques. The shape of the stream might differ from the shape assumed for systems of later spectral type. The cone through which the stream leaves the L_1 point is wider, and the stream experiences deformation and acceleration by the stellar wind, with the result that most of the stream can leave the system somewhere around the secondary component. The CBM velocity has to be higher than the escape velocity at about $2R_2$ (~300 km s^{-1}), in accordance with the observed line widths. Also during the primary minimum — which is only partial in the first three cases discussed here — the stream is seen projected against the disc of the loser, *i.e.*, it is present also above/below the orbital plane; then the density and velocity of the stream deform the spectral lines in such a way that they give more negative values. Very probably the effect differs in various lines due to different temperatures of the stream and photosphere (the stream being cooler). The transverse components of the stream velocity can explain the abnormal velocities of the secondary component of V448 Cyg as shown, *e.g.*, by Petrie[26] for phases out of quadratures (it partly applies also to data by Harries *et al.*[27]: *cf.* the velocity at phase 0·85). The presence of the stream in a wide phase interval can also explain the invisibility or only partial visibility of the secondary lines in UW CMa and V453 Sco.

Conclusions

We suggest that in all the cases examined here the eccentricity is spurious. A possible reason is the line deformation by CBM. However, a detailed description of the CBM distribution and motion is still missing. It is yet to be found how the other parameters of the radial-velocity curve, namely K_1 and K_2, are affected. High-dispersion, high S/N spectra are needed for a more rigorous treatment of this problem.

Acknowledgement

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CORRESPONDENCE

To the Editors of 'The Observatory'

On the Statistical Model of Growth by Accretion

The recent paper by Basu & Jones[1] claims to have derived a *new* distribution to model growth by accretion. The probability density function (pdf) of this *new* distribution is given as:

$$f(x) = \frac{\alpha}{2} \exp\left(\alpha\mu_0 + \frac{\alpha^2\sigma_0^2}{2}\right) x^{-(1-\alpha)} \operatorname{erfc}\left[\frac{1}{\sqrt{2}}\left(\alpha\sigma_0 - \frac{\ln x - \mu_0}{\alpha_0}\right)\right] \quad (1)$$

for $x > 0$, $\alpha > 0$, $\sigma_0 > 0$, and $-\infty < \mu_0 < \infty$ (see equation (7) in Basu & Jones[1]), where erfc denotes the complementary error function defined by

$$\operatorname{erfc}(x) = 1 - \frac{2}{\sqrt{\pi}} \int_0^x \exp(-t^2)\, dt.$$

The paper by Basu & Jones claims that the equation (1) "... represents a new three-parameter (μ_0, σ_0, α) probability density function which tends ....". We would like to point out that the distribution given by (1) has been known since the 1990s. In fact, (1) is the pdf of the Pareto Lognormal (PL) distribution introduced by Colombi[2]; see also Chapter 3 of Kleiber & Kotz[3]. The PL distribution arises as the distribution of the product $X = YZ$ of two independent random variables, Y following the Pareto distribution given by the pdf

$$f(y) = \frac{\alpha}{y^{\alpha+1}}$$

(for $y > 1$), and Z following the two-parameter lognormal distribution given by the pdf

$$f(z) = \frac{1}{\sqrt{2\pi}\sigma_{0_z}} \exp\left(-\frac{(\ln z - \mu_0)^2}{2\sigma_0^2}\right)$$

(for $z > 0$). The kth moment of the PL distribution exists for $k < \alpha$ and equals

$$E(X^k) = \frac{\alpha}{\alpha - k} \exp\left(k\mu_0 + \frac{k^2{\mu_0}^2}{2}\right).$$

In particular, the mean, variance, skewness, and the kurtosis of the PL distribution are

$$E(X) = \frac{\alpha}{\alpha - 1} \exp\left(\mu_0 + \frac{{\mu_0}^2}{2}\right) \qquad (\text{for } 1 < \alpha),$$

$$\mathrm{Var}\,(X) = \frac{\alpha}{\alpha-2}\exp\,(2\mu_0 + 2\mu_0^2) - \frac{\alpha^2}{(\alpha-1)^2}\exp\,(2\mu_0 + \mu_0^2) \qquad (\text{for } 2 < \alpha),$$

$$\mathrm{Skewness}\,(X) = \left\{ \frac{\alpha}{\alpha-3}\exp\left(3\mu_0 + \frac{9\mu_0^2}{2}\right) - \frac{3\alpha^2}{(\alpha-1)\,(\alpha-2)}\exp\left(3\mu_0 + \frac{5\mu_0^2}{2}\right) + \frac{2\alpha^3}{(\alpha-1)^3}\exp\left(3\mu_0 + \frac{3\mu_0^2}{2}\right) \right\} \Big/ \left\{ \mathrm{Var}\,(X) \right\}^{3/2}$$

(for $3 < \alpha$), and

$$\mathrm{Kurtosis}\,(X) = \left\{ \frac{\alpha}{\alpha-4}\exp\,(4\mu_0 + 8\mu_0^2) - \frac{4\alpha^2}{(\alpha-1)\,(\alpha-3)}\exp\left(4\mu_0 + \frac{11\mu_0^2}{2}\right) + \frac{6\alpha^3}{(\alpha-1)^2\,(\alpha-3)}\exp\,(4\mu_0 + 3\mu_0^2) - \frac{3\alpha^4}{(\alpha-1)^4}\exp\,(4\mu_0 + 2\mu_0^2) \right\} \Big/ \left\{ \mathrm{Var}\,(X) \right\}^{2}$$

(for $4 < \alpha$), respectively. Similarly to the Pareto distribution, the PL family is closed with respect to the formation of moment distributions. Colombi[2] showed that (1) is unimodal and provided an implicit expression for its mode. He also discussed sufficient conditions for Lorenz ordering and derived the Gini coefficient, which is of the form

$$G = 1 - \mathrm{erfc}\left(\frac{\sigma_0}{2}\right) + \frac{\exp\,(\alpha^2\sigma_0^2 - \alpha\sigma_0^2)}{2\alpha-1}\,\mathrm{erfc}\left(\frac{2\alpha\sigma_0 - \sigma_0}{2}\right),$$

an expression that is seen to be decreasing in σ. Moreover, Colombi[2] discussed an application of (1) to Italian family incomes from 1984 to 1986.

The purpose of this correspondence is not just to point out any shortcomings of the paper by Basu & Jones[1]. We feel the references mentioned above can help the readers of this journal, and authors in general, in making appropriate choices with regard to similar modelling problems and help to prevent such oversights in the future.

Yours faithfully,

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REVIEWS

Letters to a Young Mathematician, by I. Stewart (Basic Books, London), 2006. Pp. 224, 20·5 × 12·5 cm. Price £13·99 (hardbound; ISBN 0 465 08231 9).

When I was an undergraduate, studying for a joint degree in mathematics and physics, I read and enjoyed G. H. Hardy's classic book *A Mathematician's Apology*, written in 1940. Going back to it now, I flinch at his attitude that a mathematician should write about mathematics only when he (and in those days that was the normal pronoun) is unable any longer to produce new mathematics himself. Times and attitudes have changed since then, as Ian Stewart points out in his preface to this delightful book, which in some sense is a modern version of Hardy's book but might better be described as a celebration of mathematics than an apology for being a mathematician.

The book purports to be a series of twenty-one occasional letters to 'Meg', an aspiring mathematician, first as she considers whether to study mathematics at university and then as she develops her career through to a tenured position. It becomes clear from the spelling, and the use of the abbreviation 'math', that the book is aimed at an American readership, and indeed that Meg herself is American, but every essential aspect of the book is of relevance to all mathematicians, and to those interested in mathematics, and many of Ian Stewart's examples are drawn from his own UK experience. Of course, the letters are just a useful peg on which to hang his ideas and comments, and some of the longer letters diverge from the natural style of a letter, but the whole book is as easy to read as a set of letters and I can thoroughly recommend it.

There is a natural progression of topics as Meg moves from high school through undergraduate and postgraduate study to postdoctoral and faculty positions, with each letter being the answer to an implied question from his correspondent. The questions of why one should study mathematics at all, and what it is, give way to more subtle questions about how to learn maths, the nature of proof, the fact that mathematicians are able to say definitively that something is impossible, and practical matters relating to a career in mathematics — how to choose a PhD supervisor, the difference between pure and applied maths, and how to become integrated in the mathematical community. It is clear that (unlike Hardy) Ian Stewart loves communicating mathematics as much as he loves doing it and his enthusiasm shines out from every page. Recommend this book to your students who find maths a struggle — but be prepared for some of them to defect to the mathematics department as a result! — ROBERT CONNON SMITH.

