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A REVIEW OF ASTRONOMY

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2007 March 9th at $16^h 00^m$
in the Geological Society Lecture Theatre, Burlington House

M. Rowan-Robinson, *President*
in the Chair

The President. First I'd like to welcome Professor Oddbjørn Engvold from the Institute of Theoretical Astrophysics at the University of Oslo to his first meeting of the RAS since becoming an Associate in 2005 [applause].

Moving to the programme, our first speaker is Professor Clive Ruggles from the University of Leicester, and he's going to speak about 'Ancient solar observatories: new evidence from the Stonehenge area and from Peru.'

Professor C. L. N. Ruggles. What do we mean by an ancient observatory? Cheomseongdae in Korea (7th Century AD) is often quoted as East Asia's most ancient observatory, and it certainly existed within a historically attested (Buddhist) tradition of sky observation, but to our knowledge it (merely) functioned as a 'star gazing tower', as the name literally means. The Caracol at Chichen Itza in Mexico (*c.* 9th Century AD), one of very few round Mayan buildings, is widely referred to as an observatory. Yet while the complexity of Mayan sky observations is well known through documentary evidence, there is no historical evidence making a direct link between these buildings and celestial observations, and the alignment evidence is highly equivocal. Newgrange in Ireland (*c.* 3250 BC) is sometimes quoted as one of Europe's most ancient observatories, but it was in fact a tomb: living people surely did not sit among the bones of the dead to see whether the shortest day of the year had arrived. It seems clear that the solstitial orientation of this tomb reflects an association in people's minds between ancestors and the Sun at the darkest time of the year, quite possibly expressing a perceived connection between death and seasonal renewal. Goseck in Germany, dating to the early 5th Millennium BC and popularly claimed to be Europe's most ancient observatory, was in fact an earthen enclosure with three entrances, two of which were roughly aligned, respectively, upon the rising and setting positions of the Sun at the winter solstice. Even if the two solstitial alignments were in fact intentional, which is far from certain, their significance probably related to seasonal festivities that took place within the enclosure around the shortest day of the year.

Stonehenge, at least, was an ancient observatory ...

Or was it? The idea rests very largely upon the identification in the 1960s of numerous pairs of stones and related features that align, within a few degrees, upon particular rising or setting positions of the Sun or Moon that were assumed to have been of significance. The idea that most of these alignments could have been intentional was, within a few years of their publication, shown to be untenable both on archaeological and statistical grounds. A similar dual critique applies to the idea that the 56 Aubrey Holes were used as a device for predicting eclipses. On the other hand, this doesn't mean that deliberate astronomical alignments did not exist at Stonehenge. Indeed, the solstitial orientation of the sarsen monument (constructed around 2500 BC) is arguably the most famous manifestation of 'ancient astronomy' in the entire world. The practice of solstitial alignment is also repeated at a number of contemporary monuments in the vicinity. In the last two years I have been working in association with a major excavation and survey project (The Stonehenge Riverside Project) to assess the archaeological and archaeoastronomical evidence relating to these alignments in a consistent manner.

The intended directionality of the Stonehenge axis is not immediately evident. The common assumption that the axis is directed north-eastwards towards midsummer sunrise is supported by the existence of a 'corridor' formed by two pairs of stones — the Heelstone and a companion plus the Slaughter Stone and a companion — through which the light of the rising midsummer Sun would have shone spectacularly into the centre of the sarsen monument. On the other hand, the formal (or ceremonial) approach to Stonehenge along the avenue was from the north-east, suggesting that the focus of interest was straight ahead, *i.e.*, towards the south-west and midwinter sunset. Supporting this interpretation is evidence from pig bones found at the nearby henge enclosure of Durrington Walls, which shows that slaughtering and feasting took place as part of winter, rather than summer, festivities.

Several circles of earth and timber are known to have been built within 3 km of Stonehenge in the second half of the 3rd Millennium BC. The closest earthen enclosure (henge), Coneybury, has been all but obliterated by ploughing but was roughly aligned NE–SW. Woodhenge contained six concentric oval rings of timber posts whose major axes were orientated NE–SW and aligned, like Stonehenge, to within 1° of both midsummer sunrise and midwinter sunset. Durrington Walls is a huge earthwork enclosure more than 300 m in diameter, which is unusual, and for our purposes highly valuable, in having been built on a significant slope: this prevents what might have been an intentional solstitial alignment in only one direction giving rise, as a passive and possibly unintended consequence, to a solstitial alignment in the opposite direction. Within Durrington Walls was a second large multiple ring of timber posts known as the South Circle. Part of this together with the main entrance of Durrington Walls (facing SE) were excavated in 2006. The South Circle also had an entrance to the SE, aligned very closely (within 0·5°) upon midwinter sunrise. A short avenue was discovered leading from the large henge to the river: this was aligned north-westwards upon midsummer sunset. In sum:

	Inward direction	*Outward direction*
Stonehenge	Midwinter sunset	Midsummer sunrise
Woodhenge	Midwinter sunset	Midsummer sunrise
Durrington Walls henge	Midsummer sunset	
Durrington South Circle		Midwinter sunrise

Whatever the intended directions were at Stonehenge and Woodhenge, it is clear that neither an exclusive interest in one or other solstice, or in sunrise or sunset, or even in the inward or outward direction, explains all the evidence. One suggestion is that Stonehenge, built in stone, represented the domain of the dead while Durrington Walls, built in timber, was within the domain of the living. The avenues (joined at their far ends by the River Avon) represented a formal or symbolic route connecting those two domains, providing the means for spirits (and perhaps ceremonial processions) to pass from Durrington to Stonehenge at midwinter, and in the reverse direction at midsummer, perhaps because ancestors were seen to provide fertility.

Despite the obvious importance of the solstices, the evidence from Stonehenge and from contemporary earthen and timber monuments in the vicinity provides no indication whatsoever that systematic observations were made of the rising or setting Sun over the seasonal year. Stonehenge, in other words, was not a solar calendar.

The Thirteen Towers of Chankillo, in contrast, provide clear evidence of systematic observations of the sunrise and sunset over the entire seasonal year. This remarkable structure, whose astronomical significance was only confirmed following a field survey undertaken in 2005, runs north to south along a low ridge within a 4th-Century-BC ceremonial complex (containing multiple structures, plazas, and courtyards) in north coastal Peru. The towers, which are well enough preserved to permit an accurate reconstruction, were flat-topped and cuboidal in shape. They varied in height from 2 m to 6 m and in horizontal surface area from *c.* 75 m^2 to 125 m^2. Viewed from the buildings and plazas to the east and west, the towers formed an artificial 'toothed' horizon with narrow gaps at regular intervals. To the west is a building with a curious 40-m-long external corridor whose sole purpose, it seems, was to provide exclusive access to a doorless opening directly facing the towers, with room for at most two or three people standing side by side. Around this point, archaeological excavations have revealed offerings of pottery, shell, and lithics, suggesting that significant elements of ritual were involved in the process of passing through the corridor and standing at the end of it to observe the towers. Remarkably, from this western observing point the towers spanned, almost exactly, the horizon rising arc of the Sun. To the east of the towers was a large plaza which, from the archaeological evidence, was evidently a setting for large gatherings and ceremonial feasts. An isolated small building within this plaza seems to have provided an eastern observing point since, as seen from here, the towers precisely spanned the horizon setting arc of the Sun.

The fact that from two evident observing points the towers spanned the whole solar arc (in both directions), rather than just a limited part of it, provides the clearest possible indication that the towers were used to mark and observe the annual movement of sunrise and sunset along the horizon. The obvious interpretation is that the towers formed part of a ceremonial centre where ritual activities were coordinated through observations of the seasonal passage of the Sun. While leaders of this cult had special access to the most precise means of observation, the masses gathered in the plazas to feast could still witness impressive solar effects.

It is well known that in the much later Inca empire, Sun-worship rituals were orchestrated by the rulers in order to reinforce their claim to a divine origin and hence to legitimize their authority. Chroniclers describe towers that stood on the horizon seen from the Inca capital, Cusco, and were used as 'sun pillars' to mark planting times and regulate seasonal observances. These, however, only covered a small portion of the solar arc and have long since vanished without trace. The Chankillo towers provide evidence of early solar horizon observations, and of the existence of sophisticated Sun cults, that preceded these Inca practices by almost two millennia.

The problem with the term 'ancient observatory' is that it carries the vision of ancient astronomers who studied the skies rather like their modern counterparts, making systematic observations in a spirit of abstract 'scientific' enquiry. For the archaeologist, the importance of identifying places from which astronomical observations were made in prehistory, together with evidence on the nature and context of those observations, is that this can reveal much about the ways in which people before the advent of written records perceived, understood, and attempted to order and control the world they inhabited. Ancient sites such as Stonehenge may have incorporated astronomical alignments, but their purpose was surely ritualistic and symbolic, an integral part of a much broader range of beliefs and practices relating to seasonality, life and death, fertility, and keeping human life in harmony with the cycles of nature. The broader context of beliefs and practices was also of fundamental importance at Chankillo.

There is, however, a major difference between the two sites. There is no evidence whatsoever that Stonehenge operated in any sense as a calendar, or even that the solstices were 'observed' beyond the fact that sunlight was seen to pass between or behind stones, or along the avenue, in the right way at the right times. Chankillo, on the other hand, was clearly used for direct, systematic, and precise observations of the changing position of the rising and setting Sun along the horizon. In that sense, perhaps, we can indeed claim that the Thirteen Towers of Chankillo form the earliest known 'solar observatory' in the Americas.

Should we call Chankillo a Peruvian Stonehenge? For me, the answer is definitely no; but the principal reason is that it is about time we stopped thinking of Stonehenge as an ancient observatory.

The President. I have to say I share some of your scepticism about the way alignments have been interpreted in the past. My only comment on the Mayan one, the Caracol tower, is that we do have the Mayan books which show they have fantastically accurate observations of Venus, so they must have been doing astronomy.

Professor Ruggles. Yes, there's abolutely no doubt about that. One of the incredible things about the Mayans is that if it were not for the Dresden Codex or one or two of those other books we really wouldn't know anything about all that astronomy. So yes, they were doing incredibly sophisticated things, and they must have been observing from somewhere. Still, when you look at the Caracol, should you leap to the conclusion that that was the place of observation, and just on the grounds of choosing two windows out of four or five?

The President. Yes, I accept that.

Professor D. Lynden-Bell. I just wanted to ask whether you thought it was significant that there were 13 towers, which is roughly the number of lunations?

Professor Ruggles. That's a very good question. In fact the person who excavated the site, Ivan Ghezzi, was drawn to that very fact. It is certainly true that numbers are laden with symbolic and cosmic significance in many different cultures, and in some cases 'calendrical numbers' are known to have been represented in sacred architecture. The problem is that in a prehistoric context we cannot reliably identify elements of 'their' numerology (*i.e.*, numbers that were deliberately incorporated and had meaning to the builders, as opposed to having arisen fortuitously) by uncritically applying 'our' numerology to the archaeological remains (*i.e.*, fitting numbers that we feel are likely to have been significant). I repeatedly say this to those who insist on seeing a lunar significance in the fact that there are 30 (or, as some would argue, '29 and a half') stones in the sarsen circle at Stonehenge. An American archaeoastronomer, Bradley Schaefer, once pointed out that for virtually every number from 1 up to 100 it is possible (for us) to conceive some astronomical or calendrical significance. Regarding the coincidence between the number of towers at Chankillo and the approximate number of lunations in a year, the idea that this is anything other than fortuitous should only, in my view, be taken seriously if other cultural information emerges to support it.

Dr. J. Steele. I would like to make a comment on the statements you were making about imprecision: if you look at Greek or particularly Babylonian astronomy it was famous for developing schemes, and once those schemes were set up they didn't really need to go out and observe to make sure they were correct; as long as they were fairly correct they didn't need to go out and check them. Also, I guess I'm still a little uneasy about this term 'observatory', like your colleagues. I'm wondering what your reasons are for deducing the Chankillo towers have a calendrical significance? Is there more than just the fact that it divides into 13, that the equinox is in the middle? It has to be if it's an odd number!

Professor Ruggles. I agree. My reasons are purely the fact that the towers span the arc of the rising or setting Sun, and although there's a little bit of leeway at the ends, this fact in itself provides far stronger evidence of their significance as sunrise and sunset markers on the grounds of the alignments alone than any other alignment evidence of which I am aware. Added to this is the fact that the gaps between the towers provided a very accurate means of identifying particular days. On the other hand, no dates known to have had significance within the American tropics (such as the day of solar zenith passage) seem to be particularly marked. Furthermore, while it is true that from the western observing point the Sun appeared between successive pairs of towers towards the centre of the line at roughly 10-day intervals, if this had been calendrically significant one might have expected that the gaps between the towers would be narrower towards the end of the line to compensate for the slowing of the Sun towards the solstices, and this is not the case.

Dr. Carolina Ödman. It occurs to me that the less accurate these alignments are, the more likely they are to be aligned with something. So how do you deal with that?

Professor Ruggles. This relates to one of the main problems with early claims that Stonehenge was a solar and lunar observatory based upon counting alignments between pairs of stones and seeing how many coincided with solar solstices and lunar extremes. Since we do not know how accurate the alignments, if deliberate, actually were, we need to make an assumption. If we choose lower accuracy, then the probability of any given alignment 'hitting' an astronomical target will be increased, but the proportion of the total horizon occupied by the targets will also increase, so a greater number of hits will be required in order to achieve statistical significance. The problem comes if we choose the assumed accuracy in order to maximise the apparent statistical significance. At Chankillo one of the key questions is how unique the observation points were. Clearly if the archaeologist had noted the possible solar significance of the towers, and on that basis proceeded to identify, and subsequently to excavate at, the ideal observing spot, ignoring alternative places that were equally plausible as observing points *per se*, then our claims would be meaningless. However, the archaeology shows quite clearly that the doorless opening was both unique and of considerable ritual significance.

The President. I think we'll have to stop there; thank you very much. Just to mention that, as you know, we are now part of the Burlington House 'cultural campus', and the second lecture in the Burlington House series will be on Stonehenge, to be given here on June 21 by Clive Ruggles and an archaeologist, Professor Tim Darvill. I hope a lot of astronomers will be there to support this important cultural occasion.

Our next speaker is Richard Stamper, from RAL, who is going to speak about 'The International Heliophysical Year.'

Mr. R. Stamper. In January 1875, at the Austrian Academy of Sciences in Vienna, the Austro-Hungarian naval officer Karl Weyprecht suggested a coordinated study of the north polar region, and in 1882–83 a series of coordinated polar meteorological and magnetic observations were carried out during what has become known as the First International Polar Year. Fifty years later the Second International Polar Year was held in 1932–33 and included observations from the Antarctic. Twenty-five years later came the International Geophysical Year (IGY) of 1957–58, with a global perspective involving more than 60 000 scientists from 66 nations, taking simultaneous observations on Earth and, for the first time, in space.

Move on another 50 years and the International Heliophysical Year (IHY) 2007 is a successor to those previous 'Years', operating in tandem with several other 'Years' including the International Polar Year, and the Electronic Geophysical Year. IHY is a natural extension of the IGY in seeking to extend global studies into the heliosphere to include the drivers of geophysical change in the system being studied.

The heliosphere is the magnetic cavity in our local interstellar wind dominated by the solar wind and the Sun's open magnetic field, and exhibits a familiar set of features: working inwards, first a bow shock, then a turbulent heliosheath where the interstellar wind decelerates; a heliopause representing the limit of the outward movement of the solar wind; and finally the termination shock where the solar wind first feels the effect of the interstellar medium. The Solar System sits within all these so we can only observe our heliosphere from within; we see similar structures elsewhere in the Universe, however, such as where the heliosphere of LL Orionis meets the dense interstellar medium of the Orion Nebula.

It is a particularly auspicious time for coordinated studies across the heliosphere. Fifty years on from the IGY there is now an armada of spacecraft providing the most comprehensive global measurements of the Sun–Earth interplanetary system yet obtained, complemented by networks of Earth-based observatories measuring terrestrial effects. *Voyager 1* passed the termination shock in 2004 December, observing an abrupt increase in the strength and turbulence of the magnetic field; the *Ulysses* spacecraft, in its second decade of operations, continues to give our only view of the interplanetary medium from outside the ecliptic plane; the *SoHO* spacecraft is still providing vital solar observations and is now complemented by the *STEREO* mission which will give a unique binocular view of the Sun from its two identical but widely separated spacecraft. Closer to home, the four-spacecraft ESA *Cluster* mission provides a detailed picture of the magnetosphere and has been joined in 2007 by the five spacecraft of the two-year NASA *THEMIS* mission. It may be no exaggeration to say that *now* is the best opportunity for global studies in the next 50 years.

Seeking to take advantage of this opportunity IHY has four strands: science, observatory development, outreach, and history. In the science strand there are five broad themes: evolution and generation of magnetic structures and transients;

energy transfer and coupling processes; flows and circulations; boundaries and interfaces; and synoptic studies of the 3-D coupled solar–planetary–heliospheric system. These are intentionally generic to encourage a cross-cutting approach that explores fundamentally similar processes wherever they occur in the heliosphere.

The detailed scientific programme of IHY is the sum of the specific activities proposed by participating scientists. These are termed 'Coordinated Investigation Programmes' (CIPs) and are modelled on the Joint Observation Programmes that have been so successful for the *SoHO* mission. Anyone can propose a CIP using an on-line form, and all proposals are public. This provides a mechanism for proposing and advertising work benefitting from or requiring collaboration and coordination. To make connections between CIPs and to assist CIP proposers in gaining access to the resources they need, a number of 'Discipline Coordinators' have been appointed for each of seven scientific sub-disciplines into which the CIPs are self-classified, namely: heliosphere/cosmic rays; solar; magnetospheres; ionospheres; neutral atmospheres; climate; meteors/meteoroids. A total of 65 CIPs have been submitted so far, most spanning multiple disciplines, and they can be explored on-line at ihy2007.org.uk/CIP_browser.shtm.

As an example, CIP 1, proposed by Richard Harrison at RAL, is entitled 'Development of algorithms for CME onset and Earth-arrival predictions'. Spectrometry from the *SoHO* spacecraft reveals dimming in the corona just prior to the explosive release of many coronal mass ejections whose progress towards Earth can then be tracked using the *Heliospheric Imager* cameras on the *STEREO* spacecraft. Other CIPs are concerned with using multiple instruments and models to construct a global picture: CIP 2 proposes assimilating ionospheric soundings into a real-time global ionosphere model, with CIP 4 looking at global plasmaspheric dynamics using networks of magnetometers to provide diagnostics of plasmaspheric processes.

A broader campaign is being planned for a 'Whole Heliosphere Interval' running for six weeks in 2007 December to 2008 January. The duration is chosen to be sufficient for a whole 28-day solar rotation plus the solar-wind transit time to the orbit of *Ulysses*. The timing is such that *THEMIS* and *STEREO* will be in good positions to characterize the 3-D structures and *Ulysses* will be over the northern solar pole. The aim is to coordinate observations from as many space-based and ground-based instruments as possible, together with input from modellers, to generate a dataset that well characterizes the quiet heliosphere of solar minimum.

The second strand of IHY is observatory development. This is being pursued principally through the agency of the UN Basic Space Science Initiative. This is a programme of the UN Office for Outer Space Affairs and is dedicated to IHY for the period 2005–2009. The programme brings together instrument providers from developed countries who donate relatively low-cost instruments to host institutions in developing nations. The two then work in partnership to generate high-quality science with an educational and technical legacy. Many networks of instruments are already being deployed — examples include the *AWESOME* VLF receivers from Stanford which can be used for remote sensing the ionospheric D-region, and the *Callisto* instrument from ETH Zurich for low-frequency spectroscopy on solar radio emissions, providing a low-cost method for detecting electron beams and CME-driven shocks.

Public outreach is the third element of IHY, with one of the aims of the year being to "demonstrate the beauty, relevance and significance of space science to the world". Much is going on in this arena: a Franco–Swiss exhibit is touring shopping centres in both countries with an introduction to solar physics, and an exhibit on the Sun–Earth connection is under preparation for the Royal Society Summer Exhibition in July; diverse educational resources are being prepared, including a series of short Japanese comic books that cover many topics in solar–terrestrial physics; on 2007 June 10, more than 40 European observatories will host public events in a coordinated 'Open Doors Day'; there is even a European space-weather multimedia dance show!

The fourth strand of IHY is celebrating the achievements of the IGY on its 50th anniversary. This is principally being done through the IGY Gold Club, membership of which is open to all who were involved in IGY and who can contribute some item of historical significance, whether an object, an account of events, some images, or documents. Members already include the President of India, Dr. Abdul Kalam, who was beginning his career as a solar physicist during IGY. It is hoped to persuade the Duke of Edinburgh to participate too, in recognition of his rôle in IGY presiding at the official launch on 1957 July 1 and presenting a 75-minute television programme broadcast on IGY the previous evening.

The IHY was officially inaugurated at a ceremony at the United Nations in Vienna on 2007 February 19 and will run for two years. To find out more go to the web site at ihy2007.org.

The President. Any questions for Richard?

Professor M. Lockwood. The big legacy of IGY was the world-wide network of ground-based observatories and data centres that we actually still have today. Looking at IHY, the theme is much more that we already have the main facilities — though I hear what you are saying about the small ones; but what do you actually think the biggest legacy of IHY will be?

Mr. Stamper. I hope it will be in the ways we work together. There's still a tendency for people to do their own thing with the small set of instruments or tools they are comfortable with, or to which they were introduced early in their careers. We need to work together in order to make progress in understanding the connections in the heliosphere using data from a much wider range of facilities, and this will be helped if the processes, both human and electronic, have been put in place to aid collaborations.

The President. Can I just ask whether the cuts announced by PPARC in the UK ground-based solar–terrestrial programme impacts on the possibility of UK participation in IHY?

Mr. Stamper. Yes! There has been a lot of scrabbling around to keep some of the facilities going during IHY. The most notable example was the HF radars that were part of *SuperDARN* (*Super Dual Auroral Radar Network*), which rings, almost, the auroral region in the northern hemisphere; in the UK, the Radio and Space Plasma Physics Group at the University of Leicester operate three of those, and they were all at risk of closing this year. So, yes, I believe that the UK facilities will keep going during IHY, but it has been hard work.

The President. Let us thank Richard again. [Applause.] The next talk is by Dr. Carolina Ödman of Leiden, and she's going to tell us about the Universe Awareness Programme, which originated in the Netherlands but has become an international programme.

Dr. Ödman. Good afternoon and thank you for giving me the opportunity to present the Universe Awareness Programme.

Universe Awareness, or UNAWE as it is known, is a new international initiative for a worldwide scientific culture. It aims at exposing very young, underprivileged children to the inspirational aspects of astronomy in order to broaden their perspectives, enhance their understanding of the world, and demonstrate the power of rational thought. By broadening children's minds, UNAWE hopes to stimulate tolerance and internationalism among the future adults they will become.

One might wonder why astronomy is suited for this goal, and why astronomy could be a tool for peace education. Well, astronomy is of course a science and as such it stimulates children's curiosity. It is also a fantastic ambassador for all sciences: physics, chemistry, even biology are subjects of research in astronomy. It leads the development of technology too, and this is very exciting.

Astronomy is also part of our culture. All civilizations have a cultural relationship with the night sky. As such, astronomy is a part of our cultural heritage and links culture and science in a human context. Astronomy is history as well, not only because looking at the stars is looking into the past, but because the history of astronomy is tied closely with that of human development. I am thinking of navigation, calculation of seasons, *etc*. Finally, astronomy is history in the making. Countries like Nigeria have already entered the space age and many others will join us in space in the near future.

What is probably most important for UNAWE, though, is that astronomy is fun for children. It is exotic, far away, colourful, stimulating, and beautiful.

The motivation behind the programme is multiple. We assume that awareness of the scale and beauty of the Universe is exciting for children. We also claim that basic knowledge about the Universe is a birthright. What we mean by that is that the present remarkable achievements in astronomy and space science are the result of thousands of years of human development and as such belong to our global heritage. All children should therefore have the right to access this knowledge.

We also acknowledge that the ages of 4 to 10 are decisive in the formation of a human value system and that knowledge about the Universe can broaden the mind.

Why does UNAWE target so young and specifically underprivileged children? We believe they are less likely to be exposed in any other way to the great beauty of the Universe compared to their more privileged peers. Finally, the youngest are the most similar: four-year-old children are very much the same across the whole world. At the age of, say, 14, they are very different: cultural and cognitive differences are much more pronounced.

The principles at the heart of the programme are as follows: the materials and activities to be developed will be based on inspiration rather than the transmission of facts. This is very important if we want this to touch the children. Internationally, it is also crucial that we adopt a bottom-up approach so that the programme can be tailored to suit the needs of all communities. Finally, we adopt a general approach in which awareness of the Earth, for example, can lead to environmental awareness, or awareness of cultural diversity, *etc*.

The programme itself is composed of three main ingredients. Materials will be developed. It is important that these are developed professionally to compete with the most appealing commercial materials available. They will also have to be translated into languages and cultures across the participating countries. Training is an important component of the programme. It offers a crucial platform for dialogue between the implementers of the programme and the producers of the materials. The level and form of training must be adapted to each country and community. Finally, UNAWE is also an international network of astronomy enthusiasts, outreach professionals, and volunteers who subscribe to the principles of UNAWE. This network offers the possibility for its members to undertake dialogue, exchange materials, and benefit from each other's experience.

We have just entered the development phase of the programme and we aim to start implementing it in a streamlined manner in 2009, the International Year of Astronomy. There have already been pilot activities in Venezuela and Tunisia, for instance.

Universe Awareness is a global programme with collaborators and contacts in over 20 countries. In the European Union, Ireland, Italy, Spain, the UK, and the Netherlands are involved to various degrees. Outside of Europe, Chile, Colombia, India, Indonesia, South Africa, Tunisia, and Venezuela are most active so far.

So how does Universe Awareness fit into the UK context? The UK is a very multicultural society with marginalized communities that UNAWE could reach out to. At the same time, the UK has a number of excellent outreach initiatives on the go already that UNAWE could piggyback onto. The idea here is not to reinvent the wheel but to join existing efforts with the UNAWE goals in mind: 4 to 10 year olds and using astronomy to inspire children.