Physical Foundations of Cosmology, by V. Mukhanov (Cambridge University Press), 2005. Pp. 421, 25·5 × 18 cm. Price £40/$70 (hardbound; ISBN 0 521 56398 4).

Why is the Universe so full of complex structures, from stars and planets to galaxies and their large-scale distribution in superclusters? We don't know for sure, but for the past quarter-century cosmologists have been working with an audacious idea in which everything we see is a hugely magnified set of quantum fluctuations. In other words, the same physics of small-scale uncertainty that governs the operation of atoms could be responsible for all the features of the Universe that astronomers are able to study. The credit for developing this remarkable hypothesis of 'inflationary cosmology' is shared between many people, but Slava Mukhanov was one of the first to suggest this quantum origin of structure, in 1981. In 1992, he co-authored a hugely influential review article on cosmological fluctuations, including both their quantum generation and their subsequent evolution. With this pedigree as innovator and pedagogue in cosmology, I was pleased to see that Mukhanov had written a textbook. Even 25 years later, the details of the generation of inflationary fluctuations are not simple for the novice to grasp, nor have we progressed all that far in testing whether this neat idea is actually the correct explanation. Therefore a guide from an authority in the subject is to be welcomed.

The preface states that the book is designed for "serious students in physics and astrophysics", but that it should be useful for undergraduates and does not assume preliminary knowledge in any specialized field. This is true in that the discussion tends to build up from first principles — but the rate of ascent is very rapid in a number of places. I feel that only the most exceptional undergraduates could cope with these without having previously been through a fairly high-level course in cosmology, and probably also one in quantum field theory. Indeed, the same is probably true of the majority of PhD students, certainly those in the UK system.

With these caveats, Mukhanov has written a superb book, which is distinguished by its willingness to dig into technical details that are often skipped or simplified in other treatments. It can be considered in five parts: an overview of cosmological models and the hot Big Bang (Chapters 1, 2 & 3); a primer on relevant particle physics (Chapter 4, which occupies a quarter of the book); inflation, including fluctuation generation (Chapters 5 & 8); fluctuation growth (Chapters 6 & 7); and comparison of the theory with data on the microwave background (Chapter 9). There are very good things to be found in all these sections, with perhaps the most novel material in Chapters 3 and 9. Here, Mukhanov has clearly worked very hard to rethink from first principles two of the key areas of cosmology: primordial nucleosynthesis and anisotropies in the microwave background. In both cases, he makes a huge effort to come up with analytic arguments that not only illuminate why numerical studies yield the results that they do, but also allow one to match the numerical results to high accuracy. These sections need to be followed in detail, and demand a certain amount of stamina — but Mukhanov clearly got a lot of fun out of making this approach work, and his enthusiasm comes over well.

However, probably the main reason people will buy this book is for the material about the generation of cosmological perturbations; certainly, this is the subject I was most looking forward to reading about. This topic comes in Chapter 8, prefaced by two chapters on growth of perturbations in the

Newtonian and relativistic approximations. The former is quite brief, but the relativistic chapter is substantial. It takes 34 pages to cover the gauge-independent perturbation formalism, including quite a bit of detail on coupled perturbations in dark matter and baryons plus radiation. This is well done, and will repay detailed study. A chapter of similar length follows on the crux of the subject: how quantum fluctuations in the scalar field that drives inflation make the transition to classical density perturbations. With good pedagogical style, Mukhanov derives the main result twice — the second approach being more formal and rigorous. The first approach covers eight pages and effectively treats the scalar field classically, apart from setting an initial amplitude at the level expected from quantum fluctuations; the second approach is a more consistent quantum treatment. As with many things in this book, the level is high and will be of great value to readers who are already familiar with many of the arguments. However, for students coming to this subject fresh, one would prefer to see the eight-page warm-up argument taken at a gentler pace, leaving the full details for another day.

In short, there is much to admire in this book, and it will undoubtedly be devoured in detail by theoretical cosmologists working at the frontiers of structure formation — both advanced graduate students and postdoctoral researchers. However, I think that students at an earlier stage will find Mukhanov too forbidding without some prior introduction and help in coming to terms with the subject. A particular difficulty, which could have made all the difference, lies in the problems. These are liberally sprinkled throughout the book (16 in the critical Chapter 8 alone), and they often concern detailed calculations which are an essential part of the arguments being made. Sadly, these are presented with barely a hint of how to proceed, and I do find myself questioning the point of repeatedly facing the student with problems that they are probably unable to solve. Although it is unquestionably true that students must try calculations for themselves in order to learn, these efforts are so much more effective when backed up by worked examples in the Landau-Lifshitz style. Of recent cosmological textbooks, those by Padmanabhan show the virtues of this approach. Perhaps one day Mukhanov will produce a second volume with these missing hints; this would greatly increase the educational value of what is in any case a wonderful contribution to the cosmological literature. — JOHN PEACOCK.

The Three-Body Problem, by M. Valtonen & H. Karttunen (Cambridge University Press), 2005. Pp. 345, 25·5 × 18 cm. Price £45/$80 (hardbound; ISBN 0 521 85224 2).

There are few books about the few-body problem, and any addition is to be welcomed, especially one by such expert authors. This one is thoroughly rooted in astrophysical applications, such as black-hole interactions in galactic nuclei and comet capture. The approach is also thoroughly practical, as the authors are fearless in making approximations, and even guesses, in order to get to an answer, provided that, at the end of the day, the result is justified by numerical calculations. This does not mean that the authors skimp details; on the contrary most derivations are given in full. Each line follows without effort from the previous one, and readers won't spend hours with sheets of paper doing the necessary manipulations, though this depends in places on how much they know about Bessel functions, spherical trigonometry, and so on.

Numerous topics are covered, such as cross sections for exchange in interactions between a binary star and a field of single stars. Therefore any astrophysicist who needs a quick formula for an application would do well to start here. On the other hand the niceties of the three-body problem aren't given as much attention as a purist might like. For instance, there is a formula for the lifetime of a triple system, which accounts well for the distribution of lifetimes obtained from computer simulation. But it gives a finite mean lifetime, whereas I think strictly the mean is infinite. — DOUGLAS HEGGIE.

Saturn and How to Observe It, by J. L. Benton, Jr. (Springer, Heidelberg), 2006. Pp. 184, 23·5 × 17·5 cm. Price £19·50/$29·95/€24·95 (paperback; ISBN 1 852 33887 3).

Julius Benton has directed the ALPO (Association of Lunar and Planetary Observers) Saturn Section since 1971, and his enthusiasm for observing the ringed planet is evident upon every page. This book begins with 46 pages of background detail about Saturn, and 38 pages of useful telescope data. How to choose an instrument, details of resolution tests, contrast theory, colour perception, colour filters, *etc.*, are all to be found here. Although this detail (some of it rather basic) seems out of place in a specialist guidebook, it will serve as a solid background for planetary work in general.

Successive chapters describe the various global and ring features, and reproduce (in colour) many beautiful, well-chosen illustrations. There are drawing templates, reporting forms, longitude tables, *etc.* There are no obvious typographical errors, but Figures 4·13 and 4·14 have been inverted. The style is very clear, though sometimes a little verbose. Pages 111–163 cover aspects of observing, such as measuring belt latitudes or carrying out CCD or webcam imaging. There is much about what to look for and details of where data might be reported (the British Astronomical Association (BAA) and ALPO are the only groups mentioned). The bibliography, though extensive, is heavily slanted towards ALPO publications.

As a systematic Saturn observer since 1972, and one who has published extensive details of long-term albedo changes on the planet, I was expecting to find detailed observational *results* that have been achieved since Alexander's 1960s classic, *The Planet Saturn*. A wonderful opportunity for updating Alexander (in Benton's disappointingly brief Chapter 4) has been missed. One looks in vain for details of any of the rotation periods published (for example by the BAA) of such features as the Equatorial Zone (EZ) white spots of the 1990s. There are no drift-charts reproduced at all. Benton fails to mention that the 57-year periodicity of the EZ white spots (1876, 1933, 1990) is mirrored by the N. Temperate Zone (1903, 1960). He does not discuss global colour changes (where the hemisphere returning to sunlight is bluer than the already sunlit one). What about actual belt latitudes derived by telescopic observation? Such long-term studies have been published, but again are not cited. I had expected more details of the next ring-plane crossings, and an explanation of why the near-edgewise rings are often of different apparent lengths on each side of the globe.

This book will be of most value to the new planet-watcher. The experienced observer will be less likely to purchase this latest addition to the Springer stable. — RICHARD MCKIM.

An Acre of Glass: A History and Forecast of the Telescope, by J. B. Zirker (Johns Hopkins University Press, Baltimore), 2005. Pp. 344, 23·5 × 16 cm. Price £20 (hardbound; ISBN 0 801 88234 6).

Late in 2005, an international review panel met to consider the concept design for *OWL*, the 100-m *OverWhelmingly Large Telescope* proposed by the European Southern Observatory and others as the 'Next Big Thing' in optical astronomy. On the whole, the review was favourable, but a number of high-risk areas were identified. The eventual outcome was that *OWL*'s proponents were encouraged to push forward with a more modest design of as-yet unspecified aperture, to be known as *E-ELT* — the *European Extremely Large Telescope*. Thus, *OWL*, which had been wowing us with the audacity of its design (and its name) since the late 1990s, finally disappeared from the working vocabulary of the large-telescope world.

This was no surprise, of course. How many other ambitious telescope projects had bitten the dust or metamorphosed into something else over the past few years? In a litany of pragmatic transformations, *SELT* (the *Swedish ELT*) had become *Euro50*, *MAXAT* (the *Maximum Aperture Telescope*) had become *GSMT* (the *Giant Segmented Mirror Telescope*), *GSMT* itself had subsequently merged with *CELT* (the *California ELT*), and *VLOT* (yes, the *Very Large Optical Telescope*) to become *TMT* (the *Thirty Metre Telescope*)… and so on. If nothing else, this bewildering cavalcade of projects serves to underline the wealth of ideas coming from the fertile minds of telescope engineers. However, like many aspects of cutting-edge scientific technology, it is very difficult to keep track of.

Into this morass of shifting sand has stepped the brave author of *An Acre of Glass: A History and Forecast of the Telescope*, J. B. Zirker. His stated purpose is to introduce the engineers and astronomers involved with these (and many other) projects, find out why they want ever-larger telescopes, and "learn how these glorious instruments will be built". And on the whole — given the maze of projects he is confronted with — he has done a good job. The book is truly compendious in its account of modern optical instrumentation, and the engineering is nicely placed into a scientific context. Indeed, one of the book's great strengths is its constant reference to the astronomical problems that all this hardware is intended to address.

In the prologue, we are told that Chapters 1 and 2 present a glance backwards at the first three centuries of the telescope, reminding us that the book does claim to be a 'history'. The glance, however, is disappointingly cursory, consisting of a brief excursion through the well-worn highlights of the story. Worse — Chapter 1 contains several errors (principally of names and dates), and is illustrated with only the most rudimentary diagrams to demonstrate the optical principles involved. It pays no heed to the wealth of historical scholarship available today and, indeed, most of the chapter could have come from Henry King's *History of the Telescope* of 1955. It is only with Chapter 2 ('The age of Hale') that confidence is restored — although even here, the brief section on Schmidt telescopes is a bit shaky in its accuracy.

In Chapter 3 ('New windows on the Universe'), the book gets into its stride, setting out the background to modern multi-wavelength astronomy clearly and engagingly. It forms an excellent springboard for the accounts of the rise of the great optical-astronomy centres and the *Hubble Space Telescope* that follow. The book then treads a nicely-balanced path through competing mirror technologies, the current generation of telescopes, adaptive optics, interferometry, and so on. Frequent references to the individuals most closely involved with these endeavours enliven the text, suggesting that in many cases they are personally known to the author — a nice touch.

The book is strongest in its presentation of the big picture, and readers are left in no doubt as to the sea changes taking place in astronomy. On the scale of fine detail, however, the sheer volume of material means that slips are almost inevitable. For example, the $13·5-million price tag quoted for the *Hobby-Eberly Telescope* neglects the fact that in order to make it work, edge-sensors had to be fitted to each of its 91 mirror segments, increasing its cost by something like 50 percent. Shifting sands again. It came as no surprise to this reviewer to find the name of his home town, Coonabarabran, misspelled. But did the Hubble constant really converge to a value of 57 $\mathrm{km\,s^{-1}\,Mpc^{-1}}$ during the 1990s? Eventually, the book winds up with ELTs and future space projects, leaving the reader feeling that Zirker must have dipped into every single volume of *SPIE Proceedings* (the standard repository for technical papers on astronomical instrumentation) during its preparation.

In fact, that may not have been the case. Surely, had he been so deeply immersed in the literature, the author would have provided us with a bibliography — but there are no references whatever. This suggests that the information in the book has come from a very wide range of sources — web sites, newsletters, magazine articles, discussions with engineers and scientists, coffee-room anecdotes, and so on. To some extent, that is the nature of the game when describing a fluid and rapidly-evolving technology. But the lack of references must compromise the value of the book somewhat, particularly to students and to scientists working in other fields who wish to pursue further details. One suspects that, in fact, there may have been some doubt in the author's mind as to exactly whom he was targeting as his readership. While the writing style is friendly enough, the frequent use of jargon seems to rule it out as a book for the general, non-scientific reader — despite the provision of a brief glossary and a useful set of background notes following the text.

The book is generally well-illustrated, and Zirker has made excellent use of a colour section by reserving it for spectacular astronomical images — once again highlighting the firm scientific foundation on which the book is based. Overall, it is a handsome and well-presented book, and it represents outstanding value in the UK at £20. Despite the minor reservations expressed here, there is no doubt that *An Acre of Glass* deserves a place on the shelves of all astronomical libraries. It provides a valuable snapshot of the current state of technology for optical astronomy, together with a knowledgeable appraisal of our most fervent aspirations for the future. As the author himself exhorts us at the end of his epilogue — "Stay tuned!" We will. — FRED WATSON.

The Very Best of the Feynman Lectures (Perseus Press, London), 2006. 6 audio CDs. Price £17·99/$29·95 (ISBN 0 465 09900 0).

Most things in particle physics come in threes, and the same is true of Richard Feynman, who exists in three distinct incarnations. The genius Feynman is the inventor of the diagram approach that made quantum field theory practical; the celebrity Feynman is the subject of books of safecracking anecdotes, and is a much healthier scientific stereotype than the mad scientist image built around Einstein's latter days. But most physicists probably first meet Feynman as a pedagogue, through his unique set of lectures on physics. These were on my first-year undergraduate supplementary reading list, and I always felt rather guilty that I didn't get much out of them. Even in later years, I found them tough going in general, although the occasional neat and unusual insight kept me coming back. My guilt subsided as I came to learn more about the lectures. Feynman intended to give an elementary introduction to physics for first-year students, but seems to have misjudged the level so totally that by the end only his senior colleagues were still surviving. Really, Feynman should have been British, since his lectures are an archetype of Glorious Failure. Over the years, they have acquired great mystique, added to by stories of his unusually engaging lecturing style. Most readers must wish they could have been there to see the show.

Well now you can. All the lectures were taped (sound only, alas) in order to help construct the books, and these CDs give you a selection of highlights. Turn on the stereo and you hear the hum of student voices waiting for the lecture to start — and what a moment in history! Starting your degree at Caltech in 1961, you had a fair chance of ending up as one of the astonishingly young team that would put men on the Moon less than a decade later. Physics was exciting, despite being tarnished by Hiroshima and Nagasaki. It was also new: the quantum revolution feels very much like ancient history today, but Feynman started lecturing when Dirac's equation was only just over three decades old.

With this level of expectation, the lectures themselves had to be a disappointment. First, the atmosphere is punctured by an announcer who seems to have taken lessons from the man that introduces "Hergé's adventures of TinTin!". Then Feynman speaks: I don't know why, but I had never imagined him with such a strong 'Noo Yoick' accent, and it came as a shock. Even after adjusting to this, there is the style of the lectures. No doubt standards have changed, but in any case the feel of the delivery is rather ponderous, formal, and quite long-winded. It sounds much more like a written script being read out, rather than something being improvised. Apparently Feynman used few notes, so it is quite an achievement to have produced such a stream of words with barely an "um" in sight. But from the point of view of the student consumer, I can't see it as having been all that engaging. There is an interesting contrast with the one 'Feynman lecture' that was delivered by Matthew Sands (I hadn't realised others had a part beyond editing the books). He seems to have a more natural style, and certainly gets the students to laugh at his jokes, whereas Feynman's often fall flat. Perhaps they were intimidated, since I'm sure the man had a fearsome reputation even then. So, all in all an interesting experience, but as much for what it points out about changes since 1961 as for what it reveals about Feynman. — John Peacock.