The last thing I would like to mention is that UNAWE will be present next month at the RAS National Astronomy Meeting in Preston, and I would like to invite you all to join the Education and Public Outreach sessions. This will also be the opportunity to set up a group of people to look into practically developing UNAWE in the UK ahead of the International Year of Astronomy in 2009.

Thank you for your attention and I hope to see you next month.

The President. Thank you very much. Questions?

Mr. H. Regnart. May I make two suggestions? The first is that although you make a very good point about setting up your programme in a way that is culturally accessible to any particular group, it should aim towards being multicultural, as the history and present state of astronomy is, and it can be a way of helping people avoid narrow-mindedness and isolation. And the second suggestion is this: you are targeting a large part of the programme at very young children, some of whom may have had rather limited opportunities; but there may be a small fraction of those children who are potential prodigies, and who will want to go beyond the programme you have devised, into the maths and logic, *etc.*, at an age much younger than the average. May I suggest that you have people on the lookout for such boys and girls? They may be the future leaders of the subject.

Dr. Ödman. Thank you very much for those two remarks. The first suggestion is completely incorporated in the programme already. It has to be multicultural; perhaps I didn't put that across well enough so thank you for pointing that out. As to your second point, I'll bear that in mind and I'll raise it at the round-table discussion next month.

Professor B. W. Jones. Have you had any interaction with the International Astronomical Union (IAU), and in particular Commission 46, astronomy education and development?

Dr. Ödman. We are working hand in hand with the IAU for the International Year of Astronomy, because it's a fantastic platform for both. The vice-president of Commission 46, Rosa Ros, is setting up and running Universe Awareness in Spain.

Mrs. Marguerite Laporte. I noticed on your proposal that you are developing hands-on material, but it didn't mention longer projects. I was part of a team myself, about 35 years ago, and we did exactly what you're doing, with education branches in the UK. We visited schools to work with children from about four years old up to sixteen, and it was astonishing: children have a natural affinity for this kind of thing — anything to do with the Universe — and they also are able to cope very well with it. So I think this programme is good. But do I take it that you will be undertaking extended projects? They are vital to getting the young children to work together, making models of the planets, the Solar System, the Sun, and that kind of thing.

Dr. Ödman. Yes, I didn't want to go into too much detail, but projects are definitely part of it. I would love to talk to you more about this, so let me catch up with you afterwards.

A Fellow. This is an ambitious programme and it's very laudable. I was just wondering how are you funding it and do you have enough money for all of this?

Dr. Ödman. Last October we received a grant for three years from the Dutch Ministry of Education to run the international office; this would be completely separate from, say, a Dutch programme that would be set up in the Netherlands for Dutch children. So there is a strong signal from the Dutch government that they support the international programme. We also have some current funding from ESO and from the Lorentz Centre in the Netherlands. We are now in the development phase and we are trying to raise funds to produce materials; we're aiming for high-quality materials and so we expect them to be expensive.

Professor J. Barrow. In the UK, school teachers for the primary age group 4–10 years are not subject specialists, so you can't necessarily expect them to have any particular knowledge about mathematical science in the way that you might with secondary-school teachers, and they need rather a lot of support. The so-called Science Learning Centres were initially set up over a year ago for supporting science teaching, and there is now a National Centre for Excellence in the Teaching of Mathematics. These organizations are nationally charged with professional development amongst science and mathematics teachers, extending right down to the primary-school age groups. So it's quite important for you to try to make contact with these, certainly with the Science Learning Centres, to make sure the subject material is available as part of the professional-development programmes offered to primary-school teachers.

Dr. Ödman. I absolutely agree with you. Although we don't consider this a science-education programme as such, it is crucial to be in close contact with the people who know how the teachers should interpret material for the children. As a matter of fact, I'm going to visit the Science Learning Centre in Durham next week, so I'm very excited about that.

The President. One last question.

Professor B. Davis. There are a lot of amateur astronomy societies throughout the UK and in many other countries which have events like star parties, and bring schools in to their observatories. I've been along to one on the Isle of Wight, and last weekend we had a marvellous star party for the lunar eclipse, with about 50 people, including families with children of all ages, marvelling at the wonderful event. I think many of these organizations might well be incorporated into your strategy of inspiring the general public, including children, with the wonder of the Universe.

Dr. Ödman. That's an excellent point and we do hope to collaborate with amateurs. One idea is that a club of amateur astronomers may have a special evening when amateurs will spend time working with a school from a disadvantaged area, an urban area, for instance. And you mentioned the lunar eclipse — we also took advantage of the eclipse last week, and we had a Skype-cast: an internet hook-up, with a text-and-voice chat room, between boarding schools in South Africa and school pupils in Germany; we were in Germany, looking at the eclipse, uploading pictures onto a website, running a show for four and a half hours. It was a great success, and people joined in from many countries. It was very, very popular, so the international link between different hemispheres and different time zones was extremely successful. That can be easily arranged, especially in the UK, which has all the infrastructure and facilities.

The President. During the eclipse the RAS website had ten times as many hits as usual [laughter], though it actually didn't have much on the website about the eclipse.

Dr. F. Diego. Along the same lines, the International Planetary Society will be a very good organization to link with, and the European Association for Astronomy Education.

Dr. Ödman. Yes, they're involved. In ten days or so there is a very interesting two-day conference in India, celebrating 30 years of planetaria in India and looking to the future, and they will also be looking at and presenting Universe Awareness there as well.

The President. Well, thank you very much. Don't forget to take part in this event at the NAM.

Our final speaker today is Professor Ofer Lahav of UCL and he's going to talk about 'The Dark Energy Survey'.

Professor O. Lahav. [The speaker described the motivation and philosophy behind the Dark Energy Survey (DES), an international project to map 300 million galaxies using the *Blanco* 4-m telescope at Cerro Tololo. The main goal is to measure accurately the dark-energy component of the Universe. The paradigm shift which started in the early 1990s has led to the current consensus in our understanding of the matter and energy content of the Universe, with about 4% made up of baryonic matter, the rest being divided between dark matter (*c.* 21%) and so-called dark energy (*c.* 75%). These quantities have been determined by a combination of many techniques, including the study of the cosmic microwave background (CMB), large-scale structure, and weak lensing, among others. The speaker emphasized that all these techniques are concerned with measuring the expansion history of the Universe and the growth of structures; since both these properties are sensitive to the nature of the dark energy, it is possible to use these techniques, in combination, to set tight constraints on models of the dark-energy component.

After summarizing the history of our understanding of the dark-energy term — from Einstein's cosmological constant to current ideas about modified gravity — the speaker described how the dark-energy term can be usefully expressed by the equation-of-state parameter, w, which measures the ratio of the pressure to the energy density of dark energy: if $w = -1$, for example, then it is time-invariant (*i.e.*, constant with redshift), and this would indicate that the dark energy is a cosmological constant of the form proposed by Einstein. Or, if the energy density evolves over time (*i.e.*, w varies and is dependent on redshift), this implies a form of dark energy such as an inflationary scalar field. For the condition where $w < -1$, we have the peculiar condition that the density of the Universe increases as it expands!

The challenge is how to measure w, and its variation (or lack of it) over cosmic time, to shed light on the nature of the dark energy — whether a vacuum energy, dynamical scalar field, or modified gravity, *etc.* Many projects have been proposed, which US and European science-funding agencies have been evaluating in terms of their power for constraining w: this has been condensed, for convenience, into a 'figure of merit' which is inversely proportional to the area of the final error ellipse on the measured parameters. The DES, using a new, wide-field camera on an existing telescope, promises to deliver results in the relatively short term, as a step towards the more accurate measurements which will emerge from the larger, longer-timescale surveys such as the *Supernova Acceleration Probe* (*SNAP*) and the *Large Synoptic Survey Telescope* (*LSST*).

For the DES, a wide-field (2·2-degree) camera is being built which will be installed at the prime focus of the 4-m *Blanco* telescope at Cerro Tololo in Chile, to carry out a multi-band, optical (*griz*) survey of galaxy clusters over 5000 square degrees. A smaller patch of sky will be repeatedly visited to detect type-Ia supernovae (SNe Ia). The galaxy-cluster survey will be coordinated with the mm-wave survey of Sunyaev–Zel'dovich (S–Z) clusters being carried out with the *South Pole Telescope* (*SPT*). Four independent, and complementary, techniques will be applied to the datasets: a study using photometric redshifts of galaxies, matched with S–Z-detected clusters from the *SPT*; a study of weak-lensing out to about $z \sim 1$; a study of the galaxy angular power-spectrum (baryonic oscillations) *vs.* redshift; and the detection of about 2000 SNe Ia up to $z \sim 0 \cdot 8$. Each of these four techniques is capable of delivering constraints on constant-w models of the order of 5–10%, and in combination will constrain w at the level of 1%, also providing strong constraints on the possible time evolution of w.

The speaker concluded by noting the involvement of UK institutions in the project, now numbering five, including the speaker's own institution (UCL) where the wide-field optical corrector is being constructed. PPARC has awarded £1·7 million to the project, conditional on the US funding contribution, and other support has come from SRIF3 funding. The projected total cost is relatively modest at $20–30M, with the survey relying on tried-and-tested technology and techniques. The DES is planned to start gathering data in 2010, taking about 5 years to complete, with the first interesting science results available within 2 years.]

The President. Well, Ofer, I would like to ask you a question about photometric redshifts. I'm interested that you selected *griz* bands — I mean, if you are interested in the redshift range 0–2, wouldn't using *ugri* be better?

Professor Lahav. Just to explain to the non-expert, *griz* is a set of Sloan Digital Sky Survey filters. We carried out an exercise, supposing that if one could choose any filters one likes, what would be the best filters? We found that the combination *griz* does a pretty good job.

Professor Barrow. It looks as though a new project with a low figure of merit could be better than one with a higher one if its error ellipse is orthogonally orientated with the previous one, because it is the intersection that is most interesting, not the actual area.

Professor Lahav. That's true, but that's exactly the power of combining independent techniques, because if you have two ellipses, the cross-section will be much tighter than their individual areas — for example, from SN-Ia and CMB constraints. There's another point: you can ask at what redshift you get most the accurate measurements — called the pivot redshift — so the error ellipse could rotate with redshift.

Rev. G. Barber. It seems unsatisfactory that we have no idea what 96% of the Universe is; but the action of dark matter and dark energy on the expansion of the Universe is first to make it devious — to accelerate and then decelerate with inflation, and then accelerate again. Is it something of a coincidence that when you actually calculate the presently understood age of the Universe you actually find it to be roughly equal to the Hubble time, and therefore the Universe appears to have expanded linearly all along?

Professor Lahav. Well, the age of the Universe is a function of these parameters and it comes out to be 13·7 Gyr. The cosmological constant helps because without the cosmological constant the age is uncomfortably low.

The President. I think I'll give another answer, which is to say the age of the Universe is always *about* the Hubble time unless you are in one of these exponential phases. So as you're about to go into one of these exponential phases, then the Hubble constant stays fixed, while the Universe just keeps on getting older, and during the inflationary period the Hubble constant would just be the expansion time. But in the rest of time when other forces are acting it's only a factor of a few between the two ages — that would be my answer.

Rev. Barber. But it is a fact that the age of the Universe is about the Hubble time.

The President. And you expect it to be.

Professor Lahav. We need to take into account important factors that make the difference as to whether the age of the Universe comes out younger than the age of globular clusters or not, for example; those factors are crucial.

Rev. Barber. Or indeed at high redshift — the existence, for example, of quasars with three times solar-abundance iron; it is difficult to explain how ancient features can exist in such a place.

The President. I don't think we can engage in querying the concordance model without losing all credibility.

Dr. Diego. Ofer, when you mention this increase in density as the Universe expands, for $w < -1$, it reminds me of the Steady State theory — just a light comment.

The President. Good question, I think!

Professor Lahav. Yes, it's really a question as to what extent you can believe that $w < -1$, and it reminds us of other models. But, I suppose, one should keep an open mind. If anything, Michael, I won't be disappointed at all if the result of this survey shows that there is no cosmological constant; I would be delighted! You have to do the measurements and see what it gives us — no prejudice, even if $w < -1$!

The President. Well, I think we'll stop it there. Let's thank Ofer again. [Applause.]

We have the National Astronomy Meeting in Preston in April. The next monthly A&G meeting of the RAS will be on Friday, May 11.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Thursday 2007 April 19th at $16^h\ 00^m$
in the Harrington Lecture Theatre, University of Central Lancashire, Preston

M. ROWAN-ROBINSON, *President*
in the Chair

The President. Well, I think we should get started, so welcome to an action-packed afternoon. For the first item, it is my very great pleasure to welcome Professor Reinhard Genzel to give the RAS George Darwin Lecture. Reinhard is one of the leading figures in Europe and in the world in far infrared astronomy — a key person in the development and achievement of the *Herschel* mission. He's Director of the Max Planck Institute for Extraterrestrial Physics in Garching in Germany and also a Professor of Physics at the University of California at Berkeley. The title of his lecture is 'The massive black hole and nuclear star cluster of the Milky Way'.

Professor R. Genzel. [It is expected that a summary of this talk will appear in a future issue of *Astronomy & Geophysics*.] [Applause.]

The President. Well, thank you, Reinhard, for a really wonderful lecture. I wonder if there are any questions.

A Fellow. How often do you expect stars to fall into the central black hole?

Professor Genzel. At a rate of once every hundred thousand years.

Dr. F. Diego. With all these stars coming so close to the centre of the black hole, certainly there'll be a point when the star goes behind it — can you image that?

Professor Genzel. That is what I described at the very end, the motions of spots. In fact, you saw a little simulation there — they were gravitational lenses — you see multiple images. The power of this technique with interferometry means the secondary image formation will be very sensitive to the metrics and the form of lensing. So there indeed you are probing the central region. Now for the more conventional forms of lensing, that would involve stars which are far behind the Galactic Centre where the Einstein radius is much bigger; unfortunately that effect is very rare. You can make a computation and basically if you track the Galactic Centre every month for ten years, then your chance of seeing a gravitational lensing effect of the normal kind is of the order of a few percent. We are always on the lookout for that but we haven't seen one yet and that's consistent with theoretical expectation.

Professor M. E. Bailey. I'm impressed by how often the prediction of theory has been confounded by observations at almost every turn. Do you see any evidence for either periodic or secular changes in the luminosity of Sgr A*?

Professor Genzel. Well, within the flares, we see reasonably convincing but still-to-be-confirmed quasi-periodic light variations, which would be consistent with the orbital motion around a large stable orbit. Any other kind of longer-time-scale variation seems to be chaotic — there's no regularity out there. The interaction between experiment and theory I find extremely rewarding at the present time, and that's why we put together a fairly large team on a large ESO programme on the Galactic Centre, including theorists from the UK, where we can very fruitfully exploit this interaction between theory and experiment. We can actually do this classical physics, where the theorist makes a prediction or proposal and we go out and look for it, and can actually come back a year later and say no or yes.

Dr. V. Debattista. At what level can we currently exclude a second black hole orbiting around the main black hole?

Professor Genzel. Keeping in mind that the answer to this question depends both on the separation of the binary and its mass ratio, it is probably safe to say that one can exclude a binary hole for all but a small part of the likely parameter range.

The President. Let's thank Reinhard again for a wonderful lecture. [Applause.]

The President. We now proceed to the RAS awards, and in this process I am going to be assisted by two postdocs from the University of Central Lancashire, Jane Noglik and Aveek Sarkar. So, first of all, we have the Gold Medal for Astronomy, which is awarded to Professor Len Culhane of the Mullard Space Science Laboratory; Jane will read the citation.

Dr. Jane Noglik. Professor Culhane's PhD work involved the first direct demonstration, with the proportional-counter spectrometer on the UK/US *Ariel-I* launched in 1962, that the Sun's X-ray spectrum hardened during solar flares and was due to emission from high-temperature, 10-million-Kelvin, gas. In 1969 he used the proportional-counter spectrometer on *Ariel-V* to discover emission lines of highly-ionized iron in the spectra of galaxy clusters. This showed clearly that the extended X-ray sources in clusters were due to the presence of large volumes of hot, 100-million-Kelvin, gas. Returning to solar work, he became principal investigator for a series of X-ray and EUV spectrometers on NASA's *SMM* and *Spacelab-2* and on Japan's *Yohkoh* mission. He has served on a number of UK Research Council and European Space Agency committees and as a member of PPARC Council. He is a Fellow of the Royal Society, an Honorary Doctor of Science at Wrocław University in Poland, a Foreign Member of the Norwegian Academy of Science, an elected member of Academia Europa, and has received the Royal Aeronautical Society Specialist Silver Medal and Geoffrey Pardoe Space Prize.

Professor Culhane has supervised and inspired many PhD students — some of whom are now leading lights in astronomy. He developed and expanded the Mullard Space Science Laboratory during his leadership, as Deputy Director for eight years and Director for 20 years. The laboratory now has 150 staff and has been involved in 35 space missions and over 200 rocket flights. He has published more than 180 refereed papers, and written a book on X-ray astronomy. [Applause.]

Professor J. L. Culhane. Thank you, Jane. It is a great honour and a great pleasure to come to this excellent meeting to receive the award in person. I found out about it in an interesting way: I was setting up to give a talk at Trinity College, Dublin, connecting my laptop to the projector, as one does, when my telephone rang and my response, I am sorry to say, was initial irritation, but the call was from Ian Howarth to tell me that I had been awarded this medal. In accepting it, clearly as an observer and experimentalist, I must recognize, and indeed do recognize, a large number of colleagues. I don't want to single out any individuals, but would mention Loren Acton at Lockheed Palo Alto, George Doschek and his group at the US Naval Research Labs, and, of course, at the National Observatory of Japan, Saku Tsuneta and Tetsuya Watanabe, with whom we've done the excellent *Yohkoh* mission. I hope those of you who heard my talks on Tuesday recognize we are about to begin yet another outstandingly successful mission with the Japanese.

Finally I should acknowledge the unusual and outstanding qualities of the activity at MSSL. I would like to single out the Solar Physics Group and Louise Harra in particular, who has taken over as principal investigator for the *Solar-B*, now *Hinode*, mission. I think the extraordinary blend of instrumentation capability and space-science expertise has made MSSL what it is, and it's a great pleasure for me to acknowledge their efforts, without which I would clearly not be standing here in front of you today. Thank you. [Applause.]

The President. The Gold Medal for Geophysics is presented to Professor Nigel Weiss; Aveek will read the citation.

Dr. A. Sarkar. Nigel Weiss is a world leader in studies of solar convection and solar magnetohydrodynamics, carried out by a combination of mathematical analysis and skilled use of the computer. In early work of lasting influence, he analysed the process of magnetic-flux expulsion by inexorable eddies, with the consequent concentration of the field into rope-like structures from which the fluid motion is largely excluded, now a recognized feature of the solar granulation and supergranulation. Over the years, he has remained at the forefront of research on convection and magnetoconvection, extending his studies (and those of his students) to include dynamical effects and compressibility.

Weiss has long been a leading contributor to our understanding of the structure and stability of sunspots. In recent work, he and his collaborators have shown the importance of magnetic-flux pumping by turbulent compressible convection in determining the complex structure of a sunspot's magnetic field.

Weiss's many contributions to dynamo theory include an early illuminating study of a nonlinear model which simulates the modulation of the solar cycle, as observed in the 17th-Century Maunder Minimum. Indeed, he has been a pioneer in introducing concepts from dynamical-systems theory into dynamo theory and other aspects of solar physics, and his work on nonlinear differential equations is of general interest to applied mathematicians.

He has an outstanding reputation as a communicator of deep physical concepts, which have arisen from a long-term, step-by-step understanding of the highly complex and subtle interaction between magnetic fields and plasmas, and have been stimulated by observations. He has supervised a series of excellent students and played an important rôle in serving the scientific community. He was elected FRS in 1992 and was President of the RAS from 2000 to 2002.

In summary, Nigel Weiss is an outstanding recipient of the RAS Gold Medal [G] for his pioneering work in developing our understanding of convection, magnetoconvection, and sunspots, for his stature as a communicator, and for his rôle and example in inspiring many of us. [Applause.]

Professor N. O. Weiss. Well, thank you very much and thank you for reading that flattering citation. It is indeed a great pleasure, an honour, and a privilege to receive the Gold Medal from the Royal Astronomical Society and I am sure my family and I will treasure the award. I am really pleased that it is the 'G' medal that I have received because my research career actually began as a research student in the Department of Geophysics supervised by Teddy Bullard, who received the same medal more than forty years ago. I think I can regard this really as a tribute not only to myself but also to all those colleagues and former research students with whom I have collaborated, some of whom are here today, and it is a joint venture we have been engaged in. It is very gratifying that in your wisdom, and that of the Society, you have given two medals to people working in solar physics, one working in the experimental side and one in the theoretical side. This is obviously a good time for our particular corner of the subject, but I think it also points to something else, that the Sun in a sense is Janus-faced, it looks both ways; it looks towards the Earth, gives us light and heat, and, as I tried to explain yesterday, its magnetic variability affects our climate. On the other hand, it is our star, one that we can observe in real detail — we can look at its atmosphere, look at features on its surface and its magnetic fields in exquisite detail. We can follow its magnetic behaviour back through centuries — millennia — and with the help of helioseismology look at its interior structure and its interior rotation. So it provides a model for looking at all the other stars out there and using those stars to look at galaxies like Andromeda, which we heard about yesterday, and even going to the centre of our Milky Way, following bright stars there, or going out into the Universe. Thank you very much. [Applause.]

The President. The Eddington Medal is awarded to Professor Igor Novikov, Director of the Theoretical Astrophysics Centre, Copenhagen, Denmark, and Jane will read the citation.

Dr. Noglik. Igor Novikov is one of the world's greatest astrophysicists. He is famous for work in relativistic astrophysics, cosmology, and General Relativity spanning more than 40 years. He played an important rôle in leading research in Moscow, from where many of the best ideas in cosmology emanated in the period 1964–1980, and then in Copenhagen, where he has been Director of the Theoretical Astrophysics Centre. He remains an active contributor to the study of the microwave background, large-scale structure, and General Relativity. He is also an experienced popularizer of his subject, who is able to reduce complicated technical ideas to their simplest terms.

As a student, in 1961, he discovered a coordinate transformation that revealed the regularity of the event horizon in the Schwarzschild metric, independently solved in the USA by Martin Kruskal. This clarified a major technical issue in General Relativity that is now standard in all textbooks. Novikov was amongst the first to be persuaded of the existence of primordial heat radiation in the Big Bang model and predicted in 1964 that it should have a thermal spectrum.

In 1964, with Zel'dovich, he pioneered the description of recently discovered quasars as super-massive objects, powered by accretion, whose luminosity could be stabilized by a balance between gravity and radiation pressure. This enabled them to estimate the quasar mass as 10^8 solar masses. He and Doroshkevich established the crucial result that non-spherical gravitational collapse of a sufficiently massive non-rotating object results in a Schwarzschild black hole: all asymmetries are radiated away as gravitational waves. These studies began a big international research effort to understand black holes and culminated in the well-known 'no-hair' theorems of General Relativity.

In 1966, he and Zel'dovich pointed out that black holes in binary orbit with normal stars should accrete material from the companion and be powerful sources of X-rays. These X-ray binaries were first discovered in 1972 and are generally interpreted as providing evidence for the existence of black holes.

Novikov has been responsible for many other important ideas in cosmology. He played a major rôle in uncovering the behaviour of anisotropic cosmological models. With Zel'dovich, he developed the paradigm of the 'pancake' theory for the formation of the first galaxy clusters by anisotropic gravitational collapse. In 1966 Zel'dovich and Novikov were the first to point out that very small black holes can form in the very early stages of the Universe because of the presence of primordial density inhomogeneities. In 1980, Novikov and his collaborator Bisnovaty Kogan were among the first to point out clearly and simply the implications of a light neutrino rest mass for the formation of cosmological structure.

Novikov's work is extraordinarily broad and significant. The advanced texts by Zel'dovich and Novikov were the most influential works for researchers for over 20 years. The leadership provided by Novikov to teams of astrophysicists in different countries is exemplary. His life has been dedicated to astrophysics and we cannot think of a more distinguished and deserving candidate for the RAS Eddington Medal. [Applause.]

Professor I. Novikov. Dear colleagues, thank you very much for this award; it's a great honour for me, of course, and I would like to say a couple of words. First, Professor Eddington was my hero from youth. His famous book on General Relativity was my first serious book in the field, thus I can consider myself a pupil of Professor Eddington and I am happy that I was awarded the medal bearing his name. Second, I dedicate this award to my family; my wife, my son, my daughter are astronomers and they helped me and supported me in all aspects during my scientific career. With my son Dmitri Novikov, who is an astronomer at Imperial

College London, we published a book on the physics of the CMB. I am happy that my children continue the traditions of the great astronomers. Thank you. [Applause.]

The President. The Award for Service to Geophysics is presented to Professor Aftab Khan of the University of Leicester.

Dr. Sarkar. Aftab Khan's career, from entering geophysics at the University of Birmingham in 1953 to a professorship and present status of Emeritus Professor at the University of Leicester, has been singularly devoted to the promotion and promulgation of his science to all facets of our society: schools, business, media, adult education, amateur and professional societies, governments, and the developing world. His audience ranges from questioning school-children to industrial chiefs and the presidents of nations.

Aftab Khan has been Vice-President of the RAS, Associate and Managing Editor of *Geophysical Journal International*, President of the Joint Association for Geophysics, member of the Royal Society Explosion Seismology Working Group and of the Earth Resources Committee, and a regional advisor of the American Geophysical Union. Throughout his career he has always been keen to assist in the development of educational systems in the developing world, in particular in Kenya, but most recently in his native Trinidad, where he has been heavily involved in the development of petroleum geoscience teaching.

His research contributions again reflect his selfless organizational abilities. In 1981 he persuaded the pick of Earth-science 'rifters' to focus on the East African Rift System. The Kenya Rift International Seismic Project (KRISP) was the outcome.

Aftab Khan has been a stalwart servant of UK geophysics for over 50 years. He has a plethora of friends and colleagues across the world who know that his love of the science has engaged their own. His production of the RAS and Geological Society report into UK Geophysics Education has involved promotion of the science to government, the press, and industry. He is a true servant of the science, a worthy recipient of the RAS Award for Service to Geophysics. [Applause.]

Professor M. A. Khan. Well, thank you very much for that wonderful citation. For a moment, I thought I was in another place, listening to my obituary! [Laughter.]

I am, of course, greatly honoured, as a mere geologist to be recognized by a body of distinguished physicists, mathematicians, and astronomers. Some time ago, way back in the last millennium, I did receive another award and curiously enough it happened to be in mathematics, and on the basis of that I was advised to go to university to read petroleum engineering, which was a natural thing to do, coming from an oil country. It meant, however, that I had to go to Birmingham rather than Cambridge, where such dirty subjects were not taught.

Anyhow, very quickly I realized that petroleum engineering is rather dull and that geology is much more interesting, so I changed to geology but also decided to do physics, and I think everything that I've done since has been strongly influenced by physicists. As an example, as an undergraduate my teachers included Philip Moon and Rudolph Peierls, both of whom were involved in the in the Manhattan Project in Los Alamos, a place which I happen to have visited a few times since. And subsequently, about fifty years after the Trinity test, I was the coordinator of a team of British scientists to record a Soviet nuclear test within the Soviet Union — we were the first people to do that — and to demonstrate that it was possible to verify a comprehensive test-ban treaty by seismic means. Of course, in the early '90s a treaty was put out for signature, not everybody signed it, and some of those who signed it have broken it, but that was an interesting collaboration.

I did my PhD starting with P. M. S. Blackett who was very interested in reversals of the Earth's magnetic field and said I should go to Skye, which at the time for a young undergraduate seemed like the end of the Earth. It took me three days to get there by bus and boat but I did find one person in the University of Birmingham who knew something about Skye and his name is Martin Johnson — not the captain of the Leicester Tigers and England's championship team who could teach a thing or two to the cricket captain in the next county, but I won't go there! Martin Johnson was an astrophysicist and an energetic little man, known affectionately by his students as [$^1/_2$]mv^2. My PhD supervisor was also a physicist, Roy King, and he was the brother-in-law of the Professor of Geophysics at the University of Birmingham, Don Griffiths. Both have been great friends and collaborators since those days, through to the present time.

Edward Bullard, who was mentioned a bit earlier, was also one of my great mentors because in the 1930s he did some pioneering work measuring gravity in Africa using pendulums; he made some great observations, but came to the wrong conclusions. This he recognized and he was very supportive when I started working there in the 1960s.

And in the University of Nairobi I met Alan Mussett, who was in the Physics Department; he's been a great friend and collaborator ever since and together we wrote a textbook, in which he was the lead author, called *Looking into the Earth*.

In my time, I have always worked in Geology Departments, under Professors Shotton at Birmingham and Sylvester-Bradley (Leicester), both interested in things like micro-palaeontology but who saw that the future of geology lay in physics. One of my great collaborators was somebody who was a physicist in the Geology Department in the University of Leicester, Peter Maguire, who worked with us in Africa for many years; interestingly enough, it was announced last week that Peter has been awarded a Coke medal of the Geological Society — an interesting reversal that I should be here among physicists and astronomers, as a mere geologist, and he the physicist should be recognized by the geologists. It augurs well for the two subjects and the two Societies which support their hybrid, geophysics.

I must finish by saying that I would not be here today if it had not been for the fact that I went to the University of Birmingham. There I met my wife, who was a great supporter and mentor throughout and was very tolerant of very long absences in distant parts of the world, something else that I think geologists can help the space travellers of the future to deal with.

I thank you very much indeed for this award, for work in geology and geophysics, and my special thanks to the Executive Secretary for choosing this very handsome award. Thank you very much indeed. [Applause.]

The President. The Fowler Award for Astronomy is awarded to Dr. Graham Smith of the University of Birmingham.

Dr. Noglik. Dr. Graham Smith has been awarded the RAS Fowler Award for Astronomy for 2007 in recognition of his early career achievements in advancing our understanding of the distribution of dark matter in galaxy clusters, and the impact of his work on attempts to measure the dark energy of the Universe.

Until recently, many observations of galaxy clusters, especially at X-ray wavelengths, were interpreted as evidence for the majority (70%) of massive galaxy clusters being simple, relaxed, self-similar systems. Dr. Smith's results are overturning this paradigm. His gravitational-lensing studies of massive clusters indicate that most of these systems have a complicated (often multi-modal) distribution of dark matter, and therefore are not simple or relaxed.

The research relevant to this nomination was begun when Graham was at Durham in 1999–2002. Earlier X-ray studies had derived total mass from the X-ray emission from the gas assuming hydrostatic equilibrium, and correlated it with temperature, obtaining a tight relationship indicative of self-similarity in the mass distributions of clusters. In contrast, Smith's mass–temperature relationship was based on cluster-mass measurements derived from gravitational lensing — no equilibrium assumption was therefore required. Smith's mass–temperature relation resembled a scatter plot, rather than a tight scaling relation.

A significant landmark arrived in 2006 April, when Dr. Smith was awarded a very large allocation of observing time (143 orbits) on the *Hubble Space Telescope*. Smith now leads the Local Cluster Substructure Survey (LoCuSS), a large international collaboration which will analyze and interpret galaxy-cluster data from several leading observatories in pursuit of accurate cluster scaling relations and a robust understanding of the astrophysical origin of the scatter in these relations. [Applause.]

Dr. G. Smith. I'll be brief, partly because I've been asked to be [laughter]. I'd like to thank the President and the RAS for making the award. Like everyone else today I acknowledge many collaborators and I would like to single out three people with special thanks, roughly in chronological order: Ian Smail in Durham, Jean-Paul Kneib now in Marseilles, and Richard Ellis at CalTech; and finally to acknowledge my family for their wonderful support and patience, thank you. [Applause.]

The President. The Fowler Award for Geophysics is presented to Dr. Duncan Mackay of the University of St. Andrews.

Dr. Sarkar. Duncan Mackay is one of the best of the new generation of young Solar System physicists in Europe. He already has an outstanding international reputation and is developing into one of the leaders in the field. He was awarded a PPARC Advanced Fellowship, followed by a lectureship at St. Andrews on completion of the Fellowship.

His early research concerned the magnetic structure of prominences on the Sun. Many of their properties were not understood at all, but he tackled several of the major questions in this field with great effectiveness. Since then, he has branched out in several directions to focus on key topics in solar coronal physics, notably in coronal heating, the solar cycle, and coronal mass ejections, producing landmark papers on each of these important topics.

A major emphasis of Duncan's research has been an in-depth comparison between theory and observations. For example, he has constructed realistic models of prominence formation and of the large-scale transport of magnetic flux across the solar surface, which is crucial for understanding the solar cycle and solar eruptions. Duncan Mackay is an outstanding communicator with a real desire to teach and inspire students. As one of the most exciting young talents in the UK, he is highly worthy of the Fowler Award.

Dr. D. Mackay. I'd just like to thank the many people who have supported me over the years — at St. Andrews, Eric Priest, sitting over there, and Alan Hood, but in particular, people with whom I have collaborated including Dr. Vic Gaizauskas, from the Herzberg Institute of Astrophysics, and Dr. Aad van Ballegooijen, from the Center for Astrophysics at Harvard. Both Len and Nigel have described the wonderful state which solar physics is in with new missions, and also with the theory work continuing. I hope to continue on this to consider again coupling the theory with observation to improve our understanding. [Applause.]

The President. And we now turn to the Cambridge University Press RAS Poster Prizes, and Margaret Penston is going to announce them.

Dr. Margaret Penston. Well, first of all, this competition is run under the auspices of the RAS Higher Education Committee and as Michael said it is sponsored by Cambridge University Press, so obviously I am very grateful to them for the prizes. And they have generously provided two top prizes of £100 book vouchers and four runner-up prizes of £50 book vouchers. Now, the conditions for judging are on the web page so I won't go into all that, but generally we look for completeness, style, readability. We didn't judge the science, it was impossible to judge solar physics against cosmology, so mostly it was the appearance and if there was a good story and that sort of thing.

The posters were of a very high standard and it was very difficult to judge. There are four commended prizes; I'm afraid those people just get a thanks and a "well done", and perhaps a piece of paper from the RAS. If I ask you to stand up: Melanie Hawthorne from IoA, Cambridge, Jason Byrne of Trinity College, Dublin, Alan Wood from Aberystwyth, and Samantha Penny from Nottingham; are any of those four here? Well done. [Applause.]

The four runner-up prizes — again if you can stand up we will applaud you and you can see me later and I will give you your voucher to take to the CUP desk. So, Stuart Sale from Imperial College London, Christina Chifor from Cambridge, Nicola Longden from Lancaster, and Nicholas Wright from UCL. Well done. [Applause.]

Our two winners are Fred Dulwich from Bristol and Anthony Yeates from St. Andrews. So, well done all of you! [Applause.]

The President. Thank you very much. The next award we will announce is the Rishbeth Prize; this prize is awarded to the presenter of the most interesting and influential talk in the area of magnetospheric, ionosphere, and Solar System–terrestrial physics. It comprises a £50 cash prize and an invitation to write up the winning presentation as an article for publication in *Astronomy & Geophysics*. The winner of the 2007 Rishbeth Prize is Dr. Mina Ashrafi from Southampton for her presentation on 'Auroral structure and kinematics: results from the new multi-spectral imager'. Congratulations. [Applause.]

The President. Right, now it's my great pleasure to express thanks from the RAS to the University of Central Lancashire for organizing this conference, including sponsorship and the welcome reception. The people that I should mention are the Vice-Chancellor, Malcolm McVicar, the Chair of the Local Organizing Committee, Professor Gordon Bromage, the Science Programme Coordinator, Professor Brad Gibson, the webmasters, Dr. Stuart Eyres and Mr. Steve Chapman, and the Conference Coordinators, Emma Kelly and Gail Simpson. Before we show our great appreciation for the organization of this conference, I should say that they only had one year's notice for this because a previous plan to go over the water collapsed. Most organizations have about three years to prepare for hosting this vast conference, so I think they've done an amazing job. Several people have said to me how wonderfully they thought this was organized and I hope you all agree with that. Let's show our appreciation. [Applause.] We should also mention our largest sponsor, which is STFC, so can we thank them too? [Applause.] The next NAM will be held in Queens University Belfast, 2008 March 31 – April 4.

Now, before we break for tea, I want to take advantage of this large audience to tell you a little about what's happening in the RAS. When I became President I put forward some ideas about how the Society should develop, which have rather grandiosely been called the 'President's Action Plan', and I want to say something about that, too. So first, the components of this were to continue to develop the RAS, the professional organization representing professional astronomers, especially to expand our activities on science policy and education and public outreach, to encourage a greater proportion of the community to join the RAS, and also to plan for the long-term viability of the Society.

On science policy we have actually been extraordinarily active in the past year, and if you've been following, for example, the RAS web page every few weeks, you will have seen some new activity or initiative. We've appointed an RAS policy officer, Dr. Robert Massey, who has been tremendous, and he is also our press officer and has been very active this week. The first thing that came up last year was the Treasury's 'Next Steps' document, which laid out the importance of science in the future of the economy, but it also announced that they were minded to merge PPARC and CCLRC. Basically we had to respond to this, and we set out what we thought were the principles for this merger; we decided it was hopeless to try totally to oppose it, even though we didn't really feel there was any great need for change, but we thought it more important to try to manage the change and make sure the organization suited us. We co-sponsored a town meeting with the IoP, and we lobbied while the thing was being designed in PPARC, in CCLRC, and in the Office of Science and Innovation.

Almost immediately, while that was still going on, there was a second consultation about the Research Assessment Exercise (RAE) to which we contributed, and not so soon after that the House of Commons Select Committee on Science and Technology started an enquiry into space policy, and again, the RAS provided views on that. I was called to give evidence to the committee, first by giving a seminar, and then giving public evidence. As part of that process the issue of human space exploration was quite a key question, and so we had an electronic vote of the membership, which achieved a good turn out, and a rather Soviet-style majority of 97% or so were in favour of the RAS statement. In a way, then, the most important activity has been lobbying on astronomy funding, in the build-up to the next comprehensive spending review, and some of you may have seen my article in *Research Fortnight* about how astronomy does help the economy in surprisingly indirect ways sometimes. Over the year I've been at the Treasury, and the OSI, banging away about the importance of blue-skies research and trying to persuade them to keep funding us. Recently we wrote a letter to Alistair Darling complaining about the DTI raid on PPARC funds.

One of the most important ways in which astronomy helps the economy, I think, is through its impact on school pupils, and students deciding what degree to study at university, and we believe there's plenty of evidence that astronomy (meaning astronomy, geophysics, space science, Solar System science — all our activities) draws pupils into science and draws university students into physics. This is widely accepted although it is a slight problem that our evidence tends to be anecdotal, and we're trying to see whether we can produce a more quantitative case to demonstrate this. There was a very good day on Tuesday on education and public outreach and the session was humming with enthusiasm and ideas. So this is expanding enormously and I urge you all to support this activity in your local institution and in any way you can, because I do think it's one of the ways that we contribute to the UK economy and justify the spending on astronomy.

I do think, in order to speak for the community, it is important that the community belongs to the Society. Currently we have about a thousand professional members, roughly a thousand enthusiasts of various kinds, and a thousand overseas members who are probably also mainly professional. Our estimate of the proportion of professional colleagues who are members is about 70%. We want to try to increase that, and so far I've had my own little campaign directed at the Professoriat, which seems to be working quite well. I think we're up to 90% there. Now we are working on everybody — academics, postdocs, and postgrads. The current deal for postgrads is so good I can't imagine how you can resist joining the RAS — I will be amazed if you haven't joined. We now want also to try to encourage postdocs to make sure they join the RAS and we're going to have a new improved offer to first-year postdocs to try to encourage that. My personal goal is that we should be aiming for 90% of all categories as members of the RAS.

The future of the RAS — well, the NAM is a very important part of it. A fantastic attendance here, wonderful meetings, but I think we should keep on trying to build on that and make it an even more significant kind of meeting, and I really feel nobody in the field in the UK can afford not to be here. It should have a place in everybody's diaries each year.

One on-going thing, that is in some ways rather tedious, is the refurbishment of Burlington House. We can't use Burlington House this year, but when it's finished, hopefully in the autumn, it will be a wonderful facility for you all. There will be a lecture theatre, other meeting rooms which you can hire, Fellows' rooms, wireless access so you can drop in and read your e-mail, and a modernized library. So I think it will be a great asset for you when it's finished. Our journals, of course, are incredibly important to the Society, but we also have to think about what's going to happen in the future under the pressure to move towards open-access journals. There is a lot of international pressure to make journals open access, and that's going to be an issue for the Society. The new initiative, which I expect you've heard about, is the RAS is having its own book list with Springer.

In the longer term, although I'm placing the emphasis on making the Society more professional, I think we also have to find a way to expand our membership amongst the enthusiasts, because I think the Society is perhaps too small to survive in the long term. We have a very important rôle which can't be satisfied by bigger organizations like IoP because they have to speak to the whole of physics. The RAS can speak for our particular science, can talk about blue-skies research, and so has a crucial rôle, so we have to make sure that's it's going to survive in the long term. Thank you very much, and we'll now break for tea. [Applause.]

THE SCIENCE AND TECHNOLOGY FACILITIES COUNCIL FORUM

The President. Welcome to this Science and Technology Facilities Council (STFC) forum. We have a panel here of great expertise and involvement in this enterprise, and I will chair the discussion. First I will ask the members of the panel to introduce themselves, and say a little about their rôle in this venture. Then Richard Wade is going to give us an introduction to STFC.

Professor E. R. Priest. I'm from St Andrews, I am a theoretician and currently a Vice-President of the RAS, and therefore I am supporting Michael in some of the new initiatives that he mentioned in his presentation before the break. In particular, one of the things that we want to do very much is to have a constructive relationship with the new Research Council. We are not going to agree with everything that they are going to do but we want to feed in your views and to communicate much better with you. So I feel that it is very important that if there is anything you feel strongly about astronomy in the UK you should get to know who are the members of the RAS Council and communicate with them; this will then get fed back into the Council meetings. Also, a few years ago I was Chairman of the Science Committee of PPARC. I am also on the RAE panel for mathematics, so we will have a lot of theoretical astronomy to consider as well.

Professor W. Gear. I'm from Cardiff University and until two weeks ago I was Deputy Chair of the PPARC Science Committee. The rôle of this in the new system is still to be defined but whatever the new equivalent turns out to be we certainly value the opportunity for input. We advised PPARC on what the community currently does.

Professor G. Bromage. I'm currently the Chair of the Standing Conference of Astronomy Professors (SCAP), which is affiliated to the RAS. The SCAP met yesterday and we have certain views on the way in which STFC is going, in particular with regard to grants, fellowships, and studentships. I'm also one of four from the astronomy area sitting on the panel for the 2008 RAE — this is completely separate from STFC and the RAS. It is the the other side of the dual-support system for astronomy research in this country. We'll be deliberating on all areas of all universities during 2008.

Professor M. G. Edmunds. I'm from Cardiff University and really I am wearing two hats here. One is that I am one of the members on the new STFC Council, another member of which is Anneila Sargent from Caltech. I am also, however, a co-opted member of Council of the RAS standing for election in May, so hopefully I can act as a conduit of some kind.

The President. Finally, Richard Wade, who is Director of Programmes at STFC, is going to tell us something about the rôle of STFC.

Professor R. Wade. I quickly want to give you an introduction to the STFC, how it matters to you, how it works, what its vision is, and basically what it is going to be doing in future, which will be concentrating on developing science strategy. Essentially STFC is a merger of PPARC and CCLRC which came into being on 2007 April 1. A year ago we discussed this at the NAM and it had not long been announced in the science budget. In addition to the merger there was also a transfer of Nuclear Physics from EPSRC. It also involves the creation of the Harwell and Daresbury campuses, and I will say more about these later.

So what is the scope of the STFC? We have the international subscriptions to CERN, ESA, and ESO and we also now have the Institut Laue-Langevin (ILL), and the European Synchrotron Research Facility (ESRF). As with PPARC, these subscriptions make up a huge percentage of the budget. We are also responsible for facilities in the UK, such as neutron and light sources including *Diamond*, and for telescopes in the UK and overseas. And, very importantly, we are responsible for the astronomy, particle-physics, and nuclear-physics grants as well as the studentships and fellowships in those areas, hence astronomy is a very important part of the STFC's remit. Our job remains to promote and support world-class science. You will be aware that PPARC was an organization whose business was virtually all international, and getting greater international clout across a broad range of science and scientific facilities is something we wish to transfer from PPARC to the new Council. Finally, greater economic impact — we constantly have to persuade Government that whilst it may not affect you, it certainly affects a lot of the work that the STFC does. Both PPARC and CCLRC had successes in knowledge transfer with spin-off companies and technologies developed, but both the Government and STFC want to go much further and so the Government announced the science and innovation campuses at the Daresbury and Harwell sites.

The Daresbury campus is already up and running and contains the Cockcroft Institute for Accelerator Science, which was created cooperatively by PPARC with the universities of Liverpool, Lancaster, and Manchester along with the NW Development Agency and the local council, and it complements the John Adams Centre for Accelerator Research in Oxford which we also set up. Further development of scientific innovation can take place on land already available at Daresbury. Nearby is a conventional science and business park and it is hoped that eventually the two areas can join together to make one huge campus. At the Harwell site, two huge new buildings have arrived: the *Diamond* light source and the second target station for the *ISIS* neutron source. These two sites are going to act as the focus for university researchers and industry to come together to form something truly world-class.

So, how does STFC work? STFC is a very small Council which is run differently to either PPARC or CCLRC. It is different in two senses. Firstly, Council has a mixture of science, industry, and executive members. It is chaired by Peter Warry from PPARC, and includes Keith Burnett from the CCLRC Council. It also has Mike Edmunds and Phil Kaziewicz from PPARC, and Keith Mason is Chief Executive with myself, Colin Whitehouse, Anneila Sargent from Caltech, and industrial members Marshall Davies and Philip Greenish. There are no particle physicists on the Council but you will have noticed that there are two astronomers. Of the non-executive members, three come from the PPARC and CCLRC councils. It is planned to have short meetings once a month which will concentrate on governance. It will not make decisions on scientific choices in the way that the PPARC Council did — that will be devolved to the Science Board. The Council works *via* a science advisory structure; at the top is the Council and Chief Executive, there is an international advisory committee, but the main body that advises Council is the Science Board. In many senses the Science Board takes on the rôle that PPARC Council used to. I run the Science Programme Office which works closely in an iterative way with the Science Board, which in turn is advised by two Science Committees. The first of these is the Particle Physics, Astronomy and Nuclear Committee (PPAN), which does what PPARC's Science Committee did but with the addition of nuclear physics. Secondly there is the Physical and Life Science Committee (PALS). PPAN not only looks after facilities but also grants, fellowships, and studentships, whereas PALS primarily looks after the provision of facilities for physical and life sciences.

Those two committees will both propose scientific strategies and will have devolved authority to advise on scientific budgets; in particular, PPAN will advise on how grant funding is used, how it is divided up, and come forward to the Science Board with proposals; those committees will be advised by a structure underneath which may comprise the user community. We are looking for input as to how all this will work but it will be for the Science Committee and the Science Board themselves to determine how to constitute this structure. This will replace what in PPARC were advisory panels, which few people thought were a very effective mechanism for doing anything at all. Under the PPAN science committee we have the familiar astronomy grants panel and particle-physics grants panel and a new body which is the nuclear-physics grants panel. Each will be set up and given a budget to advise PPAN on how grants should be given out. The progress on setting up this structure has been a little slower than we would like. We called for nominations before STFC was set up and we are working through the long list with an internal nominations panel to put together the committees. It is essential to get the right balance of expertise, as well as geographical and gender balance.

We have decided that we will interview all the people we think are suitable for the Science Board and then the Chairs and Deputy Chairs of the other committees over the next few weeks. The intention of the advisory structure is to work with the Science Programme Office in the area of tension and investment choices. Most importantly, we need transparency in decision making and recognizing choices in terms of delivery. In other words, a lot of what is needed for STFC can be provided by the universities, in-house labs, and international labs, and we need to make sure that in making those decisions it is clear why the choices that are made have been made.

The final topic I want to discuss is science strategy. We are going to have a single science and technology strategy covering the whole of STFC activities, and when I proposed this some time ago many people said that it would be impossible. We are going to do it anyway because what we do must be driven by science strategy — it can't just be for astronomy and particle physics, it needs to be for the life sciences and physical sciences as well. To start that process over the next year we need to do three 'impossible' things. Firstly, we must recognize the science opportunities that might arise over the next 15 years or so. Based on those opportunities we have to prioritize short-to-medium-term investment projects in the context of the funds available and then we have to carry out another programmatic review to see how the projects should move forward in the future. There are some major European projects currently pressing the UK for decisions but we cannot make such decisions at the moment because we need to work rigorously through the science strategy.

The next points were made by John Womersley, Director of Science Strategy, who made them before the particle-physics audience, bravely in my view. There is a danger of spreading resources too thinly and we need to say "no" to good science, which is easier to say than to do. It is better to say "no" sooner and it is better to say "no" to a whole project rather than part of a project. The science must be adventurous, not solid, and we must be doing new things. The UK should be leading projects and not acting as minor players; there is no shortage of such projects but we must concentrate on the areas where the UK has a record of excellence. The strategy will inform the detailed STFC scientific-investment plan against the budget set by Council, *i.e.*, detailed long-term plans now which will tell us how it will affect things in five or ten years' time. This is a strategy which comes up from you and we make it happen, and that is why the advisory structure is so important. The plan needs to be clearly explained to STFC staff and 'stakeholders'. [Applause.]

The President. You now have about an hour for questions. Please make questions and comments brief. If a question is directed to a particular member of the panel, please say so; similarly if you want to hear the view of the whole panel make that clear.

Dr. D. Carter. Richard, can you define what you mean by 'lead' and 'project'; *i.e.*, by 'project' do you mean scientific project or technology project and by 'lead' do you mean provide a scientific lead or build the hardware?

Professor Wade. Principally I mean to provide a scientific lead. It is sometimes tempting to build something for which you are not taking the scientific leadership and sometimes the two go together. Even if we are not the leaders in a project, we should be playing a leading rôle in what we are doing. This is easier in some subjects than in others.

Dr. Carter. If you provide the leadership in a niche which is someone else's project, would you class that as leadership?

Professor Wade. You need to look at each project separately. If it is a major scientific project and you are leading in a particular scientific facet of it, or, if you are leading in a predominantly UK project, or, to take an extreme position, a Nobel Prize comes out of the work and goes to a Briton, then that should be given greater weight when we come to decide whether it should be funded or not.

The President. Would any of the other panel members like to comment on this in so far as it possibly represents a change in philosophy?

Professor Gear. I don't think it represents a change of philosophy because on the Science Committee recently where I have been involved there have been many occasions where projects come up and you have to balance what appears to be a high price for a relatively small UK rôle in a cutting-edge question, compared to the UK playing a leading rôle in a niche area. It's a balance, and I think the Science Committee and the grants panels as well are familiar with those kinds of trade-off.

Professor Yvonne Elsworth. I actually wanted to ask the same question but with a different tilt on it. I'm very sympathetic to what you say but I would like to know where the evolution of the next generation fits into the concept that you go into the project as the leader. How do your new researchers fit that rule and how do new ideas fit into it?

Professor Wade. On the PPAN side of it we have fellowships, studentships, and grants so there is a lot of scope in there for people to build up activities and we need to look at ways to be more responsive. One of the things we have tried to do in PPARC over the last few years is to look where these ideas are coming from, and if you were in the last session this morning you would have heard about the leading rôle that Britain is taking in the *SKA*. Peter Wilkinson can tell you that PPARC has been very responsive in order to get the *SKA* team to that leading position. The same is true in other projects such as *ALMA*.

Professor Elsworth. What I'm looking for are hooks on strategy. I'm utterly convinced that the goodwill is there but the if up-front strategy says nothing about how you start things then it sends out the wrong message.

Professor Wade. The answer is that the strategy has to be responsive to new ideas.