The Sky at Einstein's Feet, by W. Keel (Springer, Heidelberg), 2006. Pp. 299, 24 × 17 cm. Price £19·50/$34·95/€29·95 (paperback; ISBN 0 387 26130 3).

The aim of this interesting book is to describe at a popular level how relativity theory has influenced the development of astronomy. This is a big subject, which appears to get bigger all the time as new discoveries of relativistic effects regularly appear at scales from the cosmic acceleration to the double-pulsar binary. The author's background as an observer rather than a theorist is a positive advantage in that he is not tempted to use any equations.

Moreover he is in an excellent position to find suitably interesting and instructive illustrations of real data, which often illustrate what is happening far better than a technical discussion.

The style is lively and engaging. There are plenty of interesting anecdotes, some from the author's own experience, which give a real idea of both the rigours and the excitement of astronomical research. Other chance snippets of science emerge too, such as the fact that the colour of gold is a relativistic effect (Newtonian gold would look silver, and Newtonian mercury would not be a liquid). The book is copiously illustrated, with some stunning images, notably in the section on gravitational lensing. For the most part it is well produced, although the author had an unfortunate attack of dyslexia (at least four typos) on the page where he acknowledges help in proof reading. Among other possible readerships, the book would make excellent motivational background reading for students struggling with a first relativity course. — Andrew King.

Meteors and Meteorites: Origins and Observations, by M. Beech (Crowood, Marlborough), 2006. Pp. 157, 23·5 × 16·5 cm. Price £14·99 (paperback; ISBN 1 861 26825 4).

As soon as I unpacked this book I had a favourable impression of it because it had a faint and quite pleasant smell reminiscent of that of a freshly painted room. My favourable impression remained after I had opened it, and continued to grow as I read it from cover to cover. The author is Chairman of the Meteorites and Impacts Advisory Committee to the Canadian Space Agency and is therefore well suited to write on this topic. The book is aimed at the amateur observer, though the experienced professional who feels that a book for amateurs has no place on his shelves will be missing a gem.

There are a couple of bad things about the book, and I'm going to deal with these first in order to get them out of the way, and then I can dwell on the remainder, which is excellent. The first negative concerns the line drawings. These seem to have been executed by a draughtsman who has no idea of what he was supposed to be illustrating, and who has let the author down badly. For example, the caption to Figure 11 states that the troposphere extends to a height of about 15 km and the ionosphere begins at about 140 km. Yet the drawing itself indicates clearly and unambiguously that the troposphere ends at 85 km and the ionosphere starts at 115 km. Which are we to believe? Then again, Figure 13 purports to illustrate a family of curves relating the apparent magnitude to the initial speed for several initial masses. Unfortunately the initial masses for the individual curves are not labelled, thus changing what could have been an interesting and informative drawing into something that is quite useless. Can anyone ascertain, from Figure 66, where achondrites come from? I certainly can't. In Figure 80, there is a drawing of a little man, labelled "Observe"; and underneath the word "Observe" is a letter "r". What shoddy draughtsmanship! Figure 120 purports to show the geometry of a sinusoidal waveform, in which one normally plots a graph of displacement (vertical axis) *versus* distance (horizontal axis). In this figure, however, the vertical axis is labelled "amplitude" and the horizontal axis is labelled "speed", while one of the troughs is labelled, incomprehensibly, "Frequency (f) counter". One would scarcely suppose that it would be possible to draw so simple a device as a Yagi aerial incorrectly, yet there are so many mistakes in Figure 127 that I just cannot spend more time and space to describe them.

The second negative concerns the mathematical typesetting. I have the impression that the compositor not only has never typeset a mathematical formula before, but he has probably never seen one. It is not acceptable to print

$$\left(\frac{3m}{4\pi\delta}\right) \text{ as } \left(\begin{matrix}3m\\4\pi\delta\end{matrix}\right),$$

nor to print 10^{-8} as $10{-}^{8}$, nor is it acceptable to use e, h, g, and w for the Greek letters ε, η, γ, and ω. Doubtless the compositor felt that he was just using a different 'font', so what does it matter? And 95 MHz is 95×10^6 Hz, not 95×10^8 Hz — but, hey, what's the difference? The moral seems to be that, if you are writing a book, insist, before you sign any contract, on seeing and approving the final galley proofs. And, if you are writing a scientific book, choose a publisher that has some experience in the field.

Now, having got that out of the way, I can get on with what I really want to write, namely that, if you are an amateur meteor observer and you want to make some interesting and useful scientific or even just aesthetic observations of meteors, you really should try and get hold of this book, which is packed with all sorts of practical information of a kind that I have not seen elsewhere. There is a plentiful variety of projects involving meteors or meteorites that one can become involved in. Searching for and collecting meteorites; tracking fireballs; radiant determination; speed measurement; photography; spectroscopy; 24/7 all-sky monitoring; ZHR determination; sound recording; radar and radio observations. All are well within the scope of an advanced, or even a beginning, amateur, and good, practical, and well-written advice on all of these is to be found in this book. And for those who are just armchair astronomers who don't want to get seriously involved in systematic observing, the book is a good read anyway, with lots of little bits of information that you may not have known before. Did you know, for example, that there are *three* meteor showers associated with Comet Halley? We all know of the Orionids and the Eta Aquarids, but do you know where the third one is? The answer may surprise you. And here's a silly question: In which constellation is the radiant of the Lyrid meteor shower?

I found no obvious mistakes, though I would like sometime to debate with the author his explanations of why there are more meteors in the morning than in the evening, and why there are more meteorite falls in the evening than in the morning. The author's explanation of the former is that in the morning, when the apex culminates, meteors are faster, hence brighter, so we see more. I'd like to suggest that the reason is that aberration displaces all radiants towards the apex. His explanation of the latter is that, because of their slower speed, evening meteoroids are less drastically ablated in their travel through the atmosphere, so more of them survive to reach the ground. I'd like to suggest that the reason is that more people (including astronomers) are fast asleep in bed before dawn than in the evening shortly after sunset.

One interesting passage is a serious quantitative attempt to calculate the probability of being struck by a meteorite. (What a way to go!) I shan't spoil things by revealing the answer; suffice it to say that there is probably no need to wear a hard hat every time you leave the house. It does occur to me that asteroid researchers have managed to pull in a few research grants by persuading the powers that be that we are all going to be obliterated by an asteroid one day. Maybe meteorite researchers could likewise obtain some funding by not downplaying the chance of being struck by a meteorite.

One thing surprised me in the section on photographing meteors. There is a lot of advice on photographing them with conventional film, but no mention of digital cameras. And the author asserts that all sorts of photographic materials which he mentions (Tri-X, HP5, size 120, 4 × 5 sheets, canisters with 24 frames rather than 36) are "generally available" at photographic stores. Film photography (I thought) surely went out with the dinosaurs, but, before I committed myself to a devastating review, I checked with two of Canada's most successful meteor photographers. Both use film rather than digital cameras, and indeed use the very materials mentioned by Beech! I went to Victoria's main photographic store, and I asked whether these materials are still available. *All* of them were, and they could sell me *all* of them there and then over the counter. So much for my intention to write a devastating review! (I was warned, however, that Tri-X is no longer going to be easy to obtain.) I should add that both of the meteor photographers I contacted do own digital cameras and are experimenting with them to see if some of the drawbacks associated with digital imaging of meteors can be overcome.

The recording of sound from meteors is a much-neglected observational project, and this is suggested as something an enterprising observer might try. In addition to the expected delayed sound from a fireball in the lower atmosphere, there are numerous reports in the literature of simultaneous ('electrophonic') sound. Some scientists believe in the reality of simultaneous sound; others are sceptical. Its reality or otherwise, however, will not be determined by believing or by disbelieving, but by observation and recording. The author points out that the most important property of any equipment designed to record sound (or indeed to photograph fireballs) is that the equipment be designed to operate 24 hours a day and 365 days a year. Constant downtime caused by frequent fiddling, adjustment, and 'improvement' is a sure recipe for failure.

The author points us to numerous useful web sites where further specialized information can be obtained. I tried four of them at random, and all of them do indeed exist and do indeed supply the expected information.

One last thing, for the benefit of my Canadian colleagues: in case you are wondering if it is possible to obtain this British-published book in Canada, you might try enquiring at sales@vanwell.com, and have $34·95 plus shipping plus GST ready. It's certainly worth it. — Jeremy Tatum.

The Stargazer of Hardwicke: The Life and Work of Thomas William Webb, edited by Janet & Mark Robinson (Gracewing Publishing, Leominster), 2006. Pp. 259, 24 × 16·5 cm. Price £14·99 (hardbound; ISBN 0 852 44666 7).