Professor Priest. I think there are lots of good features in what is being proposed, one of which concerns standard grants, which I liked very much. At the moment if you are in a group with a rolling grant then you are barred from having a standard grant, but there is now a responsive-mode scheme which means that if you have a new idea you can apply for a standard grant even if there is already a rolling grant in your group. I feel that potentially this is very important, especially for young people. Many young lecturers, advanced fellows, and so on, who are in big groups with rolling grants, are limited because they cannot have a postdoc to work out their own idea, and I see this scheme as being one way to work out that very important demand. In EPSRC young lecturers can apply for a specific scheme. We don't have that in PPARC or in the new Research Council and I'm hoping that this responsive-mode scheme will be a way of satisfying that very important need.

The President. I wonder if what you are referring to was really selection of big projects rather than, let us say, standard grants. I was thinking in terms of joining 'Project X' or do we award a grant for one postdoc?

Professor Wade. I think to some extent it extends across what we do. We should expect our grants panels and our science committees to be asking the same sorts of questions and the weighting you give to the different answers may vary depending on the project, but the question you should always ask is: is the UK taking a leading rôle in this? Is this really important science, will it have a major impact? Does it have an economic impact or impact in terms of science outreach in society? The balance you give to the answers is a bit of an art.

Professor Priest. Don't you agree that with all the different criteria, by far the most important is the quality of the science. That is what drives the new science and leads to major discoveries.

Professor Wade. Absolutely.

Professor Priest. So I hope that when we have to tick all these boxes, many of them will be regarded as secondary.

Dr. Penston. A question to Richard about gender balance. I'm disappointed to see only one woman on the new Council. There are none out in the front today. What are you doing to ensure gender balance?

Professor Wade. Let me tell you what we are doing as part of this nomination process. We called for nominations and we got lists for each of the different panels. As we went through these lists we tried to identify the outstanding women. We tried to get the composition of each panel right. We then went back to the list again to see if we had missed any women from the list who could be on the panel. I'm not in favour of positive discrimination — the women should be on these panels because of their own strengths and I believe that we have bent over backwards to ensure that this is the case. When you see the results they will not be quite what you hoped for, *i.e.,* they are not 50/50, although one of the panels will come out close to 50/50; on the whole we are failing to meet that as a target, but it is not for the want of trying.

The President. Yes, I believe that one woman dropped out of this panel, but as far as the RAS is concerned the last two Presidents have been women and if you look at the keynote speakers in the NAM meetings there has a been a fair balance of gender; but I hope everyone is conscious of the need to do this.

Dr. Penston. Yes, I think the RAS have been good at this and that there have been lots of equal opportunities for women, but not in PPARC. One way of getting women on panels is to have then on the selecting panel in the first place, because people tend to choose themselves, *i.e.*, middle-aged white men. [Laughter.]

Professor Elsworth. I chair a women's forum which existed within PPARC and we have constantly complained about the fact that the big money-awarding committees have almost all have been exclusively male, and I will be interested to see if it is the Education and Training Committee that turns out to be 50/50.

Professor Wade. It's not.

Professor Elsworth. Good. We did instigate a survey of women in the community and have provided PPARC with the database and, given that statistically women do not nominate themselves, I would hope that you have been more proactive than just taking the people who have nominations.

Professor Wade. We used that list and that is how a lot of names got on to the nominations.

Professor Elsworth. Fantastic!

Professor I. D. Howarth. Could you say a bit more about the relationship between the PPAN side and the life-sciences side? It was my impression that you said PPAN would run grants and facilities and life science would be only grants or only facilities, and how do you tension those two things? Is there any danger that the astronomy grants line will be diverted into the Human Genome Project?

Professor Wade. PPAN will be responsible in the area of applied physics, astronomy, and nuclear physics for grants, studentships, fellowships, *i.e.*, the whole gamut of what we used to do in PPARC. On the PALS side they are coming up with proposals where we should develop and upgrade facilities for life sciences because we are not responsible for the grants in those areas. At some level you are actually right that we have to tension the requirements of those two and that happens at the Science Board. That tensioning has to be applied across a huge range of science. It's for the Science Board to look at how that balance evolves in the future. I think it would be totally wrong to say that in the long term the balance in funding between different areas of science will remain exactly as it is today. In PPARC we let this free-run — when PPARC was created the spending divide between astronomy and particle physics was roughly 50/50 and on the day that PPARC closed down the balance was still almost exactly 50/50. That was done on the basis of scientific excellence and not on the basis of ring-fences around particular subjects. I don't see this as a danger, I see it as a strength in the organization.

Professor Howarth. Will the initial funding levels be set at the same level as the previous funding?

Professor Wade. It's been a real rush to get this Council up and running and we are about to lay out our initial estimates of what budgets will be going into the various areas and what we have been informed were the spending plans of PPARC and CCLRC. In the first year, at least, spending across the Council will reflect what the plans were for the two Councils. By the time we get to next year we will have developed a science strategy and then you may start to see some moves in our spending plans.

Professor Howarth. And the level as well as the ratios are being preserved?

Professor Wade. Well, you preserve the ratios and you hope that the overall level goes up and you hope that the Government will be very generous in funding the new Council, but we won't know about that for a few months yet.

Dr. R. Walsh. I wonder if you would expand a bit further on the peer-review aspect? Is that going to be radically changed from the previous PPARC structure; for instance, whether to award a rolling grant or a standard grant and how that process has changed? I would like the panel to respond to that as well. I was on one of the advisory sub-panels of the previous structure and whenever you received the standard grant at least 12 – 15 people would have reviewed that case and you knew that that was the best science, so I wondered how the structure would be set up and whether the process has been diluted in some respects?

Professor Wade. The aim is not to dilute that process. A huge amount of effort goes into peer review not only from the point of view of the Research Council but also that of the community. We need to make sure that effort is used effectively and efficiently. I think that the biggest change you are going to see in astronomy grants is that we are trying to recognize that the purpose of a rolling grant is somewhat different to that of a standard grant; therefore we will move away from a situation where we are trying to compare two dissimilar things and use a system where all of a panel will look at all of the rolling grants; we will have a large sub-panel which will deal particularly with standard grants because it has to have experts in all the areas in which we might receive standard grant proposals. The aim is definitely to maintain an excellent peer-review process but to tailor it to the different kinds of grants which we use in astronomy.

The President. I think that all of the panel should respond to this particular point.

Professor Edmunds. There is certainly a perception that rolling grants have become monolithic and part of the impetus of the new structure is to get away from that. By getting all the rolling grants to be considered by one committee it should allow much better tensioning so that they compare them with each other. Some will win and some will lose and one hopes that that will be on a more equitable and transparent basis than in the past. I believe that to the Council and the executive scientific excellence remains the major basis on which grants are judged, but avoiding over-refereeing of proposals. Also, it is not clear yet how far we are driven by strategy in the award of grants within the new system. There is need for input from the community on that as well as from STFC itself. We want to make sure that the money is used efficiently — that we really do play a major rôle in world astronomy, and that requires being efficient at having a strategy that is good and does work; but that requires cooperation between us and the community. I think this needs more development.

Professor Bromage. I believe there are some great advantages, but also some potential dangers, in the changes that are now taking place. I will try and reflect the view of the SCAP professors as well as my own views. The first point is that scientific excellence must be paramount. If the community perceives that that is not going to be true then something needs to be done about it. The balance between the rolling grant and the standard grant — how the money will be divided, and whether it will be ring-fenced — is a very important issue. Of course, at the moment the community is not clear about how that will take place and therefore those with rolling grants will be worried that it will go one way and those with standard grants will worry that it will go the other way; that's just human nature. I understand the rationale in what is being done. One way of looking at this is that quite a lot of the rolling grants are hybrids in the sense that there are rolling grants that are based on facilities or instrument groups, but there are also rolling grants that have been consolidations of standard grants and the way in the past in which standard grants have been judged is by the same panel as for individual postdocs. Making up those standard grants was going to be one of the advantages of the system, so you can understand the community's feeling about the danger on the horizon that scientific excellence won't necessarily be seen to be paramount in the new process.

Professor Gear. This balance between rolling grants and standard grants and the whole peer-review process in certainly something that PPARC's Science Committee was wrestling with for a couple of years, long before the merger took place, for a number of reasons. One is that there was a recognition that on the rolling grants, for example, there was a huge amount of work done in both preparing and peer-reviewing them and the consequence of this is that the vast majority of them come out at the same level (of funding) that they were the last time around. There was concern that this was not an efficient use of this system and this resource; also, in terms of strategic priorities, scientific excellence, of course, has to be the most important element of peer review. When you have grants that are more or less of equal merit it is entirely appropriate to take strategic priorities into account when making a decision. In the PPARC Science Committee that element had not been taken into account properly. That is a not a criticism of the panel, it's just the nature of the system. To give an extreme example, it makes no sense to invest 20 million pounds in building an instrument for a satellite and not exploit it scientifically. Neither does it make any sense to fund a rubbish proposal just because we have already invested 20 million pounds. What it means is that the investment must be taken into account, along with the exploitation of subscriptions. There was a lot of debate at the Science Committee and the results that came out reflect the strong views of the executive as well as the disparate views of the committee members.

Professor Priest. Two points. Firstly, in rolling grants, flexibility is very important and this is stressed in the current guidelines. I felt that with rolling grants, assessed every two years as a series of standard grants, there was a lot of micro-management going on. But now with the move to have five-year rolling grants, renewed every three years and the stress on flexibility, I hope, it won't feel like someone is looking over our shoulder to see if we are keeping this or that postdoc going for 18 months or two years. I hope we will have genuine flexibility to take account of new opportunities as they come along, as they are bound to. The second point is that one of the great benefits of the old system was in-depth peer review and we must keep this in the new Council so that we can all be confident that our applications are being reviewed by people who know the subject. In particular the new grants panel should have the same number of members as the old panel. There was a whisper that the new grants panel was much smaller than the old, and many of us were not happy about that. The RAS, for example, wrote in to Richard Wade and Keith Mason about it. It sounds as though they are responding to that and it's likely the grants panel will be much larger than they were planning at one time. It is essential to have the confidence of the community.

Professor Bromage. Just one more proposal that members of SCAP would like to see and that is that responsive-mode grants are more responsive — one small change might be to have a six-month cycle which is more closely allied to telescope-time cycles than a one-year cycle, and I do hope that change is made.

Dr. Clare Parnell. I would just like to know how the STFC is going to encourage the next generation of leaders in their fields. We have advanced fellowships at the moment, but in general the average age of those who get them is much older than in EPSRC, and once you have an advanced fellowship you seem to be precluded from applying for a standard grant or having a postdoc of your own. In EPSRC you are encouraged and there is actually a standard-grants round for new lecturers or those who have advanced fellowships and they have an easier ride than the main grants panel. I just wondered what the new Council was going to do to encourage the new generation of leaders.

Professor Wade. I think we are going to think about the comments that you have made and this new-lecturer scheme in EPSRC. I don't necessarily like the sound of grants getting an easier ride. That seems to me to go against the idea of excellence. I agree there needs to be an opportunity for young people to get research funding and to get involved in the whole life of the Research Council. I think that through the peer-review process you will see that we have a mixture of the old-and-grey generation and the younger generation and I think that is particularly important as well. We'll take on board your comments about new leaders but it is different in different areas and I can assure you that some of your colleagues in particle physics look with a great deal of envy at astronomy and the youth of some of the project leaders in astronomy. It's easier in some areas of science if you are leading a small team in a lab rather than running a major international project.

Dr. Parnell. If it comes down to a choice between two projects of equal scientific merit would you take the known name or the unknown name?

Professor Wade. Being the well-known name can be very much a mixed blessing. If you are an unknown name with a brilliant idea, I would say you are more likely to get funded than someone with a well-known name but poor history of delivery.

Professor B. Gibson. Just following up on the theme of the next generation in relation to STFC studentships against fellowships — currently the latter are open to anyone in the world. Given that, is there any reason not to extend the same philosophy to the studentships, if not to the world then at least to the EU, to allow them to come in, relocate, and train here?

The President. The EU students can apply, can't they?

Professor Wade. Not at the moment. There's been a lot of discussion about the possibility of them doing that. I'm not sure that's driven by the altruism of the UK in wanting to educate the world rather than a shortage of students in some areas. Certainly in the case of PPARC we have drastically increased the number of studentships and not yet run out of quality, although some people tell me that we are close to that. My aspiration in the new Council is to continue to increase the number of students in the PPAN area until we are limited just by the quality of people we can bring in. It is one of our very best strengths that we attract the very best students and we should play to our strengths.

Professor Gibson. Have we run out of quality for UK-based people for the fellowships then?

Professor Wade. No. With the fellowships it is a different issue. I think what is happening with the fellowships is that you are attracting people that you hope will stay and add to the life of UK plc. Studentships may be a similar story but perhaps less pronounced than that.

Professor Gear. I'm not sure if this is true; perhaps Jim Hough can tell us, but in my time on the Education and Training Committee we pressed quite hard on this and the response that came back was that within the EU PhD studentships were regarded as training and education. If you wanted to have overseas students funded by the UK they would have to pay tax.

Professor J. Hough. There is quite a lot of pressure on UK studentships. EU students can come now their fees are paid. I believe that it was the Treasury that did not like this idea but I think the discussion is now closed and in fact it is something that can be taken up again by the new Council over the next period. Of course, you can also apply for project studentships on STFC grants and this can be used to pay stipends to people from the EU without them having to pay tax on that. It's not so easy to pay for people and students from the US unless you are willing to come up with the extra overseas fees that are required. The situation is improving and will be better over the next 18 months or so.

Dr. Anita Richards. Following on from that, I hope that what Eric said about giving a postdoc 18 months or two years is not going to be the typical length of a postdoc because one of the other things which we discussed when I was on RAS Council was how to improve career prospects immediately after you finished your PhD. I accept that you should be prepared to be flexible. One of the things that has been suggested, for instance, is that your pension rights, and your continuity of employment should follow you from establishment to establishment, so that you can get a mortgage, for example, because it is very hard to get a mortgage with a student loan and an eighteen-month contract. We lose a lot of good people from astronomy. It's hard to get PPARC fellowships if you started as a PDRA doing an instrumental-support project, for instance, where you don't have three 'killer' personal papers but have your name on many multi-authored papers. Such people can feel that their careers are rapidly dying away in astronomy.

Professor Wade. Whilst I agree with what you say, I'll risk countering it by repeating something I said last year which was very unpopular. The reason we train students is not solely to provide a supply of future astronomers and academics. It is to provide trained people who leave the subject and make a contribution elsewhere. If you look at the reasons why the Government funds particle physics and astronomy in particular, that's one of the strong cards that we have to keep playing; inherent in the idea that we are trying to increase the number of students we train is the idea that more students will *not* be taking up postdoc positions but will be moving out to do other things. I think we should regard this as a triumph rather than a failure of the system.

Dr. Richards. If you want to stay in astronomy I don't think you should have to gamble with your family's livelihood.

Professor Wade. We all have to make life choices and I think if a student chooses to do a PhD in particle physics or astronomy then that is a fantastic choice but it does not necessarily set a course for their future career.

The President. I'd like to take advantage of being the Chair to comment on that. Over the years it seems to me that embarking on a PhD in astronomy has become a little bit less of a life gamble. When I took on a research student as a young lecturer, I felt very guilty that I was probably making them unemployable if they did not make it in astronomy. Nowadays I am conscious that there are people around whose sole job it is to try and recruit our students and postdocs into the City and other areas because it is perceived that the kind of training that they receive is of high value in terms of skill and expertise. It is my feeling that there has been a change in the employment prospects of those who come into astronomy. I don't know if people here would agree with that?

Dr. Richards. We should be keeping the best people in astronomy, not just the ones that are most foolhardy. [Laughter.]

Professor Edmunds. Can I react very strongly against that? I don't think it should just be the best people staying in astronomy. We must try and get away from the idea that not pursuing an astronomical career after doing an astronomical PhD is in some way second-rate. The most valuable contributions could come outside the field from brilliant people, and that will increasingly become true as science becomes more interdisciplinary.

Professor J. Zarnecki. Much of the earlier discussion was, I think rightly, on the balance of strategy and scientific excellence, which will be the area of subtle changes in the new Council. I would like to broach something that Richard touched on, and that's the business of the campuses. I think it is fair to say that in our area of astronomy and space these haven't had a big impact yet but the potential is there and my understanding is that there are significant budgets that might be made available to develop particularly the Harwell and Chilton campus. I feel fairly positive about it and in the area of developing instruments and technology there are benefits to be had by a greater interaction between academics and industry and with the central-facilities people. I know, by talking to colleagues, that there are some people who feel slightly threatened in that there are some universities which have developed specialized techniques and feel that these may become centralized or redundant.

Professor Wade. The announcement that was made to create these campuses was a real change in Government policy, in the sense that previously they viewed Daresbury and RAL as institutes that competed across the board to host facilities, and the change is to focus on these locations for the provision of new facilities but not to the exclusion of other sites. The reason is that the Government wants to put world-class facilities on UK soil and we should rejoice because we have a Government with that aspiration. Inevitably there will be winners and losers. Whilst some universities wish to host these facilities, Government policy is tending to push them towards the new campuses, for the UK as a whole, and I see this as a gain. People will see the inward investment and scientific focus on the space area at the Harwell site, for instance, which could not be contemplated if there was an argument about where the facilities are to go.

Professor Gear. This was moved up the order of priority of things in the knowledge-transfer agenda. It did concern me because when I look at engineering in my own university they always seem to chase the money rather than stick to their core activity, and I don't want this to happen in the astronomy area. In astronomy and particle physics, many people have transferable skills but don't have the expertise to do it. In my own group we gain significant monies from the transfer of technology so provided people do not lose sight of the primary goal — which is excellent science — then I think there can be a benefit.

Professor Jocelyn Bell-Burnell. I want to go back to Anita's point. When she spoke I was not totally in favour of what she was saying, but having seen the way the panel handled it I am now 110% behind Anita. We have an all-male panel who say that they want to raise the number of women around. The panel perhaps needs to remember that women are more risk averse. The men who have power need to stop assuming that one pattern, *i.e.*, the male pattern, fits all and need to be aware of other patterns of life, other approaches, and other points of view. Instantly, please. [Applause.]

Professor Wade. Can I ask Jocelyn what actions she proposes we should take?

Professor Bell-Burnell. A number of things can be proposed which would actually benefit postdocs as well as females. Better management in universities, better career advice, better mentoring. Transferability of pensions and opportunities that do not require mobility. Allow the funding to attach to the person, so that if your spouse moves, you and your family can move.

Professor Priest. I have one very simple suggestion here. The Royal Society has an excellent scheme called the Dorothy Hodgkin Fellowships which allow the holder a huge amount of flexibility. You can take it where you like, you can have a gap in it or have some of it part-time. Clearly this would be of great benefit to women when they start having children. I see no reason why these qualities could not be attached to the normal fellowship scheme.

Professor Wade. In reply to Jocelyn, whilst I think I agree with everything you are saying, much of the control lies within the universities, particularly in terms of employment conditions, *etc.* If you have a standard grant you should be able to take funding with you. I worked closely with a number of university administrators when we were looking at the consequences of changes to the regulations on employment. I can tell you that it is very difficult for the Research Councils to influence the way that the universities choose to employ people and we can only continue to talk to the universities about this particular area.

Professor Bell-Burnell. And Research Councils UK (RCUK) could put pressure on universities?

Professor Wade. I wouldn't stake my career on the effectiveness of that particular group. The Research Councils have individual relationships with universities to try and deal with them about this. There is only so much we can do in the Council.

Professor Priest. What about my suggestion of adopting the Dorothy Hodgkin scheme for advanced fellows and postdocs? It would be simple to do; it would make a huge difference.

Professor Edmunds. Is there a problem at the moment about an advanced fellowship or a post-doctoral fellowship?

Professor Priest. You can't have a post-doctoral fellowship and have it for six years at 50%, say, instead of having it for three years.

Professor Wade. You should ring up people in the Research Councils. You'll find us much more flexible than you might imagine.

Professor Priest. But if it's written into the rules then it makes it seem that you are really genuinely willing to do as much as you possibly can for women.

Professor Elsworth. It is in the rules. It's one of the things that we fought for: that it should be seen not as a privilege but a right. Very few people take up the flexibility but I think that the flexibility is actually there and official.

Professor Wade. If it is not written into the rules then the flexibility exists because that *is* our rule.

Dr. Parnell. As an Advanced Fellow I was told that I couldn't move except on scientific grounds, not that my husband was working elsewhere, for example.

Professor Wade. Drop me an e-mail with the details.

Dr. G. del Zanna. I can confirm that the rules are very strict so I welcome the view by Eric Priest about the flexibility. That will help research in general and scientific output.

Professor Wade. I strongly believe that flexibility exists and would encourage you to talk to people in the office. Sometimes what you see as inflexibility is because there is something behind it preventing us from being flexible. I can assure you that our intention is not slavishly to follow rules.

Professor Priest. The perception is that the flexibility is not there, so you need to work on that.

The President. We don't have another half hour to discuss this topic. It is clear that clarification needs to be made.

Dr. D. Ward-Thompson. Change of subject. I know a lot of people were worried when the name of the new Council did not incorporate the word 'research', and when you gave your presentation, nowhere was it clear in your committee structure where the decision is made between the fraction of funding for facilities and the fraction of funding for what used to be called the grants line. Perhaps Richard could clarify that one for us please?

Professor Wade. First of all, I think it was bit of a triumph to get 'Science' included in the name [laughter]. We tried to get 'Research' into the name as well but ended up with Technology at the last moment, and that was at the request of David Sainsbury. The charter does say a lot about science research. I think that the balance you are talking about — particularly in the PPAN sphere, the choice between investment in exploitation *versus* new investment — will start at the PPAN level. The big decisions about the distribution of funding of facilities between the PPAN side and PALS side will take place at the Science Board and there will be some lively debates. There is no sense that because we have 'Facilities' in the title it means there is going to be an immediate move away from research or exploitation. It simply doesn't make sense to build new facilities which you cannot exploit.

The President. I think that you might be amused to hear that in the Order placed in Parliament to establish the STFC, it says 'Within the meaning of the Act, STFC is a Research Council'. There are still lots of hands up, some of whom have spoken and others who have not but we are due to finish now. I'd like to thank you all for your contributions to this discussion. A lot of interesting points have come out and I'd like to thank Richard Wade for coming here to answer our questions and also the Panel for their views. Thank you all. [Applause.]

THE RELATION BETWEEN CN AND O–NA–AL INHOMOGENEITIES AMONG RED GIANTS IN NGC 6752

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Data are drawn from the literature to investigate the systematics of oxygen, sodium, and aluminium abundances among red giants of different CN-band strengths in the globular cluster NGC 6752. The CN-strong giants have higher sodium and aluminium, but lower oxygen, abundances than those with weak CN bands. Compared to CN-weak giants, those with strong CN bands are composed of material that has been more extensively processed through the CNO, Ne–Na, and Mg–Al proton-addition cycles. These enrichment patterns are typical of CN-strong stars in other intermediate-metallicity globular clusters such as M13 and M5.

Introduction

The earliest recognition of abundance inhomogeneities within globular clusters came through studies of molecular CN and CH bands[1,2]. Subsequent investigations of both λ3883 and λ4215 CN bands showed that many globular clusters contain a population of CN-enhanced stars[3,4]. One of the first clusters in which a bimodal CN-band-strength distribution was found among the red-giant stars is NGC 6752[5]. With the application of high-resolution spectroscopy it became clear that the CN inhomogeneities are closely linked to variations in the abundances of the elements O, Na, Mg, and Al[6,7]. In clusters such as M13 and M5 it has been found that CN-strong red giants are enhanced in Na and Al abundance, but depleted in O, relative to CN-weak stars of similar effective temperature and gravity[8–10]. A number of high-resolution spectroscopic studies of NGC 6752[11–17] have shown that the red giants in this cluster exhibit large dispersions in the oxygen, sodium, and aluminium abundances, with anticorrelations between [O/Fe] and both [Na/Fe] and [Al/Fe], of which the O–Na anticorrelation has been particularly well documented. In this paper we use data from those studies to confirm that the red giants in NGC 6752, like those in M13, also exhibit a CN–O anticorrelation as well as CN–Na and CN–Al correlations. The CN–Na–Al trends were first shown for much smaller numbers of red giants in NGC 6752 by Norris *et al.*[5] and Cottrell & Da Costa[18].

Compiled data

We searched for oxygen, sodium, and aluminium abundance data from the literature for those red giants in NGC 6752 that were observed by Norris *et al.*[5] as part of a survey of λ3883 CN-band strengths. The stars for which combined CN and element-abundance information were obtained are listed in Table I. The stellar designations listed in Column 1 are those adopted by Norris *et al.*[5], with the prefixes CS and A referring to the colour–magnitude-diagram studies of Cannon & Stobie[19] and Alcaino[20]. The prefix CL refers to a photographic survey by Lee & Cannon[21,22] that was unpublished at the time of the Norris *et al.*[5] work;

TABLE I

Data for NGC 6752 stars

Star[a]	*V*	*B–V*	*δS(3839)*	*Type*[b]	*Other*[c]	*[Na/Fe]*	*[Na/Fe] Ref*[d]	*[Al/Fe]*	*[Al/Fe] Ref*[d]	*[O/Fe]*	*[O/Fe] Ref*[d]
1	2	3	4	5	6	7	8	9	10	11	12
CS 3	11·50	1·25	0·34	RGB,S	Y-mg8	0·36	1,4	0·77	1,4	0·39	1,4
CS 105	13·76	0·83	0·52	RGB,S	B4446, Y-16	0·36	4	0·83	4	0·09	4
CS 106	13·60	0·86	0·46	RGB,S	M37091	0·47	5			−0·01	5
CS 107	12·34	1·09	0·14	RGB,W	M37221	0·09	5			0·65	5
CS 111	12·78	0·90	0·42	RGB,S	B4437, M38412, CL139	0·43	5	<0·28	3	0·42	5
CS 114	13·61	0·83	−0·01	RGB,W	M29686	−0·17	5			0·58	5
CS 115	13·55	0·87	0·37	RGB,S	M30409	0·30	5			0·60	5
CS 118	12·18	1·07	0·45	RGB,S	B1095, M30433	0·37	5	1·21	3	−0·39	5
CS 119	13·00	0·93	0·44	RGB,S	M30272	0·40	5				
CS 124	13·22	0·86	0·47	RGB,S	M2162	0·50	5			0·18	5
CS 125	12·16	1·08	0·47	RGB,S	M2097	0·77	5			0·07	5
CS 128	13·73	0·85	0·50	RGB,S	M1797	0·35	5			0·09	5
CS 135	11·44	1·15	0·16	AGB,W	B277			0·65	3		
CS 137	14·26	0·80	0·44	RGB,S	M6840	0·37	5			0·22	5
A3	12·02	1·09	0·06	RGB,W	B3011, M9756	0·04	5	−0·02	3	0·67	5
A8	12·03	1·12	0·48	RGB,S	B2580, M7627 Y-mg22	0·70	4,5	0·99	4	0·08	4,5
A9	11·03	1·20	−0·03	AGB,W	B2403			<0·03	3		
A12	11·25	1·35	0·12	RGB,W	B2113, Y-mg6	0·13	4	0·53	3,4	0·60	4
A29	11·85	1·14	0·48	RGB,S	B1518, Y-mg18	0·17	1,2,4	0·57	1,3,4	0·47	1,2,4
A30	12·18	1·06	0·07	RGB,W	B1285, M11189, Y-mg24	0·03	4,5	0·12	4	0·65	4
A31	10·80	1·60	0·05	RGB,W	B1630, Y-mg1	0·38	4	0·82	4	0·46	4
A33	12·28	1·02	0·37	RGB,S	M14348	0·47	5			0·10	5
A36	11·59	1·16	0·06	RGB,W		0·21	2			0·69	2
A45	11·57	1·23	0·34	RGB,S	B2575, Y-mg10	0·27	1,4	0·81	1,4	0·47	1,4
A46	12·84	0·84	0·01	AGB,W	M25726	0·01	5			0·76	5
A48	13·05	0·94	0·37	RGB,S	B2482, M27778	0·37	5			0·51	5
A59	10·90	1·59	0·12	RGB,W	B3589, Y-mg2	0·19	4	0·78	3,4	0·55	4
A68	12·02	1·11	0·57	RGB,S	B3805, Y-mg21	0·57	4	1·19	3,4	0·01	4
CL1015	11·88	1·17	0·15	RGB,W	M48975	−0·06	1,5			0·55	1,5
CL1066	11·53	1·22	0·04	RGB,W		−0·19	1			0·56	1
CL1089	11·08	1·33	0·02	RGB,W		−0·03	1			0·67	1

[a] Designation used by Norris *et al.*[5].
[b] W and S denote CN-weak and CN-strong stars, respectively.
[c] Other designations: B = Buonanno *et al.*[27], M = Momany *et al.*[28], Y = Yong *et al.*[15].
[d] Sources of abundances: 1. Norris & Da Costa[11]; 2. Minniti *et al.*[12]; 3. Cavallo *et al.*[14]; 4. Yong *et al.*[15]; 5. Carretta *et al.*[17].

coordinates for the three CL stars in Table I are given by Norris, Da Costa & Tingay[23]. The V and $(B-V)$ values listed in Table I are those cited by Norris *et al.*[5], who in turn took them from the same references as the star designations.

Norris *et al.*[5] introduced the index $S(3839)$ to quantify the strength of the λ3883 CN band in the spectra of NGC 6752 red giants. It measures the flux emitted by a star in the wavelength range of the CN band (λλ3846–3883) relative to that in a nearby comparison region (λλ3883–3916). The CN-excess index $\delta S(3839) = S(3839) - S_0(3839)$ is listed in Table I, where the equation $S_0(3839) = -0{\cdot}146\,V + 1{\cdot}832$ represents an empirical baseline to the location of CN-weak giants in a plot of $S(3839)$ *versus* V magnitude. In effect, $\delta S(3839)$ measures the CN-band strength of a red giant relative to that of a CN-weak star of the same V magnitude.

Column 5 of Table I lists the classification of each star as either red-giant branch (RGB) or asymptotic giant branch (AGB), and either CN-strong (S) or CN-weak (W) according to Norris *et al.*[5]. Their work remains the largest survey of CN-band strengths among evolved giants in NGC 6752. Grundahl *et al.*[24] measured λ3590 CN and λ3360 NH indices for 21 RGB stars in the cluster but did not give the individual values in their paper, so their data cannot be incorporated into the current search. Their Figs. 1 and 2 show that the NH band strength correlates with both CN band strength and [Al/Fe] abundance.

Information on λ3883 CN band strengths of NGC 6752 red giants can also be obtained from Bell, Hesser & Cannon[25]. Their m_{CN} index measurements show some scatter with respect to $S(3839)$, but there is a clear correlation (Fig. 1). A plot of all m_{CN} measurements from Bell, Hesser & Cannon[25] *versus* absolute magnitude M_V is shown in Fig. 2. Their work added no new stars in the magnitude range covered by Norris *et al.*[5] ($V < 14^{m}{\cdot}5$) but does extend to fainter magnitudes. Filled and open circles refer to CN-strong and CN-weak stars according to Norris *et al.*[5], and it can be seen that the CN-strong giants do have larger m_{CN}

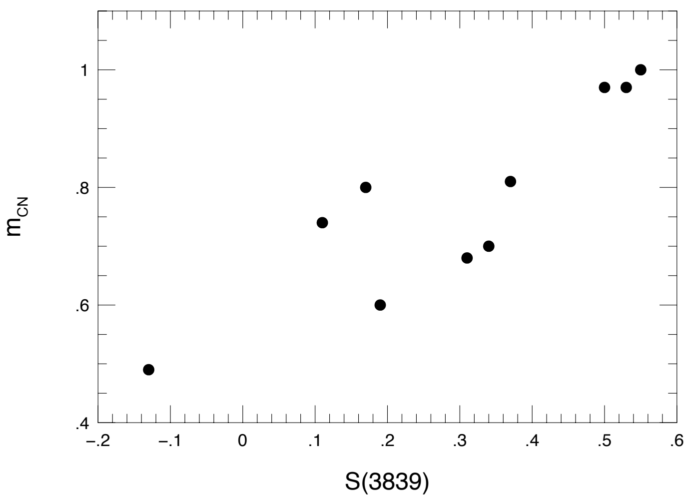

FIG. 1

The correlation between the CN index $S(3839)$ from Norris *et al.*[5] and m_{CN} from Bell, Hesser & Cannon[25] for red giants in NGC 6752.

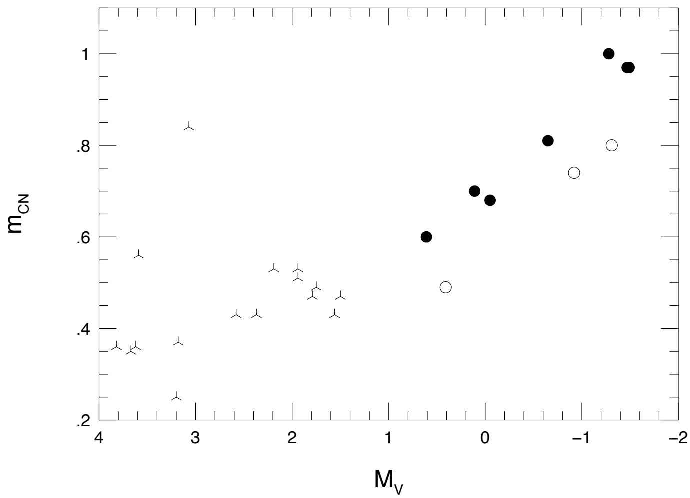

FIG. 2

The m_{CN} index values from Bell, Hesser & Cannon[25] *versus* absolute magnitude M_V for stars in NGC 6752. Stars identified as being CN-strong or CN-weak[5] are depicted as filled and open circles, respectively. Stars fainter than the sample of Norris *et al.*[5] are depicted as 'arrows'.

values than CN-weak stars of comparable magnitude, although the spread in m_{CN} (at a given M_V) is much smaller than in $S(3839)$. The data in Fig. 2 suggest that the occurrence of CN-strong stars continues to magnitudes fainter than those covered by Norris *et al.*[5]. The range in m_{CN} among stars with $+1^m{\cdot}5 < M_V < 2^m{\cdot}0$ is $\sim 0{\cdot}1$, whereas the standard deviation that Bell, Hesser & Cannon[25] give for their m_{CN} measurements is 0·025. The existence of CN inhomogeneities among stars on the main sequence of NGC 6752 was subsequently confirmed by Suntzeff & Smith[26].

The DDO colour $C(41-42)$ given by Norris *et al.*[5] for a subset of their sample contains potential information about the λ4215 CN band, which at the metallicity of NGC 6752 ([Fe/H] $= -1{\cdot}5$ dex[16]) is typically much weaker than the λ3883 feature. A plot of this colour *versus* $(B-V)$ is shown in Fig. 3, with filled and open circles again denoting red giants with strong and weak λ3883 CN absorption, respectively. Comparisons are restricted by the fact that only two of the RGB stars plotted are CN-weak; however, both appear to have bluer $C(41-42)$ colours than CN-strong giants of similar $(B-V)$. This suggests that the λ4215 CN band does exhibit an abundance-induced variation among the NGC 6752 stars.

The sources for O, Na, and Al abundance data to which we have turned are Norris & Da Costa[11], Minniti *et al.*[12], Cavallo, Suntzeff & Pilachowski[14], Yong *et al.*[15], and Carretta *et al.*[17]. Prior to combining the data from these works it is necessary to test for systematic differences between the abundance scales of each paper. Comparison of abundances between these studies is somewhat hindered by the use of different star designations by these authors. Yong *et al.*[15] use the numbering system of Buonanno *et al.*[27], while Carretta *et al.*[17] use that of Momany *et al.*[28]. Fortunately both studies list the right ascension and declination

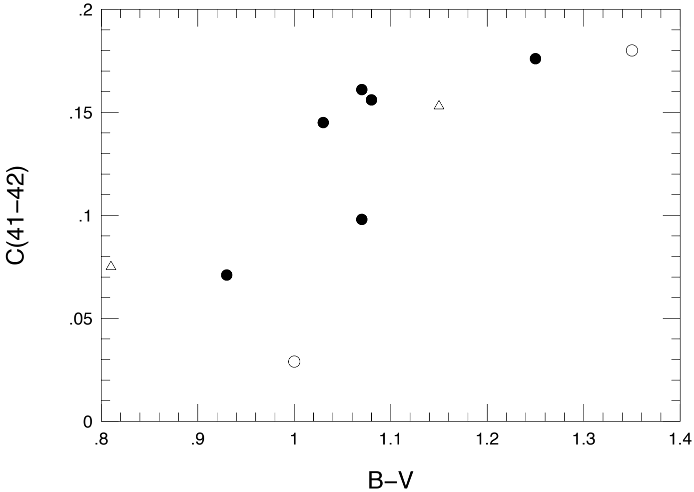

FIG. 3

The DDO colour $C(41–42)$ from Norris *et al.*[5] *versus* $(B-V)$. Red-giant-branch stars identified as being CN-strong or CN-weak on the basis of the 3883 Å absorption band[5] are depicted as filled and open circles, respectively. Asymptotic-giant-branch stars are shown as triangles.

of their programme stars, allowing them to be cross-referenced. Coordinates for stars in the work of Norris & Da Costa[11], Minniti *et al.*[12], and Cavallo, Suntzeff & Pilachowski[14] were obtained from the *Simbad*[29] database, or by overlaying the *2MASS All-Sky Catalog of Point Sources*[30] on digitized sky images of NGC 6752 inspected through the *Aladin*[31] interactive sky atlas.

We have referenced the various abundance studies of red giants in NGC 6752 to that of Yong *et al.*[15]. The comparisons are summarized in Table II. For each star in common we calculated the abundance difference Δ[X/Fe](Yong *et al.* – other source) for the elements X = O, Na, and Al. The mean and standard deviation (σ) in Δ[X/Fe] for each source comparison are listed in Table II, together with the number of stars *N* on which the comparison is based. There are only two stars in common between the Yong *et al.*[15] and Minniti *et al.*[12] programmes; in this case the value of σ given in Table II is simply the difference in the two resulting values of Δ[X/Fe].

The offsets listed in Table II between the various O–Na–Al abundance sources were used to transform all abundances onto the system of Yong *et al.*[15]. The resulting homogenized abundances for red giants from the CN survey of Norris *et al.*[5] are listed in Table I, together with references to the sources for each star. Where transformed values of [O/Fe], [Na/Fe], or [Al/Fe] were available from more than one source for a particular star, the tabulated values are unweighted averages.

A plot of the homogenized [O/Fe] abundances *versus* CN-excess index $\delta S(3839)$ (Table I) is shown in Fig. 4. There is a clear anticorrelation. Filled and open circles represent CN-strong and CN-weak red giants[5], while the triangle depicts an AGB star. In NGC 6752 the red giants having large values of $\delta S(3839)$ are enhanced in the abundance of nitrogen[5,18,24]. The relationship seen in Fig. 4 therefore reflects an anticorrelation between the N and O abundances of the

TABLE II

Abundance comparisons

Abundance Difference[a]	*Mean Δ*	*σ*	*N*
Δ(Y05–Car07) [O/Fe]	0·08	0·12	7
Δ(Y05–ND95) [O/Fe]	0·28	0·05	3
Δ(Y05–Min96) [O/Fe]	0·35	0·09	2
Δ(Y05–Car07) [Na/Fe]	−0·07	0·12	14
Δ(Y05–ND95) [Na/Fe]	−0·03	0·04	3
Δ(Y05–Min96) [Na/Fe]	0·14	0·08	2
Δ(Y05–Cav04) [Al/Fe]	0·23	0·05	4
Δ(Y05–ND95) [Al/Fe]	0·40	0·10	3

[a] Abundance sources:
ND95: Norris & Da Costa[11].
Min96: Minniti *et al.*[12].
Cav04: Cavallo *et al.*[14].
Y05: Yong *et al.*[15].
Car07: Carretta *et al.*[17].

NGC 6752 red giants. This in turn indicates that the atmospheres of the CN-strong giants contain material that has been processed through the ON-cycle of hydrogen burning.

The CN-weak giants in NGC 6752 have oxygen abundances of [O/Fe] ~ +0·6 dex on the scale of Yong *et al.*[15]. The oxygen-abundance scale of these authors is higher than those of the other sources used here; although it is only 0·08 dex higher than that of Carretta *et al.*[17], it is ~ 0·3 dex higher than both Norris & Da Costa[11] and Minniti *et al.*[12]. Using the later abundance scales, the oxygen abun-

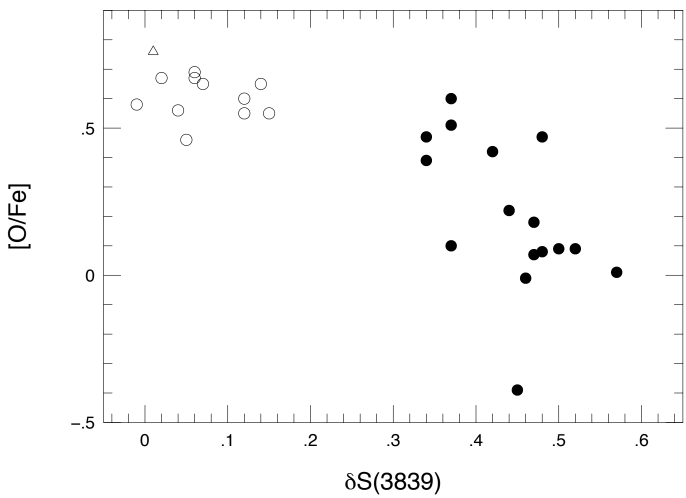

FIG. 4

The oxygen abundance *versus* CN-excess index δ*S*(3839) for stars in NGC 6752. Filled and open circles refer to CN-strong and CN-weak RGB stars, respectively. The triangle depicts an AGB star.

dance in the CN-weak giants would be [O/Fe] $\sim$ $+0 \cdot 3$ dex. These uncertainties of zero-point notwithstanding, with such high oxygen abundances it seems reasonable to assume that the CN-weak giants in NGC 6752 have not depleted their surfaces through the dredge-up of O→N-processed material from the region of the hydrogen-burning shell.

The range in [O/Fe] among the CN-weak red giants is quite small at $0 \cdot 2$ dex. However, among the CN-strong giants there is a spread of at least $0 \cdot 6$ dex, with most of these stars falling in the range $-0 \cdot 5 \leqslant$ [O/Fe] $\leqslant +0 \cdot 6$ dex. There is an overlap between the oxygen-abundance distributions of the CN-strong and CN-weak giants, with several of the former stars having [O/Fe] $= +0 \cdot 5$ to $+0 \cdot 6$ dex. The most oxygen-deficient star is a CN-strong red giant (CS 118) with [O/Fe] $= -0 \cdot 4$ dex, about $0 \cdot 4$ dex lower than any of the other CN-strong giants in the sample. This star may be similar in nature to the 'super oxygen-poor' red giants in M13 identified by Kraft *et al.*[32]. In M13 the majority of such stars have absolute magnitudes of $M_V < -2^{\mathrm{m}} \cdot 0$ (Sneden *et al.*[9]), however CS 118 is not as bright as this, having an absolute magnitude of $M_V = -1^{\mathrm{m}} \cdot 2$ (adopting an apparent distance modulus of $(m–M)_V = 13^{\mathrm{m}} \cdot 35$ following Newell & Sadler[33]).

The behaviour of CN band strength with homogenized [Na/Fe] and [Al/Fe] abundances are shown in Figs. 5 and 6 respectively. The filled and open circles again denote CN-strong and CN-weak RGB stars, with one AGB star being shown as a triangle. It is clear that the abundances of both Na and Al are higher in the CN-strong giants. These figures strengthen earlier demonstrations of this phenomenon in NGC 6752[5,18,24]. The correlation between CN band strength and sodium may appear more marked than with aluminium, but this could be due to the more restricted database of Al abundances. Grundahl *et al.*[24] show that in their sample of NGC 6752 giants the [Al/Fe] abundance correlates very closely with a λ3360 NH-band index. This is analogous to the δ*S*(3839)–[Al/Fe] correlation seen in Fig. 6.

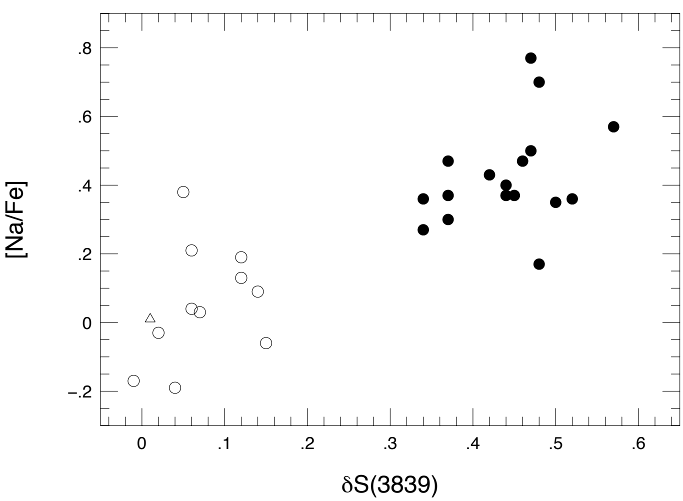

FIG. 5

The sodium abundance *versus* CN-excess index δ*S*(3839) for stars in NGC 6752. Filled and open circles refer to CN-strong and CN-weak RGB stars, respectively. The triangle depicts an AGB star.

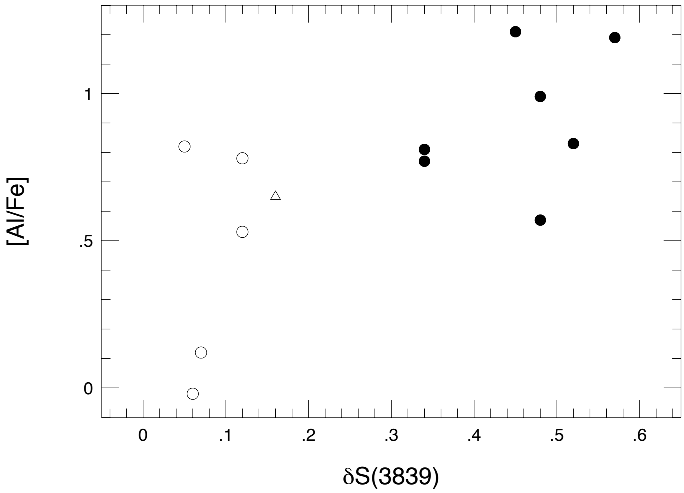

FIG. 6

The aluminium abundance *versus* CN-excess index $\delta S(3839)$ for stars in NGC 6752. Filled and open circles refer to CN-strong and CN-weak RGB stars, respectively. The triangle depicts an AGB star.

Discussion

As noted in the *Introduction*, the pattern of abundance inhomogenities within NGC 6752 as regards the elements O, Na, Mg, Al have been studied rather extensively[14–17,34], revealing the same type of O–Na and O–Al anticorrelations seen in a number of other globular clusters such as M13 and M5[8,35,36]. As with both of these other systems[8–10,37], the λ3883 CN band strength of red giants in NGC 6752 is anticorrelated with oxygen abundance and correlated with Na and Al abundances. Such results bespeak the continued utility of using CN bands as tracers of abundance inhomogeneities within globular clusters. The relationships between O–Na–Mg–Al abundances in NGC 6752, and what they imply about the nucleosynthesis mechanisms responsible for these element inhomogeneities in the cluster have been discussed in detail elsewhere[34,38–40]. Here we focus on the behaviour of carbon, nitrogen, and oxygen, since these elements affect the strength of the CN bands in NGC 6752 red giants.

The red-giant-branch stars of NGC 6752 exhibit one of the best examples of a bimodal CN distribution within a globular cluster. There is an anticorrelation between CN and λ4300 CH band strengths[5,24] that is the consequence of an anticorrelation of carbon and nitrogen abundances. The relative [C/Fe] and [N/Fe] abundances found for a small number of CN-strong and CN-weak RGB stars are[5,41]

$$[\mathrm{N/Fe}]_s = [\mathrm{N/Fe}]_w + 0\cdot8, \qquad (1)$$

and

$$[\mathrm{C/Fe}]_s = [\mathrm{C/Fe}]_w - 0\cdot3. \qquad (2)$$

The absolute C and N abundances of the cluster giants are more uncertain, however, with quite a diversity among analyses in the literature. As an example, Suntzeff & Smith[26] derived a carbon abundance of [C/Fe] = −1·05 dex for star A8 whereas Norris *et al.*[5] obtained −0·4 dex. Similarly, for star A31 the value of Suntzeff & Smith[26] (−1·05 dex) is 0·5 dex less than that obtained by Bell & Dickens[42] (−0·5 dex). Similar differences exist between these authors for stars CS 125 and CS 126. Thus, as with the oxygen-abundance scales discussed above, the carbon abundances from available studies of NGC 6752 can differ in zero point by 0·3 dex or more.

An additional constraint on the abundances can be obtained if it is assumed that the total C+N+O abundance of the CN-strong cluster giants is equal to that of the CN-weak stars. (Evidence for and against this assumption can be found, for example, in Brown, Wallerstein & Oke[43] and Carretta *et al.*[44].) This equality would apply if the composition of a CN-strong giant has been CNO-processed relative to that of a CN-weak giant. We adopt relations (1) and (2) and take the oxygen-abundance difference between CN-strong and CN-weak giants to be

$$[O/Fe]_s = [O/Fe]_w - 0{\cdot}6 \tag{3}$$

on the basis of Fig. 4. Requiring equality of the C+N+O abundance between the CN-strong and CN-weak RGB stars, with a solar C:N:O ratio of 4:1:8, then gives a relationship between the C, N, and O abundances of the CN-weak giants:

$$10^{[N/Fe]_w} = 0{\cdot}38 \times 10^{[C/Fe]_w} + 1{\cdot}13 \times 10^{[O/Fe]_w}. \tag{4}$$

If carbon in the CN-weak red giants is depleted by deep mixing but oxygen is not, such that the oxygen term dominates the right-hand side of this equation, this leads to $[N/Fe]_w \sim 0{\cdot}05 + [O/Fe]_w$. Such an abundance would imply that nitrogen within the NGC 6752 CN-weak giants is behaving as a primary rather than a secondary element.

If $[O/Fe]_w = 0{\cdot}6$ dex as in Fig. 4, then the value of $[N/Fe]_w$ inferred from equation (4) is ~ 0·65 dex, with the nitrogen abundance of the CN-strong giants being $[N/Fe]_s \sim 1{\cdot}45$ dex (from equation 1). The analysis of Da Costa & Cottrell[41] for a pair of CN-strong and CN-weak red giants in NGC 6752 gives values of $[N/Fe]_s$ and $[N/Fe]_w$ that are 0·85 dex lower than these calculations would require. If instead we adopt the oxygen abundance scales of Norris & Da Costa[11] and Minniti *et al.*[12] such that $[O/Fe]_w \sim 0{\cdot}3$ dex, then equation (4) would require that $[N/Fe]_w \sim 0{\cdot}35$ with $[N/Fe]_s \sim 1{\cdot}15$ dex. This is still some 0·55 dex higher than the abundances found by Da Costa & Cottrell[41]. Such nitrogen abundances are, however, consistent with those derived for dwarf and subgiant stars in NGC 6752 by Carretta *et al.*[44].

Differences in CN band strength and carbon and nitrogen abundances have been found among main-sequence stars in NGC 6752[26,44], along with anticorrelated [Na/Fe] and [O/Fe] variations[13,44]. Thus at least some component of the abundance trends seen in Figs. 4–6 are likely to precede the RGB phase of evolution, and date back potentially to the very early history of the cluster. Cottrell & Da Costa[18] introduced a scenario in which the CN-strong stars represent a second stellar generation that formed within the cluster from stellar ejecta that was enriched in CNO-processed material[38,45-49]. The fact that the CN variations correlate with the elements Na and Al indicate that the ejecta from the prior generation of enriching stars must also have been processed through the Ne–Na and Al–Mg proton-addition cycles[50–52] that can also occur within a stellar-interior region where hydrogen burning is occurring *via* the CNO-bicycle. Theoretical scenarios for the primordial nucleosynthesis sites in which such processed material was produced have centred on intermediate-mass AGB stars[18,38–40,48,49], and more recently on rapidly-rotating higher-mass stars[40,48,53], possibly in the analogues of a Wolf-Rayet phase[54,55].