It seems that the Victorian age, while one of rapid advance in all sorts of ways, was also more leisurely in some respects, at least where 'men of the cloth' were concerned, for many apparently had the time to make significant contributions to areas outside their professional domain. Thomas William Webb was one such, who, through his lecturing and writing in astronomy — and in particular his *Celestial Objects for Common Telescopes*, first published in 1859 — has encouraged generations extending to the present day to enjoy the delights of the night sky.

Janet & Mark Robinson were local historians in Herefordshire with no previous knowledge of Webb or, indeed, astronomy, but whose interest was sparked when they bought the old vicarage in Hardwicke, once occupied by Webb and his wife Henrietta. They have performed a truly sterling service in bringing together an excellent collection of essays on Webb and his times, which reveal a vivid picture of this gentle Church of England parson who, from his rural retreat in Herefordshire, carefully observed the skies and other natural phenomena and brought them to the attention of a wider public as manifestations of God's wonderful world.

Webb's personal life has been carefully researched by the Robinsons in the first five chapters before we turn to the 'invited reviews' of his scientific interests. And they were many: earthquakes (Roger Musson), telescope making (Robert Marriott), the Moon (William Sheehan), the planets (Richard Baum), comets (Jonathan Shanklin), the Sun (Lou Marsh), and double and variable stars (Robert Argyle); his work is all recorded carefully in notebooks, some of which are now in the care of the RAS (Peter Hingley). Allan Chapman shows how his efforts fitted well into the 'Grand Amateur' pattern beloved of Victorian clergy (at least those with adequate resources). But the activity that put Webb into a special category was his skill at communicating his deep passion for the sky to the common man, and this is nicely explored by Bernard Lightman.

The book is well annotated and will prove invaluable to historians, with full references and a bibliography of Webb's work. The Robinsons have also done an exemplary job in editing so that the chapters flow seamlessly and easily together, with minimal overlap and consistent style; I hunted for typographical errors without success! If I have a suggestion for a second edition, it is to be bolder and go for glossy paper for the figures, which will allow sharper, higher-resolution illustrations. I think Webb deserves it. — DAVID STICKLAND.

Russia's Cosmonauts: Inside the Yuri Gagarin Training Center, by R. D. Hall, D. J. Shayler & B. Vis (Springer, Heidelberg), 2005. Pp. 386, 24 × 17 cm. Price £18·95/$29·95/€24·95 (paperback; ISBN 0 387 21894 7).

Over the past 46 years, hundreds of hopeful young men and women have visited the Yuri Gagarin Training Centre near Moscow in anticipation of joining the *élite* cosmonaut cadre and participating in the exploration of the final frontier. Some have succeeded, but many have fallen by the wayside.

This comprehensive account of the once-secret centre is written by three of the leading western authorities on the Soviet–Russian human spaceflight programme. The first part of the book covers the history of the centre, the main training facilities (including a brief summary of overseas facilities used by cosmonauts), simulator programmes, and survival training.

This is followed by a section which describes the various military and civilian cosmonaut groups selected since 1960. The final third of the book deals with international training and joint programmes with NASA, ESA, and other agencies or countries, particularly *Shuttle–Mir* and the *International Space Station*.

One of the strengths of the book is the inclusion of numerous photographs of training-centre facilities, past and present, which help to bring the unique base to life for anyone who has never visited it. Also of value for spaceflight enthusiasts are the detailed appendices of the individuals and crews who have trained there and flown in space. My only complaint is an index that is limited to the names of cosmonauts and other individuals, which makes it difficult to find references to particular places or facilities.

This book is recommended for anyone who wants an in-depth look at the centre that trained the world's first human space explorers. — PETER BOND.

The Infinite Cosmos: Questions from the Frontiers of Cosmology, by J. Silk (Oxford University Press), 2006. Pp. 248, 24 × 16 cm. Price £18·99 (hardbound; ISBN 0 198 50510 8).

Joseph Silk, one of the world's foremost cosmologists and the Savilian Professor of Astronomy at Oxford University, has written this new book, *The Infinite Cosmos*, where he gives a very broad outline of cosmology and galaxy formation. It starts with a description of the physical principles behind understanding our Universe, such as simplicity, universality, and the Copernican principle, which states that our position in the cosmos is not unique. The book then goes into detail concerning galaxy and structure formation, from our own Galaxy to the earliest epoch where galaxies are found. Silk highlights important results about how galaxies form through galaxy–galaxy mergers, and how the *Hubble Space Telescope* has advanced our understanding of this process through very deep imaging. The book also includes descriptions of the various possible forms of dark matter, including WIMPS and neutrinos, and how we can determine from the structure of galaxies and galaxy clusters that the dark matter is likely cold. Whatever the dark-matter particle is, it cannot be moving very quickly, which rules out neutrinos as a dominant form of dark matter. Black holes are now thought to be at the centres of nearly all massive galaxies. Silk describes how we are able to detect these supermassive black holes, and what their rôle might be in driving galaxy formation. The book contains an excellent description of how galaxies are clustered together, as well as how to detect gravitational waves and 'dark energy'. The discussion of the cosmic microwave background, and how we can use it to learn about the Big Bang and the early Universe, is particularly well described, and in my opinion is the best part of the book.

Silk throughout weaves his descriptions with a mixture of theory and observations, making both accessible to readers. The final few chapters of the book are of a more speculative nature, and include topics such as the existence of multi-universes, the meaning of infinity, time machines, and how cosmology might say something about God. One problem with the book is that it would be difficult to understand for someone new to astronomy, with some terms not fully explained. Those already familiar with basic cosmological and astronomical ideas are the ones who will likely get the most out of it. The book also repeats itself many times, and could have benefitted from better editing. There are also a number of interesting features of galaxies and cosmology that could be better explained through having more colour figures in the book, which includes only about a dozen black-and-white drawings. — CHRISTOPHER J. CONSELICE.

Handbook of CCD Astronomy, 2nd Edition, by S. B. Howell (Cambridge University Press), 2006. Pp. 208, 23 × 15 cm. Price £55/$95 (hardbound; ISBN 0 521 85215 3), £24·99/$39·99 (paperback; ISBN 0 521 61762 6).

Charge coupled devices (CCDs) are now the most commonly used detector in optical astronomy. There is a large commercial and technical market for CCDs outside astronomy, which is driving technical improvement and helping to keep prices down. The first edition of this book appeared in 2000 and quickly became widely accepted as a standard reference but, inexplicably, was never reviewed in *The Observatory*. There have been so many advances since 2000 that a second edition is fully justified. In fact, developments are appearing so frequently that it will not be long before another is needed.

This work is a handbook which guides the reader through the bewildering array of devices available and describes how to set them up and exploit them. There is a wide choice of disposable parameters when setting up a CCD for observing and their optimal values depend on the objective of the observer, *e.g.*, accurate photometry of bright stars, detection of faint objects, or spectroscopy of extended sources. Once an image has been secured it must be cleared of instrumental signature, *i.e.*, corrected for bias and flat field. Bias is no longer a major problem but flat-fielding remains difficult to accomplish with any degree of rigour. The dawn sky does not have the same spectral distribution as a star and it is difficult to arrange an experiment involving the dome interior which exactly mimics the angular distribution of light that a star provides. A related persistent problem with thinned chips is the fringing on images caused by interference of night-sky emission lines.

It is important to use a plate scale which avoids under-sampling. With under-sampling the image structure varies dependent on whether the centre of the star image is close to the centre of a pixel or near the edge. This can affect the photometric total of light measured for a star and, more importantly, the centre position of the star used in astrometry. Most reduction packages, *e.g.*, *Starlink* software, assume that each pixel can be treated as a uniform rectangle. However, the gate structure of each pixel results in non-uniformity of response within it. Some electrons may be lost or even migrate into adjacent pixels. Intra-pixel non-uniformity can be important in low-dispersion spectroscopy and high-accuracy astrometry. The only solution is to over-sample the chip and, in the latter case, bad seeing is a positive benefit.

This book is up to date in most respects with very few errors. However, on p. 59 we are told of a "CCD in current operation at the Royal Greenwich Observatory". Following a short-sighted decision, the Royal Greenwich Observatory was closed in 1998 and the Loral chips were taken out of service before that.

It would be nugatory to recollect just how bad CCD chips were in the past. The future lies in chips with faster, less-noisy readout and greater size and uniformity. These large chips are routinely joined into mosaics and one worries that the acreage of silicon may soon expand beyond that of the primary mirrors.