The discoveries of multiple stellar generations in the colour–magnitude diagrams of the globular clusters ω Cen[56] and NGC 2808[57], with possible associated differences in helium abundance[58–60], lend support to primordial enrichment scenarios in which the CN-strong stars formed as a chemically-enhanced population some time after the initial population of CN-weak stars.

The primordial enrichment of NGC 6752 may have occurred in several ways. The cluster ω Cen has received much debate as the possible former nucleus of a now-disrupted dwarf galaxy that merged with the Milky Way[47,61–63]. Enriched gas may have flowed into the nucleus from massive stars in surrounding regions of the dwarf system, allowing an extended epoch of progressively-enriched star formation to occur[49]. Perhaps NGC 6752 also formed as the nucleus of a dwarf galaxy[47], or at least at the core of an ultra-massive progenitor cloud of the type proposed by Searle & Zinn[64]. In the case of NGC 6752 the self-enrichment was largely limited to the products of the CNO-process and the Ne–Na and Mg–Al proton-addition cycles, with some possible *s*-process-element enhancements[15]. Unlike ω Cen, the enrichment didn't extend to the products of the Fe-peak, there being very little scatter in [Fe/H] among the red giants of NGC 6752[15,16].

Prantzos & Charbonnel[48] and Decressin *et al.*[53] have proposed that both the element enrichment of the CN-strong stars as well as their very formation could have been triggered by massive stars associated with the initial (CN-weak) stellar generation. This idea recalls the sequential-star-formation scenario discussed by Elmegreen & Lada[65] in the context of OB associations. It may have been that globular clusters formed from massive clouds within which multiple episodes of star formation occurred at adjacent sites. Massive stars at one site may have influenced the chemical evolution of star-forming regions at neighbouring locations. The starbursts from adjoining sites may subsequently have merged or coalesced to produce a massive, chemically-inhomogeneous globular cluster.

Although there is likely a primordial component to the abundance variations within NGC 6752, any initial inhomogeneities in the surface C, N, and possibly O abundances could be modified during the RGB phase of evolution by a process of deep mixing, as in M13[66,67]. The transport of material between the hydrogen-burning shell and the base of the convective envelope of a red giant can lead to changes in the surface abundances of the elements from carbon through aluminium[68]. The results of Suntzeff & Smith[26] and Carretta *et al.*[44] indicate that subgiants in NGC 6752 have lower carbon abundances than main-sequence stars, and that [C/Fe] declines with increasing luminosity on the upper red-giant branch. One has to be aware of possible systematic differences in the various carbon-abundance papers before deciding just how much of a change occurs in [C/Fe] between the main sequence and the tip of the RGB. A direct comparison of the dwarf abundances of Carretta *et al.*[44] with the red-giant abundances of Suntzeff & Smith[26] indicates more than a 1·0 dex difference in [C/Fe]. However, as noted above, the Suntzeff & Smith[26] [C/Fe] values are systematically lower than those of Norris *et al.*[5] and Bell & Dickens[42], and the change in [C/Fe] along the RGB might be smaller ($\sim$ 0·6 dex).

What would deep mixing do to a primordial spread in nitrogen abundance among the NGC 6752 stars? If the nitrogen abundances on the main sequence are initially very high, such as the values of [N/Fe] $> +1 \cdot 0$ dex measured by Carretta *et al.*[44], then RGB deep mixing may not lead to a significantly higher surface nitrogen. Among dwarf stars in NGC 6752 those authors find [C/Fe] = $0 \cdot 0 \pm 0 \cdot 2$ and [N/Fe] = $+1 \cdot 4 \pm 0 \cdot 2$ dex. These nitrogen abundances are considerably higher than found for the CN-strong red giant CL 166 by Da Costa & Cottrell[41], but are comparable to the values of $[N/Fe]_s$ inferred above if equation (4) is applicable. It is possible that many of the dwarf stars in the Carretta *et al.*[44] sample are main-sequence progenitors of the CN-strong RGB stars, and that [N/Fe] changes very little along the red-giant branch despite the occurrence of deep mixing and any consequent dredge-up of CN(O)-processed material.

The carbon and nitrogen abundance data could therefore be consistent with deep mixing on the red-giant branch of NGC 6752 being accompanied by a larger change in [C/Fe] than in [N/Fe]. Such behaviour has been shown for the RGB stars of the cluster M13 (see Fig. 2 of Langer & Kraft[69], which is based on the abundances of Suntzeff[70]). Such a situation can be obtained if material in the atmospheres of these stars has been subjected to such extensive prior CN(O) processing that the nitrogen has reached a high-abundance 'plateau' following which further conversion of carbon (or oxygen) produces only a small fractional increase in the nitrogen abundance. This circumstance was also discussed by Trefzger *et al.*[71] in the context of the clusters M92 and M15, within which nitrogen appears to behave in a similar manner.

Although the discussion above has assumed equal C+N+O abundances among the CN-strong and CN-weak giants in NGC 6752, the effects which deep mixing has on the behaviour of nitrogen abundance and CN band strengths on the red-giant branch could be quite sensitive to whether there were primordial C+N or C+N+O differences between the progenitors of these stars. Carretta *et al.*[44] argue that this may be the case, which in turn has ramifications for the types of stars that contributed to the primordial inhomogeneities within globular clusters. If the enriching stars had a significant range in mass, then their ejecta could have contained a range of products from the CNO bicycle, Ne–Na and Mg–Al cycles, and triple-α processes of nucleosynthesis. When such ejecta are combined and homogenized they may still preserve anticorrelations between elements such as N and O, or correlations between N and Na, relative to unprocessed material, but it is conceivable that the total C+N+O abundance would be different.

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SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 196: HR 770, HD 64207, HD 187160, AND HD 212790

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The four stars are all double-lined spectroscopic binaries, none of which has been recognized as such previously, although they are all quite bright stars whose magnitudes range from 6·4 to 7·6. They are main-sequence systems apart from HD 212790, whose primary (at least) is a giant of about K2 type, and they have very unequal components except in the case of HD 64207. The primary components of HR 770 and HD 187160, and both components of the metal-weak system HD 64207, are F stars. HD 212790 has an orbital eccentricity close to 0·5; the other orbits are much less eccentric. The periods of HR 770 and HD 64207 are 269 and 152 days, respectively, while the other two systems have periods of nearly five years.

Introduction

The characteristic that is common to the four objects treated in this paper is their double-lined nature. Apart from that, they are a rather motley lot, and they came to the writer's attention in different ways. HR 770 was selected, in the manner outlined in the *Introduction* to Paper 188[1], from the stars found to be binaries by de Medeiros & Mayor[2], after the listing of all their individual radial velocities had been lodged at the *Centre de Données Stellaires* (although its double-lined nature had been overlooked in that work[2]). HD 64207 was identified through the ΔM_{C_0} criterion[3], in a listing provided privately by Suchkov, as a prospective double-lined system; it did not feature in the survey[4] of such systems that was performed by the writer in collaboration with Suchkov, because its photometrically estimated metallicity of −0·6 dex placed it outside the relevant acceptance criterion (−0·5 < [M/H] < 0·3) for that survey. It is somewhat embarrassing that the author cannot now recall what created the original interest in HD 187160 and HD 212790, both of which were first observed in a run with the Haute-Provence (OHP) *Coravel* in 1986; the interest was evidently not very great at the time, because six years elapsed before they were re-observed. In both of those cases the radial-velocity traces appeared single-lined at the first observation and so did not generate the enthusiasm with which they were later regarded.

HR 770

HR 770 (HD 16399) is the easternmost of a compact group of four 6^m late-type stars within the head of Cetus, about 2° north of ν Ceti, the others being HR 751, 753, and 766. Three sets of measurements[5–7] of its *UBV* magnitudes list results close to $V = 6^m\cdot39$, $(B-V) = 0^m\cdot43$, $(U-B) = -0^m\cdot02$. Its spectral type was shown as F5 in the *Henry Draper Catalogue*; MK classifications of F6 V and F6 IV have been given by Harlan and Cowley, respectively, on the basis of slit spectrograms taken with prismatic instruments giving about 70 Å mm^{-1} at Hγ on the Lick 36-inch refractor and the Ann Arbor 37-inch reflector, respectively. The *Hipparcos* parallax of $0''\cdot01211 \pm 0''\cdot00092$ corresponds to a distance modulus of 4·58 ± 0·17 magnitudes and thus to an absolute magnitude of 1·81, with the same uncertainty. That puts it well above the main sequence and approaches the luminosity of the few late-F stars usually regarded as being of class III; before its parallax was measured, HR 770 was considered by several authors[7–10] to be about one magnitude fainter than the *Hipparcos*-derived value.

At first sight there appears to be a considerable literature on HR 770, but it is limited to only a few topics and much of it is secondary in the sense of being quotations of results from earlier work. Thus O. C. Wilson[11] was just (but only just) able to detect in his 10-Å mm^{-1} Mount Wilson spectra a slight rotational broadening of the spectral lines; he placed the star in his 'rotational class 0+', which he rightly considered to correspond to $v \sin i$ values of 10–15 km s^{-1}. Wilson's estimate was then copied by, and into other papers and catalogues from, Kraft[12] and Danziger & Faber[13] and is clearly the origin of the value 12 km s^{-1} printed in the *Bright Star Catalogue*[14]. Thus, for example, Rutten & Pylyser[15] listed a value of 15 km s^{-1} for the rotational velocity of HR 770, and made it look very secure because it was averaged from the values found in no fewer than five papers. But *all* of them were direct or indirect quotations of Wilson[11], one[16] of which was a misquotation listing Wilson's result as 20 km s^{-1} and thereby raising the value averaged from the five sources to near 15! Balachandran[9], however, offered an independently measured value of 16 km s^{-1} derived from reticon spectra at the McDonald 82-inch coudé, and overlapping syndicates of *Coravel* observers[17,18] listed 16·5 km s^{-1} and 17 km s^{-1}.

The other main interest in HR 770 has related to its chemical abundances, particularly of light elements. Balachandran[9] was the first to measure the lithium abundance, finding $\log \varepsilon(\mathrm{Li}) = 3\cdot10$ on the usual scale on which $\log \varepsilon(\mathrm{H}) = 12$; she also derived [Fe/H] values of −0·13 from Strömgren indices and −0·08 ± 0·16 from her own spectroscopy. The lithium abundance is representative of 'non-depleted' values such as characterize the interstellar medium and meteorites; it was subsequently confirmed by Boesgaard *et al.*[19], who also found in HR 770 abundances of beryllium and of boron that are near the maxima for normal stars.

Although the Ca II *H* & *K* flux has not been systematically monitored, isolated measurements by Middelkoop[20] and Duncan *et al.*[21] have yielded values near 0·24 on Wilson's[22] instrumental scale. They seem to fall somewhat above a line drawn by Wilson in his Fig. 1* to indicate the empirical upper bound to the distribution, among the stars on his own programme, of *HK* values with respect to $(b-y)$ colour indices.

The radial velocity of HR 770 was measured a long time ago by Shajn & Albitzky[23] from Cassegrain spectrograms taken at 36 Å mm^{-1} at Hγ with the 40-inch Grubb reflector at the Simeis station of the Pulkovo Observatory. They gave a value of +13·0 km s^{-1}, with a 'probable error' of 1·1 km s^{-1}, as the mean of four plates. The velocities were subsequently tabulated individually, with dates, in a more detailed publication[24] from Pulkovo. They have been transcribed to the head of Table I here. The reason why their authors did not discover the binary nature of the system is immediately evident, inasmuch as the four observations were all made within an overall duration of a fortnight — about 1/20 of the orbital period. De Medeiros & Mayor[2] reported that they had made two radial-velocity measurements with *Coravel*, which gave a mean of +20·76 km s^{-1} with a standard deviation of 4·05 km s^{-1} — a way of saying that they disagreed with one another by 8·1 km s^{-1}. Later, those authors supplied the individual data to the *Centre de Données Stellaires*, and indeed the two measurements differ by 8·1 km s^{-1} although their mean is not exactly as specified in the paper; they are included in Table I, and it was the evidence that they provided of the binary nature of HR 770 that led to that object's being placed on the present writer's observing programme. The star is listed by Nordström *et al.*[24] in their large table of F and G dwarfs, which gives a photometric abundance estimate of [Fe/H] = −0·18 and radial-velocity information identical with that of de Medeiros & Mayor.

Radial-velocity observations were started with the Cambridge *Coravel* in the autumn of 2002 and now number 39. It was not until the fourth one that the weak secondary dip was recognized; in the ensuing three measurements the trace was nearly single-lined and was reduced as such, but in two of the three preceding cases it proved possible afterwards to derive the velocities of both components notwithstanding that the duplicity had passed unnoticed at the time of observation. Subsequently, it was a point of principle always to utilize velocity ranges and integration times sufficient to enable all traces to be reduced as double-lined; one of those obtained near a nodal passage is shown in Fig. 1. It will be seen that the two dips are still quite heavily blended together even at the node, but the velocity separation is just sufficient to enable their individual profiles to be reasonably reliably determined. The relative sharpness of the dip given by the secondary then allows its position to be ascertained even when it is completely superimposed on the one given by the primary. Finally, therefore, there are 35 pairs of Cambridge measurements of both components, and the four early single-lined ones. All are listed in Table I, in which the velocities have been adjusted by −0·5 km s^{-1} from the 'as initially reduced' values, an adjustment that increasing experience suggests to be appropriate to a star of HR 770's colour to maintain as nearly as possible the 'Cambridge zero-point' to which most of the papers in this series are tied. The Simeis and OHP velocities have been adjusted by

*In Wilson's cited paper[22], which was the first in a long series on instrumental monitoring of *H* & *K* intensities, the intensities were left as photon counts in the *HK* signal channel and not normalized by division by the constant 10 000, which was the number of monitor-channel counts at which integrations were all terminated. Thus the point that would represent HR 770 in Wilson's Fig. 1 would come at ordinate 2400, $(b-y)$ = 0·282.

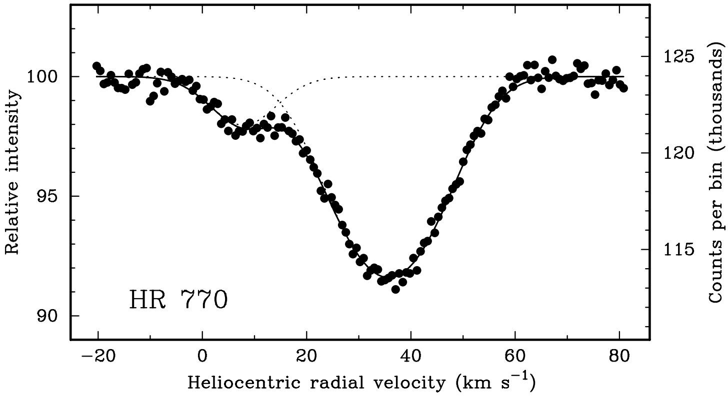

FIG. 1

Cambridge radial-velocity trace of HR 770, obtained on 2004 October 26 when the two dips were at nearly their maximum velocity separation.

+0·8 km s^{-1} for the same reason, although the effect is only cosmetic since, as they were reduced as single-lined, they (like the single-lined Cambridge observations) have been given no weight in the solution of the orbit. The solution depends entirely, therefore, on the 35 double-lined Cambridge measures. To equalize the variances of the velocities of the two components, those of the secondary star have been weighted 1/4 in the solution. The elements are given in the comprehensive Table V towards the end of this paper, after the work on the other three stars treated here has been presented, but the solution is illustrated here in Fig. 2.

TABLE I

Radial-velocity observations of HR 770

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

Heliocentric Date	*HMJD*	*Velocity* Prim. km s^{-1}	*Velocity* Sec. km s^{-1}	*Phase*	*(O−C)* Prim. km s^{-1}	*(O−C)* Sec. km s^{-1}
1930 Sept. 15·02*	26234·02	+17·7		$\overline{97}$·201	—	—
21·00*	240·00	14·4		·223	—	—
23·01*	242·01	9·8		·231	—	—
29·91*	248·91	13·3		·256	—	—
1987 Sept. 7·13†	47045·13	25·4		$\overline{20}$·558	—	—
1988 Nov. 27·01†	47492·01	17·3		$\overline{18}$·219	—	—
2002 Sept. 2·14	52519·14	+37·8	+10·6	0·905	+0·7	+0·8
Oct. 28·10	575·10	22·3		1·113	—	—
2003 Jan. 7·90	52646·90	17·6	38·6	1·380	+0·5	−0·5
17·88	656·88	+18·2	+37·7	·417	0·0	+0·2
27·85	666·85	+20·7		·454	—	—

TABLE I (*concluded*)

Heliocentric Date	*HMJD*	*Velocity* Prim. km s^{-1}	*Velocity* Sec. km s^{-1}	*Phase*	*(O−C)* Prim. km s^{-1}	*(O−C)* Sec. km s^{-1}
2003 Feb. 14·83	53684·83	+24·1		1·521	—	—
22·80	692·80	25·2		·551	—	—
Aug. 30·13	881·13	+15·8	+40·4	2·251	0·3	−0·1
Sept. 14·13	896·13	16·1	40·6	·306	+0·2	−0·2
29·08	911·08	17·1	41·1	·362	+0·4	+1·4
Oct. 18·09	930·09	18·6	35·8	·433	−0·1	−0·9
Nov. 4·07	947·07	21·3	34·1	·496	0·0	+1·3
Dec. 28·92	53001·92	32·2	18·0	·700	+0·1	+0·9
2004 Jan. 12·79	53016·79	35·1	14·4	2·755	+0·3	+1·3
24·87	028·87	36·1	9·8	·800	−0·4	−0·9
Feb. 7·82	042·82	37·8	8·5	·852	+0·3	−0·6
23·81	058·81	37·0	10·2	·911	+0·1	+0·2
Sept. 1·14	249·14	27·8	24·3	3·618	+0·1	+0·7
14·12	262·12	30·3	18·3	·667	0·0	−1·4
26·15	274·15	32·9	17·5	·711	+0·2	+1·3
Oct. 26·08	304·08	36·3	8·6	·823	−0·8	−1·2
Nov. 13·05	322·05	36·9	9·1	·890	−0·5	−0·2
15·02	324·02	37·0	9·4	·897	−0·2	−0·1
Dec. 1·00	340·00	34·9	13·3	·956	0·0	+0·3
16·99	355·99	29·0	19·2	4·016	−1·6	−0·1
2005 Jan. 4·91	53374·91	24·5	26·6	4·086	0·0	−1·6
12·90	382·90	22·3	31·8	·116	+0·2	+0·1
Aug. 16·14	598·14	37·5	11·3	·916	+0·7	+1·0
Sept. 8·15	621·15	32·6	17·7	5·001	+0·8	+0·1
Oct. 27·03	670·03	18·5	38·4	·183	+0·4	+0·8
Nov. 4·08	678·08	16·8	39·0	·213	−0·2	−0·2
25·01	699·01	15·4	40·7	·291	−0·4	−0·2
Dec. 8·95	712·95	15·9	41·2	·343	−0·4	+1·0
2006 Sept. 9·16	53987·16	16·8	38·1	6·362	+0·1	−1·6
Oct. 3·15	54011·15	19·2	36·1	·451	−0·2	+0·5
Nov. 22·01	061·01	28·8	20·4	·636	+0·2	−1·8
Dec. 16·90	085·90	33·1	14·8	·729	−0·5	−0·1
2007 Jan. 11·84	54111·84	37·1	9·3	6·825	0·0	−0·4
20·83	120·83	+37·7	+9·3	·859	+0·2	+0·2

*Photographic measurement by Shajn & Albitzky[23,24]; weight 0.
†Sent to *CDS* by de Medeiros & Mayor[2]; weight 0.

HD 64207

HD 64207 is a $7^{1}/_{2}^{m}$ star about 3° south-following Pollux and just 12′ south of ϕ Geminorum. Of course, its situation in that constellation would lead a superstitious observer to an actual *expectation*[25] that it would prove to be a binary*!

*Is it not hard to believe that it is just coincidence that the leading stars in the constellation (ι and α Gem) are both striking examples of multiple systems that include both spectroscopic *and* visual binaries, and that significant newly discovered double-lined binaries seem to exhibit a marked preference to reside in Gemini, *e.g.*, 23 Gem[4], 65 Gem[26], OW Gem[27,28], HD 44780[26], and HD 55510[29], not to mention the remarkable quintuple system HR 2879[30]? Through inexperience the author did not recognize the double-lined nature of the star treated at the very start, Paper I, of the present series of papers[31], but in due course that object, too, proved[32] to share the usual characteristic and even acquired the coveted constellation designation, OU Gem!

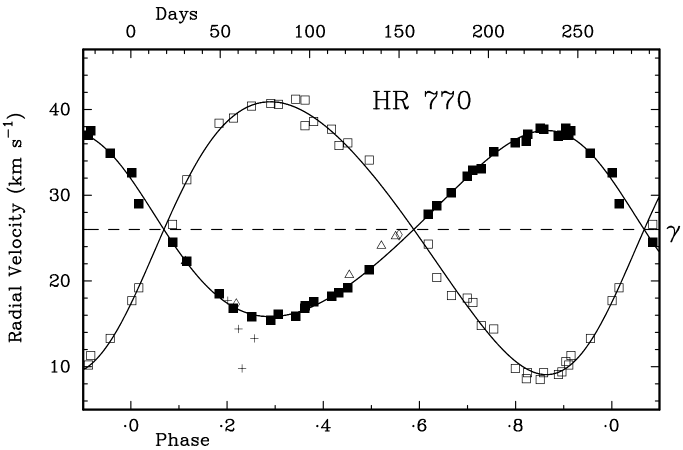

FIG. 2

The observed radial velocities of HR 770 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. The orbital solution is based on the 35 double-lined observations, plotted as filled squares for the primary component and open squares for the secondary, obtained with the Cambridge *Coravel*. Observations reduced as single-lined and not used in the solution are plotted as open triangles (Cambridge), open diamonds (OHP), and plusses (Pulkova[24]).

That expectation was reinforced by the star's appearance in a list, provided in 2002 by Dr. A. A. Suchkov, of objects that were too metal-poor to meet the criteria for inclusion in the collaboration[4] between him and the present writer but nevertheless had large values of his 'over-luminosity index' ΔM_{c_0}. In the case of HD 64207 the relevant quantities were [Fe/H] = −0·59 and ΔM_{c_0} = 1^m·12.

The star has a substantial proper motion, amounting according to *Hipparcos* to +0″·088 in right ascension and −0″·185 in declination, equivalent to a total annual motion of 0″·205 in position angle 151°·6. Attention was drawn to the object, under the designation BD +26° 1668, by Luyten in his successive catalogues of stars with proper motions exceeding 0″·2 annually. It appeared first in 1961 in his *LTT* (*Luyten Two-Tenths*) *Catalogue*[33] as no. 12086, with a motion of 0″·20 in p.a. 149°, and then in *NLTT* (*New LTT*)[34] as having 0″·212 in 148°. The latter catalogue has no running-number designations, and the designation *NLTT* 18582 must have been assigned to HD 64207 by some other author or method. Actually Luyten was not the first to recognize the motion of the object, since proper-motion components of (+110, −181) were published already in the *Yale Zone Catalogue*[35] in 1953; the present author is not sufficiently well versed in astrometric matters to be sure that no still-earlier measurement exists.

There does not appear to be any ground-based photoelectric photometry of HD 64207, but *Hipparcos* offers the values V = 7^m·60, $(B-V)$ 0^m·57. It also gives the parallax as 0″·01544 ± 0″·00113, leading to a distance modulus of 4^m·06 ± 0^m·16 and so to an absolute magnitude of 3^m·54 with the same uncertainty; that places it nearly three-quarters of a magnitude above the main sequence, as would befit an object whose luminosity was nearly doubled by duplicity in comparison with that of a single star. In their large tabulation of the properties of F and G dwarfs, Nordström *et al.*[18], while agreeing with the

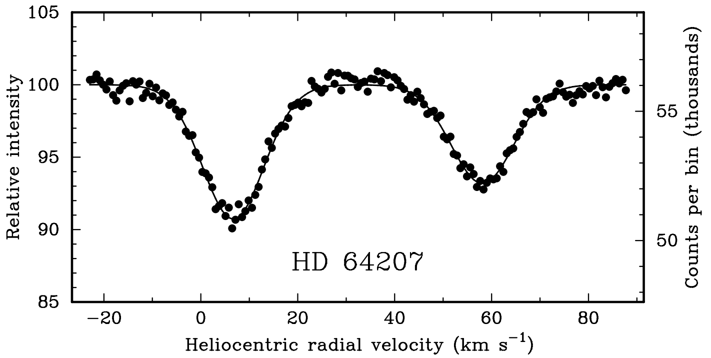

FIG. 3

Cambridge radial-velocity trace of HD 64207, obtained on 2003 December 15 when the two dips were at nearly their maximum velocity separation.

M_V = 3^{m}·54 assessment just given, list it as being 1^{m}·99 above the ZAMS, seemingly implying that the ZAMS is near 5^{m}·5 at type F9.

The star, F8 in the *Henry Draper Catalogue*, was classified as F9 V quite early in the MK era by Moore & Paddock[36] from spectra taken with a prismatic dispersion of 75 Å mm^{-1} at Hγ with the Lick refractor. They had three plates, from which they derived a mean radial velocity of +25 km s^{-1} with a 'probable error' of 1·6 km s^{-1}. Very little else has been published about the object. It features in tables by Ibukiyama & Arimoto[37] and Nordström *et al.*[18] as having [Fe/H] = −0·64 and −0·71, respectively. The latter authors also report that they have 12 *Coravel* radial velocities which show that HD 64207 is a spectroscopic binary with a mean velocity of +32·2 ± 1·0 km s^{-1}, but they also tabulate for it a mass of 1·0 $M_{\odot}$, which seems not to take into account any effect of duplicity. There is one other paper[38] in which *Simbad* says the star is listed, but the *Simbad* entry appears to be a dyslexic transcription of HD *62407*.