This book is number 5 of the *Cambridge Observing Handbooks for Research Astronomers* but it can also be commended to the increasing number of amateur astronomers who use CCD cameras. It will also prove valuable as a textbook, with exercises at the end of every chapter. — DEREK JONES.

Comets II, edited by M. C. Festou, H. U. Keller & H. A. Weaver (University of Arizona Press, Tucson), 2005. Pp. 780, $28 \cdot 5 \times 22 \cdot 5$ cm. Price \$85 (about £48) (hardbound; ISBN 0 816 52450 5).

Comets are often dismissed as being merely 'minor bodies' in the Solar System. But as time passes, their rôle as the fundamental, pristine, building blocks of the gas-giant planets, and fascinating objects in their own right, and as a major repository of our system's water, becomes recognized. Their study also exemplifies modern space-age astronomy. Much has been learnt about their complicated orbital dynamics by the use of large, fast computers. Cometary studies from Earth have also been enlivened by recent 'great' comets, like Halley, Hale-Bopp, and Hyukatake, convincing people in charge of large telescopes that it is worthwhile spending a few hours investigating their ever-changing activity. And comets have provided challenging targets for spacecraft exploration. We have just passed the twentieth anniversary of the first cometary fly-by, and the first detailed imaging of a cometary nucleus. Since then spacecraft have improved resolution, made *in-situ* dust collections, and even 'bashed' into one.

When it comes to 'all we know about comets', we have progressed from the 221 pages of N. B. Richter's slim monograph *The Nature of Comets* (Methuen, 1963) to the heavyweight 766 and 745 pages of the Arizona Space Science Series' *Comets* (1982) and now *Comets II* (2005). The latter tome laudably concentrates on posing three questions. (*i*) What do we know about comets? (*ii*) How have we obtained this knowledge? (*iii*) What are the next steps? The answers are provided by a well-chosen international team of experts. Thirty-seven review chapters concentrate on topics such as: the origin of comets and the relationship between comets and both the solar nebula and the interstellar medium; cometary orbits and the Oort Cloud and the Edgeworth-Kuiper Belt; the dirty-snowball nucleus — its size, shape, colour, albedo, rotation, decay, splitting, structure, physical and chemical composition, and evolution; the gaseous coma — its photometry, chemistry, ionization, dispersion, plasma-tail production, and interactions with the solar wind; comet dust — its mineralogy, thermal emission, and light-scattering properties; and finally the relationship between comets and Centaurs, Trans-Neptunian Objects, meteoroid streams, and the zodiacal dust cloud.

The watchwords are clarity and thoroughness. Today's research students are very fortunate to have such an excellent book to introduce them to the joys and challenges of the subject. *Comets II* abounds with optimism. Soon we will be flooded with results as the European Space Agency's *Rosetta* spacecraft orbits comet 67P/Churyumov-Gerasimenko on its eventful journey from beyond the asteroid belt to perihelion. Soon we will be landing softly on the fragile surface of a nucleus and watching the decay process from close quarters. Soon such characteristics as cometary mass, density, and interior structure will be less of a complete mystery. These are exciting cometary times. — DAVID W. HUGHES.

Pulsar Astronomy, 3rd Edition, by A. Lyne & F. Graham-Smith (Cambridge University Press), 2006. Pp. 309, 25·5 × 18 cm. Price £85 (hardbound; ISBN 0 521 83954 8).

With the publication of the 2nd edition of this classic introduction to pulsars in 1998, one's first reaction to the appearance of another edition is to ask whether it was justified to bring it out so soon. My answer is definitely yes! Such is the vigour of research in this area of astronomy that the number of known pulsars is now around 1500, more than double the total of seven years ago. While there have been no penetrating new insights — for example, we still do not know why millisecond pulsars have magnetic fields four orders of magnitude smaller than typical pulsars — observations have provided a wealth of information and indicated some intriguing fresh avenues of research. Roughly each decade has produced some unexpected excitement and this time it was the discovery in 2003 of the first double-pulsar binary PSR J0737-3039 A&B. Having an orbital period of 2·4 hours, and a maximum separation of merely 700 000 km, this will provide yet more stringent tests of General Relativity than was possible using the famous single-pulsar binary discovered by Hulse & Taylor in 1975. Already the observed Shapiro delay has confirmed Einstein to an accuracy of 0·2% and the precession of the orbit is 17 degrees per year, compared to 43 arcseconds per century for the planet Mercury! Another crucial factor is that the binary orbit is viewed almost directly edge-on, so that radiation from one pulsar traverses the atmosphere of the other. Thus it is now possible directly to probe the structure of a pulsar magnetosphere for the first time. But this is for the future and the double pulsar is only mentioned briefly here.

Overall, the 3rd edition of *Pulsar Astronomy* is a considerable improvement. Much of the material and chapter headings are similar, but the text is about 30% longer and there has been much revision and reorganization. There are more than thirty new illustrations, amongst which is my favourite, the famous X-ray image of the Crab pulsar obtained by the *Chandra* telescope. The wind nebula surrounding the pulsar looks strikingly similar to a giant catherine wheel, and there is another feature aligned with the rotation axis. Some new topics discussed include evidence for free precession of neutron stars, the confirmation of geodetic precession of the rotation axis of the original binary pulsar, which may shift the radio beam away from our line of sight causing the pulsar to disappear in about 20 years, and the puzzling magnetars. These are X-ray pulsars which rely on something other than spin-down energy to drive the radiation. As before there is a catalogue of pulsars, now extended to around 1400 sources: this gives only the periodicities, as other details such as distance, dynamical age, *etc.*, are now readily available on the web. The bibliography has been roughly doubled to about 700 references. For anyone starting research, or preparing a graduate lecture course, this comprehensive, authoritative, and readable introduction to pulsars, with some interesting historical asides, is strongly recommended. — ANTONY HEWISH.

Star Formation in the Interstellar Medium: In Honor of David Hollenbach, Chris McKee and Frank Shu (ASP Conference Series, Vol. 323), edited by D. Johnstone *et al.* (Astronomical Society of the Pacific, San Francisco), 2004. Pp. 417, 23·5 × 16 cm. Price $77 (about £42) + $20 airmail shipping (hardbound; ISBN 1 583 81185 0).

This volume represents the proceedings of a conference held in 2003 to celebrate the respective 60th birthdays of Hollenbach, McKee, and Shu. If this seems like just an excuse for another conference on star formation then at least it can be said that the resulting volume (and presumably therefore the conference itself) is a cut above the average. The book begins with reviews by each of the three main protagonists. Hollenbach chooses the disruption of discs as his topic, concentrating on photo-evaporation. McKee selects massive-star formation, both for primordial stars and present-day star formation. Shu plumps for the stellar initial mass function (IMF), showing how an analytical interpretation of his X-wind model can reproduce the observed IMF. Much of the material has appeared elsewhere, but the insights offered by three such leading lights serve to make their chapters extremely readable. The themes touched upon in the opening chapters are then expanded in numerous other contributions from many of the field leaders.

My attention was caught by the recurrent theme of the rôle of magnetic fields and turbulence in the star-formation process. The debate over which of these two is the dominant mechanism of star formation has been rumbling on for a number of years, and this book reviews the various aspects of this debate in different chapters. Shu reminds us of the result that the magnetic field appears roughly constant — at least in the envelopes of molecular clouds — and shows how this is a direct consequence of magnetic criticality. Zweibel returns to this theme and extends the relation to higher-density gas in molecular-cloud cores, showing how the observation that the magnetic field only increases as the square root of the density leads to the necessity for magnetic diffusion. Galli looks at magnetized self-gravitating equilibria of molecular clouds and discusses whether symmetry is necessary for equilibrium. Putting these contributions together with the so-called 'Larson Laws' one arrives at a relation between magnetic criticality, cloud density, and ambipolar diffusion that this reviewer had not seen spelt out in exactly this way before.

Elmegreen shows how the power spectra of whole galaxies resemble that produced by Kolmogorov turbulence. Pudritz discusses turbulent gravitational collapse and relates the ratio of the turbulent-damping time over the free-fall time to the formation of clusters *versus* isolated stars. Klein concentrates on turbulence as a mechanism for forming binary and multiple star systems. Neither of these latter authors includes magnetic fields in their simulations. However, the reader is left to judge which of the various mechanisms relating to magnetic and turbulent effects produces the most physically plausible results.

This is only a taste of what the book contains. There is much else besides, on the ISM, star formation, circumstellar discs, and the formation of planets, amongst other topics. I definitely recommend perusal of this fascinating tome. — DEREK WARD-THOMPSON.

Patrick Moore on the Moon, by P. Moore (Cassell, London), 2006. Pp. 239, 23·5 × 16 cm. Price £14·99/$17·95 (paperback; ISBN 1 844 03536 0).

The original version of Patrick Moore's *Guide to the Moon*, published in 1953, was the first astronomy book I owned. I still have it, wrapped in plastic to protect the dust cover with its imaginative depiction of the lunar surface. Back then, the combination of Patrick Moore and the Moon was irresistible. The Moon was still the province of the amateur, and Patrick was the natural guide, a man who knew its surface better than almost anyone. Gazing through his telescopes, he had hovered like a phantom astronaut over its rocky landscape night after chilly night.

How quickly things were to change. By the time of the second edition of *Guide to the Moon* in 1976, orbiting space probes had photographed the Moon in detail front and back, robotic landers had touched down on it, and astronauts had plucked rocks from its dusty surface. In many ways, the romance had been dispelled. Once tantalizingly beyond reach but now conquered and discarded, the Moon languished while the spotlight turned to the canyons and volcanoes of Mars and the clouds, rings, and moons of the outer planets. Yet the Moon remains an ideal starting place not just for amateur observers but also for planetary scientists attempting to understand the Solar System. With several books now being produced by a new generation of lunar observers, and a new series of lunar probes addressing unanswered questions, interest in the Moon is undergoing something of a revival.

Time, then, for a third edition of *Guide to the Moon*, albeit with a change of title. *Patrick Moore on the Moon* was first published in 2001; this is the paperback reissue. Despite the new title, the content remains substantially the same. For example, the main advance in lunar research since the last edition of *Guide to the Moon* has been the development of the giant-impact model of lunar origin, but this idea rates a mere paragraph. Lunar meteorites, another hot topic, receive even more superficial coverage. A brief new chapter concerns the work of the probes *Clementine* and *Lunar Prospector* in looking for lunar ice, a subject on which Moore is an avowed sceptic. The books ends, as ever, with Patrick's own sketch map of the Moon and an extensive description the main features.

Unfortunately, the book is let down by its illustrations. The black-and-white line diagrams in the text appear to have been reproduced directly from the printed pages of the last edition, while the colour plates are for the most part sub-standard — one is so badly blurred that it is impossible to read the labelling. Given the number of excellent scans that are freely available on-line there is no excuse. To make matters worse, several of the captions are on the wrong pages. Those caveats aside, this remains a classic introduction to the Moon for amateur astronomers. — Ian Ridpath.

It's About Time: Understanding Einstein's Relativity, by N. D. Mermin (Princeton University Press, Oxford), 2005. Pp. 186, 23 × 15·5 cm. Price £22·95/$35 (hardbound; ISBN 0 691 12201 6).

The author of this book is a leading theoretical physicist and this book grew out of his courses to non-science undergraduate majors for more than thirty-five years.

It is an excellent book on Einstein's special theory of relativity, primarily for people with almost no education in mathematics and physics beyond algebra and elementary geometry. I clearly see the strength of this book in lucid, self-contained, lively, down-to-earth, and meticulous presentation. Though this book is written at a very elementary level for non-science majors, I very strongly believe that undergraduate physics majors, graduate students, and researchers in relativity will find a few things very clearly explained that they had not seen elsewhere. This book has also a concise and lucid introduction to Einstein's general theory of relativity.

Anyone who reads this book carefully will definitely acquire a very good understanding of the subject: I have no hesitation in saying that this is the best book on the special theory of relativity at a semi-popular level I have ever read. It would be very suitable for all libraries, including public ones. — K. S. Virbhadra.

THESIS ABSTRACT

Eclipsing Binary Stars in Open Clusters

By John K. Taylor

The study of detached eclipsing binaries allows accurate absolute masses, radii, and luminosities to be measured for two stars of the same chemical composition, distance, and age. These data can be used to test theoretical stellar models, investigate the properties of peculiar stars, and calculate the distance to the binary using empirical methods. Detached eclipsing binaries in open clusters allow a more careful test of theoretical models, which must simultaneously match the properties of the eclipsing system and the morphology of the cluster colour–magnitude diagram. In addition, an accurate distance and a precise age and metal abundance may be found for the cluster from the properties of the eclipsing system, avoiding the difficulties inherent in obtaining these properties from matching isochrones to the cluster stars in a colour–magnitude diagram.

Absolute dimensions have been found for V615 Per and V618 Per, which are eclipsing members of the young open cluster h Persei (NGC 869). The fractional metal abundance of the cluster has been found to be $Z \approx 0{\cdot}01$ by comparing the properties of V615 Per and V618 Per to the predictions of theoretical stellar evolutionary models, in disagreement with assumptions of a solar chemical composition in previous works.

Accurate absolute dimensions (masses to 1·4%, radii to 1·1%, and effective temperatures to within 800 K) have been measured for V453 Cygni, a member of the young open cluster NGC 6871. The current generation of theoretical stellar models can match these properties for an age of 10·0 ± 0·2 Myr and a solar chemical composition. The models also successfully predict the central concentration of mass of the primary star derived from a study of the apsidal motion of the system. A Monte Carlo simulation technique has been implemented to determine robust uncertainties in the results of the photometric analysis of detached eclipsing binaries.

The B-type subgiant eclipsing system V621 Per, a member of the open cluster χ Persei (NGC 884), has been studied. The absolute dimensions of the system have not been measured as the secondary star is not detectable in our spectroscopic observations, but have been inferred from a comparison with theoretical models. The secondary star should be detectable in very-high-quality spectra, in which case further study of this system will be very rewarding.

Absolute dimensions have been determined for HD 23642, a member of the Pleiades. A new method of measuring the distance to detached eclipsing binaries has been introduced, based on calibrations between surface brightness and effective temperature. This method gives a distance of 139 ± 4 pc to the Pleiades, in good agreement with several recent distance measurements but not with the controversial distance of 120 ± 3 pc found using parallax measurements from the *Hipparcos* satellite. Both the new distance-determination method and well-established techniques using bolometric corrections perform better at near-infrared wavelengths, where surface brightness depends less strongly on effective temperature and the effects of interstellar extinction are smaller than in the optical.

The metallic-lined eclipsing binary WW Aur has been studied using extensive new spectroscopy and published light curves. The masses and radii have been found to accuracies of 0·4% and 0·6%, respectively, using entirely empirical methods. The effective temperatures of both stars have been found by using a method which is almost fundamental. The predictions of theoretical models can only match the properties of WW Aur by adopting a high metal abundance of $Z = 0{\cdot}060 \pm 0{\cdot}005$. — *Keele University; accepted 2006 March.*

John Taylor publishes under the name of John Southworth. The full thesis is available electronically at http://www.astro.keele.ac.uk/ ~ jkt/pubs.html#thesis, along with several other resources useful in the study of eclipsing binary stars.

OBITUARY

Wulff-Dieter Heintz (*1930–2006*)

Wulff-Dieter Heintz, Professor Emeritus of Astronomy at Swarthmore College, passed away at his home on 2006 June 10, following a two-year battle with lung cancer. He had just turned 76 a week earlier. He was one of the leading authorities on visual double stars, and was also a chess master. A prominent educator, researcher, and scholar, Wulff was noted for being both succinct and meticulous in everything he did.

Wulff Heintz was born on 1930 June 3 in Würzburg (Bavaria), Germany. Naturally left-handed, his elementary school teachers forced him to learn to write 'correctly' using his right hand, and so he became ambidextrous. During the 1930s, Wulff's family saw the rise of Adolf Hitler and lived under the repressive Nazi régime. Conditions were austere, and it was often difficult to find fuel to keep the house warm. As a teenager during World War II, he listened to his family radio for any news from the outside world. He used to say that he loved the blackouts during the bombing runs because it made it much easier to see the stars. One night, an incendiary bomb landed on the roof of his family home, and Wulff climbed up to the roof and extinguished it. The next morning, he saw that his high school had been completely levelled by allied bombs. As Germany continued to suffer massive losses on the Russian Front, primarily due to unexpectedly severe winters, teenage boys were inducted into the military and sent off to replenish the troops. To avoid an uncertain fate, Wulff hid out in a farmhouse in the countryside outside Munich. When the allied troops invaded Germany in 1945, Wulff volunteered to translate information from the American and British soldiers to the local villagers. In return for his valuable service, the soldiers taught him how to smoke cigarettes, a habit which he continued until his final days, even after having been diagnosed with lung cancer.