The object was placed on the Cambridge programme in late 2002 and proved at the very first observation to be conspicuously double-lined, with somewhat unequal components. There are now 26 observations, all but two of which have been reduced as double-lined. An example — actually an atypical one, with an unnecessarily long integration — appears as Fig. 3; it was obtained at the node close to periastron, when for a small range of phase the two dips are entirely separated. The data are listed in Table II; as described for HR 770, they have all received an adjustment of −0·5 km s^{-1} in the hope of placing them more nearly on the usual zero-point. For no obvious reason the velocities of the secondary star, which gives a dip about three-quarters the depth of the primary's, seem to be significantly more ragged, and to equalize the variances they have had to be weighted $^1/_3$ in the orbital solution. The resulting orbit is shown in Fig. 4, and its elements are presented with those of the other stars in Table V.

HD 187160

HD 187160 is to be found about 1° south of δ Cygni. It is a 7^{m} star a little earlier than solar type, its magnitudes having been determined by Eggen[39] as V = 7^{m}·08, $(B-V)$ = 0^{m}·55, $(U-B)$ = 0^{m}·03. There seems not to be any MK classification;

TABLE II

Radial-velocity observations of HD 64207

All observations were made with the Cambridge Coravel

Heliocentric Date	*HMJD*	*Velocity* Prim. km s⁻¹	*Velocity* Sec. km s⁻¹	*Phase*	*(O – C)* Prim. km s⁻¹	*(O – C)* Sec. km s⁻¹
2002 Dec. 9·16	52617·16	+47·5	+17·3	0·576	+0·1	–0·1
2003 Jan. 5·14	52644·14	37·3	26·4	0·753	–0·7	–0·8
27·08	666·08	18·8	48·6	·897	+0·2	+1·2
Feb. 15·07	685·07	7·7	58·6	1·021	0·0	–0·2
Mar. 14·97	712·97	+32·6		·204	—	—
27·90	725·90	38·8	26·6	·288	–0·4	+0·7
Apr. 7·92	736·92	43·7	20·9	·361	–0·3	0·0
15·88	744·88	46·7	18·6	·413	+0·5	0·0
Oct. 25·24	937·24	44·0	21·0	2·672	0·0	0·0
28·23	940·23	42·6	22·2	·692	–0·2	0·0
Nov. 4·21	947·21	39·2	26·1	·738	–0·2	+0·3
13·16	956·16	+32·7		·796	—	—
Dec. 8·15	981·15	10·0	56·3	·960	+0·1	–0·2
15·19	988·19	7·6	59·0	3·006	+0·1	0·0
27·21	53000·21	13·4	52·4	·085	–0·1	–0·3
2004 Jan. 9·12	53013·12	25·4	39·3	3·169	–0·4	–0·6
Feb. 26·01	061·01	47·5	17·8	·483	–0·3	+0·8
2005 Mar. 25·94	53454·94	11·2	55·6	6·062	+0·4	0·0
Apr. 3·89	463·89	18·5	48·1	·121	–0·2	+0·8
Nov. 19·16	693·16	46·3	19·1	7·622	+0·1	+0·5
2006 Nov. 1·22	54040·22	19·2	46·8	9·895	+0·2	–0·3
17·21	056·21	7·2	60·0	·999	–0·4	+1·0
29·21	068·21	12·4	53·3	10·078	–0·2	–0·4
Dec. 9·17	078·17	22·3	43·6	·143	+0·2	–0·2
2007 Jan. 23·05	54123·05	47·2	17·8	10·437	+0·3	–0·1
Feb. 7·00	138·00	48·0	17·4	·535	+0·1	+0·6
Mar. 25·93	184·93	27·5	38·8	·842	+0·4	+0·2
Apr. 1·89	191·89	20·0	46·5	·888	–0·1	+0·6
9·93	199·93	+12·0	+54·0	·940	–0·2	–0·1

the *HD* type is G5, but a probably more acceptable type is the F8 given by Young[40] on the basis of four prismatic spectrograms obtained at a reciprocal dispersion of 66 Å mm^{-1} at Hγ at the Cassegrain focus of the DDO 74-inch reflector in the late 1930s. Radial velocities measured from those plates were listed as having a mean of +4·3 km s^{-1} with a 'probable error' of only 0·6 km s^{-1}. They cannot be included in the discussion here, however, because they were not published individually but only as a mean.

The parallax of HD 187160 is given by *Hipparcos* as 0″·02336 ± 0″·00058, leading to a very accurate distance modulus of $3^{m}\cdot 16 \pm 0^{m}\cdot 05$ and thus implying an absolute magnitude of $3^{m}\cdot 92$. The reported parallax is derived from an 'acceleration solution', given in Vol. **10**, p. DG11 of the *Hipparcos Catalogue*, where it is seen that there was not only substantial acceleration but also particularly fierce 'third-order motion at 1991·25'. Those facts are readily appreciated qualitatively, since the orbital period of HD 187160 is considerably longer than the duration of

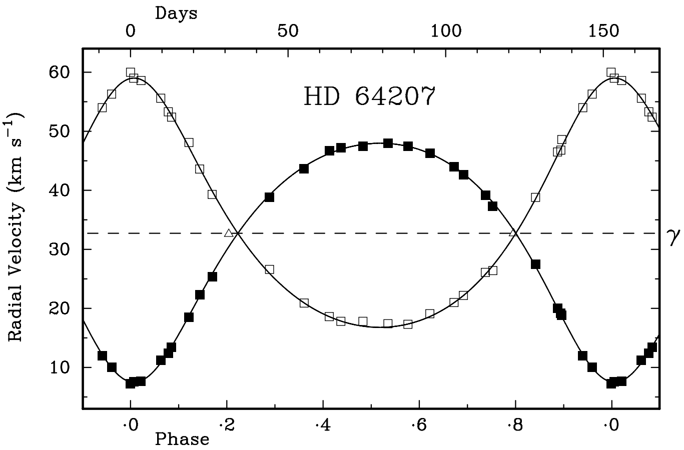

FIG. 4

The observed radial velocities of HD 64207 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. Filled squares represent velocities of the primary star, open squares the secondary, while open triangles refer to blends reduced as single-lined and zero-weighted in the solution of the orbit.

the *Hipparcos* space mission and there happened to be a periastron passage in 1991, the mean date of the mission. We shall see from Fig. 5 below that the components of the binary are very unequal, so the photocentric motion observed by *Hipparcos* for the system as a whole would be a substantial fraction (quantified in the discussion later) of the motion of the primary component, which the orbital elements will show to be well above 1 AU and therefore corresponds in angular terms to more than the annual parallax of $0''\cdot023$.

Unusually, almost the whole of the small literature that exists on HD 187160 is concerned with its radial velocity. First of all, there are two measurements made by Beavers & Eitter[41] with the Iowa State University spectrometer[42] at Ames; in the case of the first one, in addition to the plausible velocity that is closely similar to that listed for the other measure obtained just a week later, there is a second, implausible, one given with a low 'quality' attribution more than 70 km s^{-1} away from it, surely an illusory one. One velocity for HD 187160 was given in a Russian publication[43] by Rastorgouev *et al.* Then, Fehrenbach *et al.*[44] published in 1997 seven measurements obtained by their objective-prism technique in the 1960s. Such measurements are by no means comparable in quality with spectrometer-derived ones, but they are nevertheless included at the head of Table III, where all the available radial velocities of HD 187160 are set out; to put the best possible face on them an empirical correction of +10 km s^{-1} has first been made to the published values. Finally, HD 187160 is included in the Nordström *et al.*[18] survey of F and G dwarfs. The individual measurements are never available for the stars listed in that survey, but the writer permitted such of his own measurements as were made with the OHP and ESO *Coravel*s (and therefore featured in the data base from which the velocity data for the survey were compiled) to be incorporated in the tabulation. The survey[18] notes the object as being a spectroscopic binary and reports that there are 11 radial-velocity measurements with a ΔT (time interval between the first and last observations) of 4132 days. That is

TABLE III

Radial-velocity observations of HD 187160

Except as noted, the sources of the observations are as follows:
1986–1998 — Haute-Provence Coravel; 1999–2006 — Cambridge Coravel

Heliocentric Date	*HMJD*	*Velocity* Prim. km s⁻¹	*Velocity* Sec. km s⁻¹	*Phase*	*(O – C)* Prim. km s⁻¹	*(O – C)* Sec. km s⁻¹
1960 July 24·98★	37139·98	−2·0		$\bar{5}$·494	—	—
26·99★	141·99	+1·0		·495	—	—
1962 July 25·96★	37870·96	+11·0		$\bar{5}$·913	—	—
31·96	876·96	+3·0		·917	—	—
1966 July 16·97★	39322·97	+10·0		$\bar{4}$·746	—	—
1968 July 22·97★	40059·97	+3·0		$\bar{3}$·169	—	—
1969 July 15·97★	40417·97	−5·0		$\bar{3}$·374	—	—
1983 Sept. 28·08†	45605·08	−1·2		0·350	—	—
Oct. 5·07†	612·07	−1·7		·354	—	—
1986 Aug. 28·95	46670·95	+4·7		0·961	—	—
1989 Aug. 1·84‡	47739·84	+3·1		1·574	—	—
1992 Aug. 17·05	48851·05	−6·8	+13·4	2·212	0·0	−0·1
1993 Feb. 12·23	49030·23	−4·1	+12·2	2·315	+0·4	+1·6
15·23	033·23	−3·8	+12·5	·316	+0·6	+2·0
18·22	036·22	−4·5	+11·0	·318	−0·1	+0·5
Mar. 18·19	064·19	−3·7	+11·8	·334	+0·2	+2·0
July 7·05	175·05	−0·1		·398	—	—
9·02	177·02	+0·2		·399	—	—
Sept. 13·81	243·81	+0·9		·437	—	—
Dec. 24·72	345·72	+2·2		·496	—	—
1994 Feb. 18·21	49401·21	+2·4		2·527	—	—
May 3·14	475·14	+3·4		·570	—	—
Aug. 2·00	566·00	+4·4		·622	—	—
Dec. 11·77	697·77	+8·9	−6·7	·698	−0·3	−0·1
1995 Jan. 6·73	49723·73	+8·9	−7·9	2·712	−0·8	−0·7
June 5·10	873·10	+10·8	−9·6	·798	−0·5	−0·3
Dec. 31·73	50082·73	+8·6	−5·5	·918	−0·1	+0·5
1996 Apr. 3·15	50176·15	+3·2		2·972	—	—
Nov. 18·75§	405·75	−5·4	+11·0	3·104	0·0	−0·8
Dec. 17·75	434·75	−6·3	+12·4	·120	−0·2	−0·2
1997 Jan. 2·74§	50450·74	−6·6	+12·0	3·130	−0·3	−0·9
Mar. 31·16§	538·16	−7·1	+14·2	·180	−0·1	+0·5
Apr. 16·14§	554·14	−7·1	+13·8	·189	−0·1	+0·1
May 7·11§	575·11	−6·6	+14·3	·201	+0·3	+0·6
June 17·07	616·07	−6·4	+14·1	·224	+0·3	+0·8
July 18·97	647·97	−6·3	+13·7	·243	+0·1	+0·8
Sept. 8·87	699·87	−5·8	+12·4	·272	−0·1	+0·3
Dec. 20·81¶	802·81	−6·3	+6·2	·331	−2·3	−3·7

TABLE III (*continued*)

Heliocentric Date	*HMJD*	*Velocity* Prim. km s^{-1}	*Velocity* Sec. km s^{-1}	*Phase*	*(O−C)* Prim. km s^{-1}	*(O−C)* Sec. km s^{-1}
1998 Apr. 29 10	50932·10	−0 2		3·406	—	—
July 8·99	51002·99	+1·4		·446	—	—
1999 Dec. 19·75	51531·75	+10·9	−8·6	3·750	+0·3	−0·3
2000 Feb. 11·26	51585·26	+11·5	−8·4	3·780	+0·4	+0·6
Apr. 6·17	640·17	+11·1	−9·7	·812	−0·3	−0·4
24·14	658·14	+11·7	−9·5	·822	+0·3	−0·1
May 20·11	684·11	+11·6	−10·3	·837	+0·3	−1·0
June 7·09	702·09	+11·3	−9·9	·847	+0·1	−0·8
July 18·04	743·04	+10·9	−8·9	·871	+0·1	−0·4
Aug. 2·02	758·02	+10·8	−7·5	·879	+0·3	+0·7
Sept. 20·87	807·87	+8·6	−7·6	·908	−0·7	−0·9
Oct. 19·82	836·82	+8·2	−7·0	·925	−0·2	−1·5
Nov. 13·77	861·77	+7·2	−3·5	·939	−0·2	+0·8
Dec. 9·77	887·77	+6·2	−3·9	·954	−0·1	−1·0
2001 July 5·03	52095·03	−3·6	+10·5	4·073	+0·2	+0·8
Aug. 1·07	122·07	−4·2	+11·7	·088	+0·5	+0·9
Sept. 22·88	174·88	−6·3	+11·9	·119	−0·3	−0·6
Oct. 31·78	213·78	−6·5	+13·4	·141	+0·1	+0·1
Dec. 14·76	257·76	−6·7	+13·7	·166	+0·2	0·0
2002 Jan. 19·74	52293·74	−7·8	+12·8	4·187	−0·8	−0·9
Mar. 2·21	335·21	−7·2	+12·9	·211	−0·4	−0·7
Apr. 7·12	371·12	−7·0	+11·9	·231	−0·4	−1·3
May 2·10	396·10	−6·3	+12·2	·245	0·0	−0·7
June 1·07	426·07	−6·0	+11·8	·263	−0·1	−0·6
July 4·04	459·04	−5·3	+10·1	·282	+0·1	−1·7
Aug. 15·00	501·00	−4·9	+10·0	·306	−0·1	−0·9
Sept. 23·93	540·93	−3·8	+10·0	·329	+0·2	0·0
Oct. 19·91	566·91	−3·8	+9·7	·343	−0·2	+0·3
Nov. 21·87	599·87	−2·7	+9·8	·362	+0·2	+1·2
Dec. 19·79	627·79	−2·3	+8·7	·378	+0·1	+0·8
2003 Apr. 29·11	52758·11	−0·1	+6·7	4·453	−0·5	+2·2
June 19·09	809·09	+2·0		·482	—	—
2004 Apr. 20·13	53115·13	+8·5	−4·3	4·658	+0·5	+0·8
May 24·09	149·09	+8·8	−5·1	·677	+0·2	+0·8
June 28·04	184·04	+9·8	−5·4	·697	+0·6	+1·2
Aug. 8·07	225·07	+9·9	−8·2	·721	0·0	−0·7
Sept. 1·98	249·98	+10·3	−8·1	·735	0·0	−0·2
Oct. 6·96	284·96	+10·9	−8·5	·755	+0·2	0·0
Nov. 12·88	321·88	+10·5	−9·7	·776	−0·6	−0·8
Dec. 17·74	356·74	+11·5	−9·1	·796	+0·2	+0·1
19·71	358·71	+11·4	−9·3	·798	+0·1	−0·1
2005 Jan. 13·73	53383·73	+11·3	−8·1	4·812	−0·1	+1·2
May 28·10	518·10	+10·5	−6·1	·889	+0·3	+1·7
July 29·01	580·01	+8·5	−3·6	·925	+0·1	+1·9
Sept. 2·96	615·96	+6·4	−2·9	·945	−0·5	+0·9
Nov. 4·80	678·80	+4·1	−1·7	·981	+0·2	−1·7
Dec. 8·76	712·76	+2·8	−1·8	5·001	+0·6	−4·1

TABLE III (*concluded*)

Heliocentric Date	*HMJD*	*Velocity* Prim. km s^{-1}	*Velocity* Sec. km s^{-1}	*Phase*	*(O – C)* Prim. km s^{-1}	*(O – C)* Sec. km s^{-1}
2006 Apr. 5·17	53830·17	−4·1	+9·7	5·068	−0·7	+0·4
June 1·09	887·09	−5·6	+11·8	·101	−0·3	+0·2
July 4·07	920·07	−6·3	+12·3	·120	−0·3	−0·2
Aug. 29·92	976·92	−6·6	+13·7	·152	+0·2	+0·2
Oct. 26·87	54034·87	−6·6	+13·4	·185	+0·4	−0·3
Nov. 29·76	068·76	−6·4	+12·6	·205	+0·5	−1·0

*Objective-prism observation by Fehrenbach *et al.*[44].
†Observation by Beavers & Eitter[41].
‡Observation by Rastorgouev *et al.*[43].
§Observed with the Cambridge *Coravel*.
¶Rejected observation.

precisely the interval between the first and the twenty-second observations made at OHP by the writer (who made two more subsequently), so why only 11 are reported in the survey is something of a mystery. In any case, we can be reasonably confident that all the velocities referred to by Nordström *et al.* (and many more) are represented here in the present paper.

As mentioned in the *Introduction* above, the writer's first observation was made in 1986 but then there was a gap of six years before the next one, when the system was revealed as double-lined. Since then the object has been observed systematically and has been seen round three five-year orbital cycles. The velocity amplitudes are not very large, and the dips are always blended together; Fig. 5 shows the most favourable resolution that is ever reached. Altogether there are 81 *Coravel* observations, 24 made at OHP and 57 at Cambridge. They are set out in Table III after the objective-prism[44], Ames[41] and Rastorguev[43] ones; the OHP velocities, as well as the Ames and Russian ones, have received the usual incre-

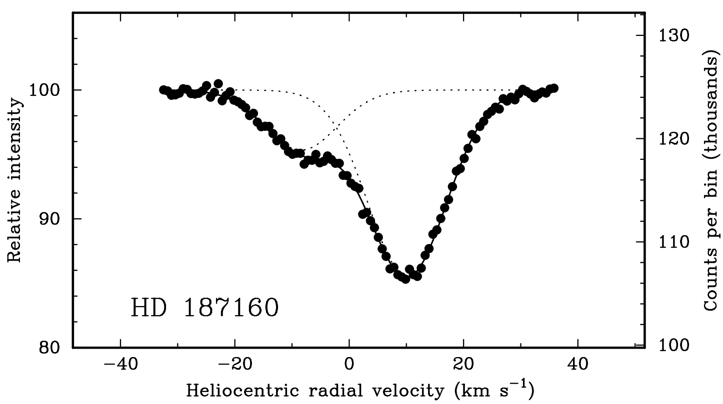

FIG. 5

Cambridge radial-velocity trace of HD 187160, obtained on 2004 September 1 when the two dips were at nearly their maximum velocity separation.

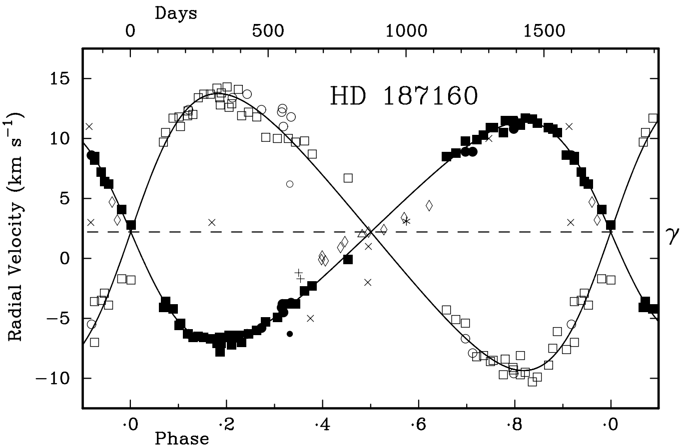

FIG. 6

The observed radial velocities of HD 187160 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. The symbols for Cambridge observations are as in Fig. 4. Analogously, filled and open circles, and open diamonds for blends, denote OHP measurements; small symbols are used for the OHP observation that was rejected. Measures made by the French objective-prism syndicate[44] are plotted as crosses; they have received an empirical correction of $+10$ km s^{-1}. There are two measurements by Beavers & Eitter[41], shown as plusses, and one by Rastorgouev *et al.*[43], indicated by an asterisk. All the published observations were measured as blends and have been zero-weighted.

ment of $+0 \cdot 8$ km s^{-1} and the Cambridge ones $-0 \cdot 5$ km s^{-1}. Eleven of the OHP observations were reduced only as single-lined; only one of the Cambridge ones was so treated, but that is largely because by the time that those observations were made the orbit was more or less known and observations were suspended in the vicinities of the conjunctions. The Cambridge and OHP data prove to give similar residuals after one altogether 'wild' OHP observation was rejected, so with that exception they were attributed equal weights in the solution of the orbit. A global weighting of $^1/_{10}$ has been appropriate to the weak secondary. On that basis the orbit shown in Fig. 6 has been derived; its elements appear in Table V.

HD 212790

HD 212790 is in a very rich Milky-Way field about a degree and a half north of β Lac. Its representation in *Uranometria 2000·0*[45] looks very peculiar because *two* bites are taken out of the dot that symbolizes it, to accommodate the dots for the fainter but closely adjacent stars BD 53° 2875 and HD 212827. As in the case of HD 64207 above, there is no ground-based photometry, but *Hipparcos*/*Tycho* again comes to the rescue with $V = 7^{m} \cdot 14$, $(B-V) = 1^{m} \cdot 10$. The parallax is $0'' \cdot 00536 \pm 0'' \cdot 00068$, just large enough to give a distance modulus that is tolerably accurate at $6^{m} \cdot 35 \pm 0^{m} \cdot 28$ and thereby demonstrating that (unlike the other stars treated in this paper) HD 212790 is a giant, with $M_V \sim +0^{m} \cdot 8$. The *Hipparcos* solution for the parallax includes acceleration, as in the case of HD 187160, but naturally at a more muted level since the star is so much further away.

The *HD* type is K2. There are two MK classifications, both made at very low dispersion (280 Å mm^{-1} at Hγ) with the 4° objective prism on the Warner & Swasey Observatory *Burrell Schmidt*[46]. Nassau & van Albada[47], in a preliminary investigation in which they were setting up a scheme of luminosity classification at that dispersion (classification by *type* had already been set up by Nassau & Seyfert[48]), found HD 212790 to be of type K0 III; they already knew it to be a giant through the parallax that they found in Schlesinger's[49] catalogue*. In the ensuing survey of Milky Way fields, McCuskey[51] classified HD 212790, under the alias LF4 +53° 667, as K2 III.

The only other papers that have specific reference to HD 212790 appear to be ones referring to radial velocities. The earliest (1931) is the second[52] of Redman's papers which set out to quantify Galactic rotation from radial-velocity measurements, made with the Victoria 72-inch telescope and a Cassegrain prism spectrograph giving 90 Å mm^{-1} at Hγ, of seventh-magnitude K giants near the Galactic equator. It was only to be expected that the K stars, selected from the *Henry Draper Catalogue*, would include a small proportion of dwarfs; Redman identified them from his spectra and omitted them from his discussion. In the text of the paper he refers, giving *HD* numbers, to seven stars that he considered to be dwarfs, and three stars, one of which was HD 212790, about which he was uncertain but classified as "dwarfish". In the ensuing table of velocities for 224 stars, the "dwarf" and "dwarfish" classifications are repeated in a column giving notes but, in just the one case of immediate concern to us here, HD 212790, the star is omitted altogether from the table! The present writer, however, is in a position to make good the omission, because upon Redman's demise in 1975 he (the writer) inherited the original reduction sheets for all the measurements in the relevant paper[52] and the preceding one on the same topic[53], which together served as the source list for one of the early programmes of photoelectric radial-velocity measurement[54]; moreover he can supply the date of the observation, which would not in any case have featured in the published paper. Thus Redman's hitherto unpublished observation is included near the head of Table IV.

In 1938 Christie & Wilson[55] gave a velocity of $-33 \cdot 8$ km s^{-1} with a 'probable error' of $1 \cdot 6$ km s^{-1} as the mean of three measurements that were no doubt made on the same plates as those used by Adams *et al.*[50] to estimate the spectroscopic parallax. The three velocities were published individually, with their (1927/8) dates, many years later by Abt[56], and are included here in Table IV, as is the single measurement made in 1983 and listed by Beavers & Eitter[41].

The present writer made two observations, just three days apart, with the OHP *Coravel* in 1986, and then no more until 1992 when (just as for HD 187160) the double-lined nature was recognized for the first time. Fig. 7 illustrates a radial-velocity trace obtained at a time when the separation of the dips was almost at its maximum. The system has been observed now for three cycles of the orbit; there is a total of 30 OHP plus 64 Cambridge observations, of which 19 and 54, respectively, have been reduced as double-lined, so the effective number of measurements available for the derivation of the orbit is 73. It will be appreciated from Fig. 7 that when the difference between the components' velocities is very small it becomes impracticable to derive the two velocities individually. All the measurements are listed in Table IV. Those from OHP (and from other sources, which

*One's incredulity that a reliable parallax could have been known before the days of *Hipparcos* is assuaged by the recognition that that parallax catalogue[49] incorporates spectroscopic as well as trigonometrical parallaxes, and in the case of interest it is quoting the π_{sp} found by Adams *et al.*[50], who also agreed with the *HD* type of K2, at Mount Wilson.