Shortly after the war ended, Wulff enrolled at Würzburg University, eventually completing his studies in 1950 with two majors, mathematics and chemistry. In 1950 he enrolled for graduate studies at Munich University. There, along with fellow classmates and future colleagues Edward Geyer and Theodor Schmidt-Kaler, Wulff received a thorough instruction in astronomy from, among others, Wilhelm Rabe (binary stars) and Felix Schmeidler (astrophysics and galactic astronomy). He also gained practical training in the use of meridian circles and position micrometers, and learned to make binary-star observations with the old (1835) *Fraunhofer* refractor of the Munich Observatory. It was there that his passion for binary stars was born.

In 1953, Munich University awarded Wulff the degree of Dr. Rer. Nat. in Astronomy, which he completed under the direction of Felix Schmeidler. He was almost immediately recruited by the Munich University Observatory to serve as the Scientific Assistant at the Southern Station on Mount Stromlo, Australia. He worked at Mount Stromlo from 1954 to 1955, then returned to Munich to serve as Research Officer from 1956–69, during which time he visited both the United Kingdom and the United States. Wulff was involved in observations of the planet Mars, and in particular the dust storms which were occurring on that planet around the time of the 1956 opposition. His sketches of the Red Planet were quite detailed, and showed then-unknown surface features which spacecraft visiting the planet years later revealed to be large volcanoes.

In 1960, Wulff published an early but substantial paper, *Die Doppelsterne im FK4*, which was very important in the construction of the FK4 and was still used in 1988 for the FK5. Subsequently, in 1961, he was invited to attend the IAU Symposium on Visual Double Stars at the University of California, Berkeley. The experience was inspirational, and solidified Wulff's devotion to double-star research. By the end of the decade, in 1969, he published the results of an extensive statistical study of binary stars in a classic paper, which became a much referenced contribution to the field.

On 1957 June 14, Wulff married Dietlind (Linde) Laschek, and the couple spent their honeymoon at the Royal Greenwich Observatory at Herstmonceux Castle in England. The marriage produced two children, a daughter Ruth, born in 1965, and a son Robert, in 1967. Wulff earned a Privatdozent (advanced postdoctoral degree) at the Technological University Munich in 1967. Shortly thereafter, he accepted an invitation from Professor Peter van de Kamp to come to the United States as a visiting astronomer at Swarthmore College, located outside Philadelphia. Wulff joined the Department of Astronomy permanently as an Associate Professor in 1969, and moved his family from Germany to the United States the following year. He became Chairman of the Department in 1972 and served in that capacity until 1982. Wulff was promoted to the rank of professor in 1973, and was a full-time faculty member at Swarthmore until his retirement in 1998. He continued to teach introductory astronomy courses as an adjunct professor at nearby Widener University until 2005.

Over his long and distinguished career, Wulff Heintz pursued numerous research interests, including fundamental astrometry, stellar statistics, planetary studies, radial velocities, and, in his last years, monitoring slow variable stars using a CCD detector. Together with the committed staff of the Sproul Observatory, Wulff determined about 800 precise trigonometric parallaxes of mostly faint, high-proper-motion stars. The lion's share of his attention over the period 1954–97 was devoted to double and multiple stars, orbit theory, and relative astrometry. An assiduous observer, Wulff logged many hours at the 24-inch Sproul refractor, striving to equal or better the record for total number of observations by a single observer set by William Herschel at the beginning of the 19th Century. Over several decades, he made a total of 54 000 micrometer measurements of double stars (47 500 by eye and 6 500 photographically) and discovered over 900 new pairs. Some of his resolutions of new binaries have only been confirmed with speckle interferometry or by the *Hipparcos* satellite. In fact, in the latter case, several of the 'new' binaries resolved by *Hipparcos* had actually been previously resolved by Wulff years earlier.

As a dynamicist, Wulff had unquestioned skill in the calculation and analysis of binary-star orbits. He employed fully both micrometry and photography, and also incorporated published spectroscopic data to calculate orbits for some 500 binary systems. He tackled some of the most complex systems which can be unravelled — astrometric systems where the secondary or tertiary is hidden and can only be disentangled by careful analysis of available observations. His prolific calculation of binary-star orbits earned him the title of the 'Swarthmore Orbit Machine' among some of his colleagues. Historically, only W. H. van den Bos made more observations of pairs than Wulff. Before the advent of interferometry, the highest-quality observations of the closest pairs were made by Wulff and his collaborator Charles Worley at the USNO. The closest pairs were not only the most difficult to split but astrophysically the most important, as from these faster-moving systems one could calculate orbits and in some cases determine masses. Wulff and Charles collaborated in the *Fourth Catalogue of Orbits of Visual Binary Stars* (US Naval Observatory, 1983), the last paper version of this catalogue, and which was a standard reference for many years. Even the more recent versions of the catalogue list more orbits by Wulff than by any other calculator.

Wulff was the author of some 150 research papers (including several in these pages), plus several articles in the popular literature and encyclopedia articles. He was the author, co-author, or editor of nine books. His early monograph *Doppelsterne* (Goldmann, 1971) was recrafted and translated into English to become *Double Stars* (D. Reidel, 1978). This was the standard binary-star text for many years, and continues to serve as the definitive text on the subject. Those familiar with Wulff's style of writing will know why it was referred to as the 'Terse Tome', but it contained all the relevant information. Wulff and I collaborated to translate the German *Handbuch für Sternfreunde* into the English *Compendium of Practical Astronomy* (Springer-Verlag, 1994). In addition to his professional pursuits, Wulff was an acknowledged chess master, and he authored *Praktische Schachbuch* (Practical Chess Book), which had 13 printings in the period 1968–81. He was also an adept pianist, and was especially fond of playing Chopin, Liszt, and Rachmaninov.

Wulff enjoyed teaching immensely, and taught courses at all levels, including introductory astronomy for both science students and for the general student population, meteorology, positional astronomy, cosmology, galactic astronomy, and the history of astronomy. He also served as a Shapley Lecturer for the American Astronomical Society, in which he visited and gave talks at colleges and universities which lacked formal astronomy programmes. One of Wulff's favourite activities was running the public viewing sessions at Sproul Observatory, in which he used the large 24-inch refractor to observe the Moon, planets, double stars, nebulae, and star clusters. Wulff also took time to run special telescope sessions for cub scouts, brownie troops, church groups, and amateur astronomical societies.

Over the span of half a century, Wulff Heintz made valuable contributions to the astronomical database. A truly international scholar, Wulff was a Fellow of the RAS and had been a member of the Astronomisches Gesellschaft and the AAS. He was active in the IAU, serving as President of Commission 5 (Documentation and Astronomical Data) over the interval 1979–85. He was also a representative and Executive Committee member in ICSTI (International Council on Scientific and Technical Information) during that same period of time. His rôle as an educator was no less significant, and he no doubt inspired numerous young individuals to pursue astronomy as a career, or at the very least, as a hobby. After having lived a career which was so rich and productive, Wulff will be much missed by the astronomical community, and especially those working in the areas of astrometry and binary stars. — HARRY J. AUGENSEN.

Here and There

HARDLY SURPRISING (ALSO AN ERRATUM!)

... the *V33* filter, which has almost identical characteristics to *V33*, ... — *The Observatory*, **126**, 167, 2006. (This should read, "the *V33* filter, which has almost identical characteristics to *V30*". Apologies for this editorial lapse.)

Advice to Contributors

The Observatory magazine is an independent journal, owned and managed by its Editors (although the views expressed in published contributions are not necessarily shared by them). The Editors are therefore free to accept, at their discretion, original material of general interest to astronomers which might be difficult to accommodate within the more restricted remit of most other journals. Published contributions usually take one of the following forms: summaries of meetings; papers and short contributions (often printed as *Notes from Observatories*); correspondence; reviews; or thesis abstracts.

All papers and *Notes* are subject to peer review by the normal refereeing process. Other material may be reviewed solely by the Editors, in order to expedite processing. The nominal publication date is the first day of the month shown on the cover of a given issue, which will normally contain material accepted no later than four months before that date. There are no page charges. Authors of papers, *Notes*, correspondence, and meeting summaries are provided with 25 free reprints if required; additional reprints may be purchased.

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(1) G. H. Darwin, *The Observatory*, **1**, 13, 1877.
(2) D. Mihalas, *Stellar Atmospheres (2nd Edn.)* (Freeman, San Francisco), 1978.
(3) R. Kudritzki *et al.*, in C. Leitherer *et al.* (eds.), *Massive Stars in Starbursts* (Cambridge University Press), 1991, p. 59.

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