TABLE IV

Radial-velocity observations of HD 212790

Except as noted, the sources of the observations are as follows:
1986–1998 — Haute-Provence Coravel; 1999–2007 — Cambridge Coravel

Heliocentric Date	*HMJD*	*Velocity Prim. km s⁻¹*	*Velocity Sec. km s⁻¹*	*Phase*	*(O–C) Prim. km s⁻¹*	*(O–C) Sec. km s⁻¹*
1927 Aug. 18·44★	25110·44	−28·7		$\overline{12}$·724	—	—
Oct. 18·20★	171·20	−36·7		·757	—	—
1928 Aug. 28·41★	25486·41	−33·8		$\overline{12}$·931	—	—
1929 Dec. 9·10†	25954·10	−8·8		$\overline{11}$·189	—	—
1983 Nov. 21·04‡	45659·04	−13·8		0·044	—	—
1986 Aug. 26·04	46668·04	−30·7		0·599	—	—
29·02	671·02	−30·3		·601	—	—
1992 Aug. 17·06	48851·06	−36·3	−18·1	1·802	−0·2	−0·3
1993 Feb. 12·77	49030·77	−36·2	−18·0	1·901	−0·1	−0·2
Mar. 23·15	069·15	−34·6	−16·9	·922	+0·4	+2·1
July 7·07	175·07	−25·9		·981	—	—
Sept. 12·03	242·03	−17·8	−35·7	2·017	−0·1	+2·0
Dec. 27·78	348·78	−13·7	−38·6	·076	+0·4	+3·1
1994 Jan. 9·75	49361·75	−14·5	−38·7	2·083	−0·3	+2·8
Feb. 18·23	401·23	−14·8	−37·4	·105	+0·2	+3·2
May 2·14	474·14	−16·5	−35·7	·145	+0·4	+2·9
Aug. 2·07	566·07	−19·0	−34·9	·196	+0·3	+1·1
Dec. 11·81	697·81	−22·9	−37·8§	·269	−0·6	−5·0
27·77	713·77	−23·2		·277	—	—
1995 Jan. 7·75	49724·75	−23·5	−40·5§	2·283	−0·7	−8·3
June 3·08	871·08	−25·9		·364	—	—
Dec. 31·78	50082·78	−28·3		·481	—	—
1996 Apr. 3·16	50176·16	−29·5		2·532	—	—
Nov. 18·85¶	405·85	−33·4	−20·2	·659	−0·3	+0·8
Dec. 4·80¶	421·80	−33·1	−19·6	·667	+0·2	+1·2
16·80	433·80	−33·1	−18·3	·674	+0·4	+2·3
1997 Jan. 24·78	50472·78	−33·6	−17·0	2·695	+0·4	+3·1
26·74	474·74	−34·0	−18·3	·696	0·0	+1·8
May 7·12¶	575·12	−35·1	−18·3	·752	+0·1	+0·5
July 19·06	648·06	−35·9	−18·3	·792	+0·1	−0·4
Sept. 9·04	700·04	−36·5	−17·5	·821	−0·1	0·0
9·94	700·94	−36·4	−16·8	·821	0·0	+0·6
Dec. 20·81	802·81	−36·8	−17·8	·877	−0·2	−0·5
1998 May 3·14	50936·14	−30·4		2·951	—	—
July 9·05	51003·05	−25·2		·988	—	—
15·06	009·06	−25·4		·991	—	—
23·02	017·02	−22·7	−36·3	·995	−0·2	−3·8
25·95	019·95	−23·0		·997	—	—
1999 Dec. 7·81	51519·81	−22·6	−33·4	3·272	−0·2	−0·8
8·86	520·86	−22·8		·273	—	—
19·80	531·80	−22·8	−33·0	·279	−0·1	−0·7
28·81	540·81	−23·6		·284	—	—

TABLE IV (*continued*)

Heliocentric Date	*HMJD*	*Velocity* Prim. km s⁻¹	Sec. km s⁻¹	*Phase*	*(O−C)* Prim. km s⁻¹	Sec. km s⁻¹
2000 Jan. 8·76	51551·76	−23·5		3·290	—	—
13·78	556·78	−23·5		·293	—	—
Apr. 10·14	644·14	−25·5		·341	—	—
Sept. 21·00	808·00	−27·5		·431	—	—
Oct. 20·00	837·00	−27·9		·447	—	—
Nov. 13·92	861·92	−28·1		·461	—	—
Dec. 29·73	907·73	−28·4		·486	—	—
2001 July 26·08	52116·08	−31·1		3·601	—	—
Aug. 17·05	138·05	−31·8	−20·9	·613	+0·2	+1·3
Sept. 29·94	181·94	−32·6	−20·8	·637	0·0	+0·8
Oct. 30·86	212·86	−32·8	−19·4	·654	+0·2	+1·7
Dec. 1·87	244·87	−33·2	−19·5	·672	+0·2	+1·2
2002 Jan. 1·81	52275·81	−33·9	−19·4	3·689	−0·1	+0·9
Feb. 23·77	328·77	−34·4	−16·3	·718	+0·1	+3·3
Apr. 7·15	371·15	−35·4	−17·9	·741	−0·4	+1·1
May 27·08	421·08	−35·7	−17·5	769	−0·2	+0·9
July 21·06	476·06	−36·1	−16·1	·799	0·0	+1·7
Aug. 21·12	507·12	−36·4	−16·6	·816	0·0	+0·9
Sept. 27·06	544·06	−36·8	−15·8	·836	−0·2	+1·5
Oct. 19·00	566·00	−36·7	−15·7	·849	0·0	+1·5
Nov. 6·96	584·96	−36·6	−15·4	·859	+0·1	+1·8
2003 Jan. 5·84	52644·84	−36·3	−16·2	3·892	0·0	+1·3
25·79	664·79	−36·2	−17·9	·903	−0·2	0·0
Feb. 17·76	687·76	−35·4	−17·5	·916	0·0	+1·1
Apr. 8·17	737·17	−33·1	−22·0	·943	−0·1	−0·8
May 6·11	765·11	−30·3	−25·7	·958	+0·4	−2·0
26·11	785·11	−28·1		·969	—	—
June 15·07	805·07	−26·4		·980	—	—
25·07	815·07	−25·0	−28·8	·986	−0·2	+1·2
July 13·07	833·07	−22·3	−32·8	·996	+0·1	−0·2
21·06	841·06	−21·4	−34·0	4·000	0·0	−0·3
28·09	848·09	−20·5	−35·3	·004	0·0	−0·6
Aug. 4·07	855·07	−19·8	−36·3	·008	−0·2	−0·7
15·08	866·08	−18·6	−36·6	·014	−0·2	+0·4
30·06	881·06	−16·9	−38·6	·022	+0·1	−0·1
Sept. 11·00	893·00	−15·9	−39·1	·029	+0·1	+0·4
18·01	900·01	−15·6	−39·0	·033	0·0	+1·0
29·01	911·01	−14·9	−39·4	·039	+0·1	+1·3
Oct. 8·05	920·05	−14·7	−39·3	·044	−0·1	+1·8
16·94	928·94	−14·3	−39·7	·048	0·0	+1·7
24·98	936·98	−14·1	−40·8	·053	+0·1	+0·8
Nov. 3·97	946·97	−14·0	−40·8	·058	0·0	+0·9
26·89	969·89	−13·8	−40·1	·071	+0·2	+1·6
Dec. 15 84	988·84	−14·0	−40·2	·081	+0·2	+1·3
2004 Jan. 2·77	53006·77	−14·4	−39·7	4·091	+0·1	+1·5
29·76	033·76	−15·0	−40·2	·106	0·0	+0·4
May 24·10	149·10	−18·5	−37·1	·170	−0·4	+0·2
July 7·10	193·10	−19·2	−36·5	·194	0·0	−0·4
Aug. 19·03	236·03	−20·3	−35·9	·218	0·0	−0·9
Sept. 15·07	263·07	−21·3	−36·1	·233	−0·4	−1·8
Oct. 25·91	303·91	−21·8	−34·2	·255	0·0	−0·9
Nov. 29·86	338·86	−22·5	−31·3	·274	0·0	+1·2
Dec. 26·78	365·78	−23·0	−31·0	·289	0·0	+0·9

TABLE IV (*concluded*)

Heliocentric Date	*HMJD*	*Velocity* Prim. km s^{-1}	*Velocity* Sec. km s^{-1}	*Phase*	*(O – C)* Prim. km s^{-1}	*(O – C)* Sec. km s^{-1}
2006 Apr. 12·18	53837·18	−30·4	−26·1	4·549	+0·1	−2·2
July 3·05	919·05	−31·7	−23·1	·594	−0·1	−0·4
Aug. 29·99	976·99	−32·1	−22·0	·626	+0·3	−0·2
Sept. 21·08	999·08	−32·7	−21·9	·638	−0·1	−0·4
Oct. 31·92	54039·92	−33·3	−22·2	·660	−0·1	−1·2
Dec. 2·84	071·84	−33·7	−18·6	·678	−0·1	+1·9
2007 Jan. 20·80	54120·80	−34·1	−18·4	4·705	+0·1	+1·5

*Mount Wilson photographic observation[55,56].
†Unpublished DAO photographic observation by Redman[52].
‡Ames photoelectric observation by Beavers & Eitter[41].
§Rejected observation.
¶Observed with Cambridge *Coravel*.

all saw the system as single-lined and therefore are not used in the orbital solution) have been adjusted by +0·8 km s^{-1}, while the Cambridge ones have been decreased by 0·2 km s^{-1}. In the solution of the orbit, it has been found necessary to weight the OHP data 1/3 relative to Cambridge = 1, and to downweight measures of the secondary by such a large factor as 40 in comparison with the primary, in order to bring the variances of the data from the different sources and stars into approximate equality. Two specially 'wild' OHP measures of the secondary have been omitted from the solution altogether. The orbit is shown in Fig. 8, while the orbital elements are presented along with those of the other three stars in Table V. An all-too-conspicuous feature of Fig. 8 is that the secondary's velocities appear to have systematically positive residuals at *both* nodes. That is believed to arise through a slight asymmetry of the primary dip, such as is seen in the dip profiles of certain late-type giant stars, compounded with the small depth of the secondary dip. Of course it is impossible to see the exact profile of the dip given by the HD 212790 primary because it is always blended with that of the secondary.

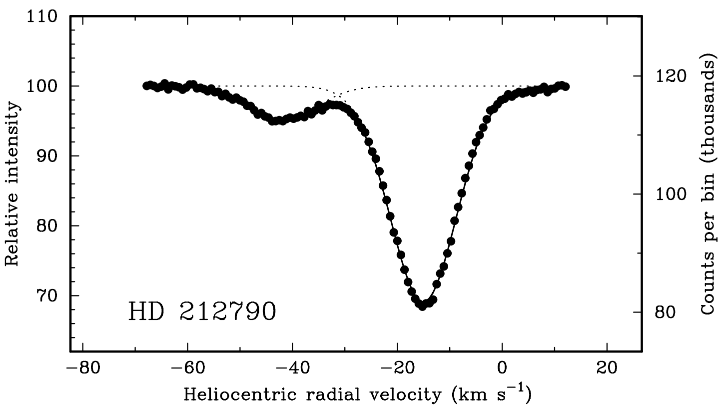

FIG. 7

Cambridge radial-velocity trace of HD 212790, obtained on 2003 November 3 when the two dips were at nearly their maximum velocity separation.

The formal standard error of the secondary's velocity amplitude, K_2, clearly does not take account of any systematic error that may arise from this problem; on the other hand it is greatly increased by the systematic offsets of the velocities and so may, after all, reasonably reflect the uncertainty on K_2.

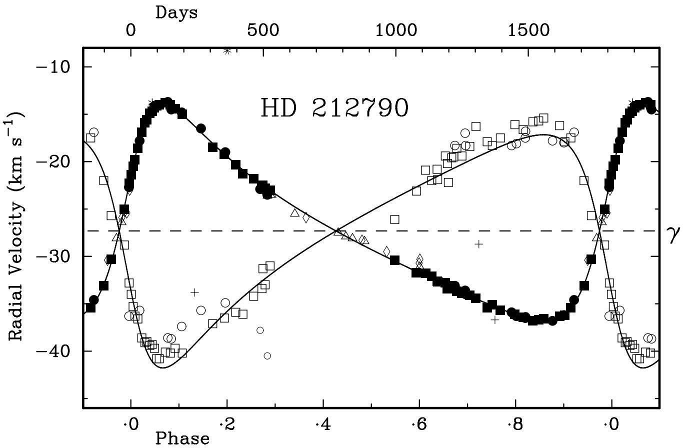

FIG. 8

The observed radial velocities of HD 212790 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. Symbols for Cambridge and OHP observations are exactly as in Fig. 6. Plusses plot the Mount Wilson measurements[55,56], an asterisk (right by the top edge of the diagram) Redman's (ref. 52, but see text), and a star (close to the maximum of the primary's velocity curve) Beavers & Eitter's[41]. The last-mentioned observation was not used in the orbital solution because it was measured as single-lined; the same is true of the earlier photographic velocities which are in any case too inaccurate to be useful, although they were obtained with much larger telescopes and longer exposures than the photoelectric data.

TABLE V

Orbital elements for the four stars

Element	*HR 770*	*HD 64207*	*HD 187160*	*HD 212790*
P (days)	269·03 ± 0·32	152·719 ± 0·021	1743·3 ± 2·5	1815·3 ± 1·2
T (MJD)	53351·8 ± 2·8	53292·73 ± 0·34	51968 ± 9	52841·0 ± 1·2
γ (km s^{-1})	+26·01 ± 0·07	+32·71 ± 0·05	+2·20 ± 0·05	−27·31 ± 0·03
K_1 (km s^{-1})	10·86 ± 0·10	20·24 ± 0·08	9·21 ± 0·05	11·37 ± 0·03
K_2 (km s^{-1})	15·92 ± 0·20	21·12 ± 0·13	11·55 ± 0·16	12·31 ± 0·21
q	1·465 ± 0·023	1·043 ± 0·008	1·255 ± 0·019	1·083 ± 0·018
e	0·134 ± 0·008	0·247 ± 0·003	0·217 ± 0·006	0·5030 ± 0·0024
ω (degrees)	62 ± 4	175·9 ± 0·9	89·9 ± 2·0	290·3 ± 0·4
$a_1 \sin i$ (Gm)	39·8 ± 0·4	41·19 ± 0·17	215·4 ± 1·3	245·3 ± 0·9
$a_2 \sin i$ (Gm)	58·3 ± 0·7	42·97 ± 0·27	270 ± 4	266 ± 4
$f(m_1)$ ($M_\odot$)	0·0348 ± 0·0010	0·1197 ± 0·0014	0·1314 ± 0·0022	0·1788 ± 0·0018
$f(m_2)$ ($M_\odot$)	0·110 ± 0·004	0·1359 ± 0·0026	0·260 ± 0·011	0·227 ± 0·011
$m_1 \sin^3 i$ ($M_\odot$)	0·310 ± 0·010	0·521 ± 0·008	0·839 ± 0·027	0·84 ± 0·03
$m_2 \sin^3 i$ ($M_\odot$)	0·212 ± 0·005	0·500 ± 0·006	0·668 ± 0·013	0·776 ± 0·015
R.m.s. residual (wt. 1) (km s^{-1})	0·44	0·28	0·33	0·18

Discussion

This section is largely devoted to the derivation of models for the four binaries in the light of their radial-velocity traces and mass ratios.

HR 770 has a mass ratio (q in Table V above) approaching 1·5, which suggests that the components began their existence well apart on the main sequence. The system is now some 1½ magnitudes above the point on the main sequence that corresponds to its integrated colour, and because the components are so unequal in brightness most of that excess must be attributed to the primary star, which therefore appears to be starting its evolution but has clearly not yet reached the giant branch of the H–R diagram. The mean ratio of the dip areas in radial-velocity traces is 1 to 0·14, corresponding in stellar-magnitude terms to about $2^{m}\cdot 1$. Because the fainter star can be expected to be of later type than the primary and therefore to match better the spectrum with which it is cross-correlated (the mask within the *Coravel*, designed from the spectrum of the K2 III star Arcturus), the actual magnitude difference between the components might be estimated at $2^{m}\cdot 5$, so the individual components can be expected to have absolute magnitudes of about $1^{m}\cdot 9$ and $4^{m}\cdot 4$.

If we now make the critical (but very reasonable) assumption that the secondary is a main-sequence star, that fixes its type as G0 and its mass as a little more than 1 $M_{\odot}$; then the mass ratio requires the primary to have a mass close to 1·6 $M_{\odot}$. By assigning to the secondary star the tabular[57] magnitude and colours of a star of type G0 V and subtracting its contribution from the observed magnitudes of HR 770, we find that the primary alone is just $0^{m}\cdot 10$ fainter than the integrated system, at an absolute magnitude of $+1^{m}\cdot 91$ (still with the uncertainty, arising from that of the parallax, of $0^{m}\cdot 17$), and its colour indices differ negligibly from those of the system as a whole, at $(B-V) = 0^{m}\cdot 43$, $(U-B) = -0^{m}\cdot 02$. Its mass indicates that it must have started, and spent most of its existence, as a star of type F2 V or thereabouts, and its present luminosity and colours suggest that it has now evolved far enough to have become a magnitude or so brighter and a little cooler than it started. The writer is much indebted to Dr. R. E. M. Griffin for seeing that hand-waving assertion as rather feeble and considering that it should be confronted by actual stellar-evolutionary tracks, which she then produced and supplied for use in this paper as Fig. 9. It will be seen that the two points corresponding in the H–R diagram to the proposed components of HR 770 are matched with almost embarrassing accuracy by coæval models for stars of the expected masses.

Those masses are about 5·2 times the minimum values permitted by the orbit, implying that $\sin^3 i \sim 0\cdot 19$, $\sin i \sim 0\cdot 58$, $i \sim 35°$. The projected rotational velocity of the primary is well determined at 15·5 km s^{-1}; allowance for the orbital inclination, with which the rotation may be expected to be more or less aligned, suggests an actual equatorial velocity of about 27 km s^{-1} and a rotation period near to 4 days. The secondary's rotational velocity is too small to be determinable with any certainty from the radial-velocity traces, in which its signature is always substantially blended with that of the primary (*cf.* Fig. 1) and in most cases is not even visible at all as an independent entity.

HD 64207 appears to be a straightforward main-sequence pair. The masses differ by 4·3 ± 0·8 per cent, indicating a spectral-type difference of between 1 and 2 sub-types. The dips in radial-velocity traces show a mean ratio of 1 to 0·76, or a difference of $0^{m}\cdot 3$ expressed in stellar-magnitude terms. Owing to the better correlation of the secondary (later-type) component with the mask in the *Coravel*, the *V*-magnitude difference can be expected to be slightly greater, say $0^m \cdot 35$, which again represents between one and two sub-types. We can confidently assess the types as being close to F8 V and G0 V, and it is encouraging to notice that the sum of the luminosities of those types in any of the standard tabulations comes remarkably close to the *Hipparcos*-based absolute magnitude of the system as a whole, noted above as being $3^m \cdot 54 \pm 0^m \cdot 16$. Neither component shows much rotational broadening.

HD 187160, as a system of approximately solar type (though lacking an actual MK classification) with an accurately known absolute magnitude of $3^m \cdot 92 \pm 0^m \cdot 05$, is clearly another main-sequence pair. The mass ratio of 1·25 to 1 agrees reasonably well with the dip ratio of 1 to 0·30, arithmetically equivalent to $1^m \cdot 3$ and probably indicating an actual luminosity difference of $1^m \cdot 5$. Stars with types of about F9 and G8 would fulfil the observed mass and dip ratios quite well, and their total luminosity would be correct. Table VI notes the absolute magnitudes and colours, interpolated from the table in *Astrophysical Quantities*[57], of such a pair of stars, and their sum; columns are included for the *B* and *U* absolute magnitudes as well as the colours, because it is the magnitudes that have to be summed and then the combined colours obtained by differencing the combined magnitudes. The model is seen to be 'too red' by $0^m \cdot 05$ in both colours in comparison with the actual star. Any interstellar reddening could only exacerbate the discrepancy. The $(B-V)$ index could be corrected if earlier spectral types were adopted in the model (which would then become 'too bright'), but the only way of obtaining the observed $(U-B)$ is to suppose that the primary star (at least) must be actually bluer than the tabulated colour index, and while we are making that supposition for $(U-B)$ we may as well do it for $(B-V)$ as well — colour indices are not in any case set in stone.

TABLE VI

Photometric model (absolute magnitudes, colour indices) for HD 187160

Star		M_V *m*	$(B-V)$ *m*	$(U-B)$ *m*	M_B *m*	M_U *m*
Model	F9V	4·2	0·55	0·04	4·75	4·79
	G8V	5·6	0·82	0·32	6·42	6·74
	F9V+G8V	3·94	0·60	0·08	4·54	4·62
HD 187160 (observed)		3·92	0·55	0·03		

Comparison of the (interpolated) tabular masses of about 1·10 and 0·85 $M_\odot$ for the components of HD 187160 with the minimum masses mandated by the orbit show that $\sin^3 i$ must be close to 0·77, so $\sin i \sim 0 \cdot 92$ and $i \sim 67°$. Allowance for $\sin i$ shows the semi-axis of the orbit of the primary star to be 234 Gm or 1·56 AU. The figures for the mass (Table V) and *V* luminosity (Table VI) ratios show that whereas the centre of gravity of the pair is 44·5% of the way from the primary to the secondary star, the photocentre in *V*-band light is only 21·6% of the way there, so the photocentric motion is slightly more than half the motion of the primary star: the true semi-axis of the photocentric orbit is about 0·76 AU.

There is a faint star about 1′ south-following HD 187160 — *Tycho* gives very approximate magnitudes of $V = 10^m \cdot 66$ and $(B-V) = 0^m \cdot 79$, and a measurement of the picture that *Aladin* (accessed *via Simbad*) brings up of the field indicates a separation and position angle of about 64″ in 138°. Its radial velocity was

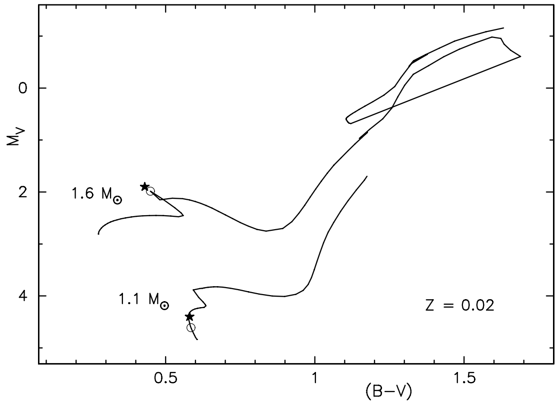

FIG. 9

Evolutionary tracks[58] in the H–R diagram for stars of initial masses 1·6 and 1·1 $M_\odot$, with the points corresponding to age $2{\cdot}28 \times 10^9$ years marked by open circles. The positions of the proposed components of HR 770 are indicated by the star symbols. Diagram kindly produced and supplied by Dr. R. E. M. Griffin.

measured at OHP on 1994 August 2·00 and at Cambridge on 1997 April 16·14, when it was found to be +12·1 and +11·7 km s^{-1}, respectively.

Although we have been able to document HD 212790 as a double-lined binary, and its parallax shows that it has the luminosity of a late-type giant, we lack sufficient information about it to derive a model with any certainty. The masses of the components differ by about 8 ± 2 per cent — not by too much to exclude the possibility that the secondary is an evolved star, like the primary but still at the base of the giant branch in the H–R diagram. The dip areas of the two components in radial-velocity traces show a mean ratio of 1 to 0·15, equivalent to just over 2 magnitudes, so it would be possible to see the system as consisting of two stars of rather similar types, about K1, with absolute magnitudes of about +1 and +3. It seems less attractive to suppose the secondary to be still on or near the main sequence, because then it would need to be so high up the sequence that it should not give much of a dip at all, unless indeed it were an Am star. Unfortunately we have no spectrum of the system, and know no $(U-B)$ colour index to enable us to assess the likelihood of the companion being a relatively hot star. Since we are not in a position to make a reliable estimate of the mass of either star, we are equally unable to estimate the orbital inclination, except to say that it must be well away from 90° because the true masses are not likely to be close to the sub-solar minimum values given by the orbit.

It has been mentioned that HD 212790 is in a very rich field. The two nearest tolerably bright stars are BD 53° 2875 (HD 212790 itself being 53° 2874) and HD 212827 (BD 53° 2877). We refer to them here because they have been casually observed for radial velocity on occasion, when the telescope has been turned to the HD 212790 field. Their positions relative to the bright star are about 100″ in p.a. 21° and 166″ in 140°, respectively. Both 'companions', as well as HD 212790 itself, were included in the major study known as the LF [Luminosity Function] Survey, initiated by McCuskey & Seyfert[59] at the Warner & Swasey Observatory in 1947 and carried out with the Schmidt telescope[46] there, of selected Milky Way fields. In McCuskey's paper[51] on the LF4 field of that survey, HD 212790 was designated +53° 667, the BD star +53° 670, and HD 212827 +53° 677. The spectral types of the last two were given[51] as K3 III and A0 II, respectively; an asterisk against the latter type leads to the initially rather confusing footnote, "Class OB star", but that expression is defined in the paper to mean simply "High-luminosity star of early type". The same type, A0 II, was found by Morgan[60] in a seemingly independent investigation that was published adjacent to McCuskey's paper[51]. The 'OB' status of HD 212827 led to that star's featuring in a photometric study by Hiltner[61], who gave its magnitude and colours as $V = 8^{m}\cdot 30$, $(B-V) = 0^{m}\cdot 26$, $(U-B) = 0^{m}\cdot 12$, from which he deduced a visual absorption (specified as $3E(B-V)$) of $0^{m}\cdot 78$ and an absolute magnitude of $-4^{m}\cdot 4$. For *BV* photometry of BD 53° 2875 we have to fall back on *Tycho*, which found $V = 9^{m}\cdot 43$, $(B-V) = 1^{m}\cdot 79$.

As far the writer is aware, there are no published radial velocities of BD 53° 2875. The star has been observed eight times with *Coravel* instruments, with the results shown in Table VII. The first five measurements were made at OHP, the rest at Cambridge. The obvious conclusion is that the star has a constant velocity, the mean value being $-34\cdot 4$ km s^{-1} with a standard error not much greater than $0\cdot 1$ km s^{-1}.

TABLE VII

Coravel radial velocities of BD 53° 2875

Date (UT)	*RV (km s^{-1})*
1994 Aug. 2·08	−34·4
1995 Jan. 7·75	−34·3
June 3·09	−33·9
Dec. 31·79	−34·5
1997 Sept. 9·94	−34·3
2000 Jan. 13·78	−35·0
2001 Aug. 17·05	−34·1
Oct. 30·86	−34·4

HD 212827 was also observed three times from Cambridge, the observer being then ignorant of its spectral type which would have argued against any such effort. On *one* occasion, however, 2000 January 13·78, the star really seemed to give a dip in the radial-velocity trace, at a velocity of about -61 km s^{-1}; on two other occasions no dip was seen within the scanned range of ±85 km s^{-1}, but the integrations may not have been long enough to bring up the dip out of the noise or possibly the velocity was outside the scan range. There are some radial-velocity measurements already in the literature for the star. A 1963 paper[62] by Abt & Bautz listed three measurements, obtained with a spectrograph giving a reciprocal dispersion of 128 Å mm^{-1} on the KPNO 36-inch telescope; they ranged from -54 to -89 km s^{-1}, a range that was considered enough even at that dispersion to indicate real variation. A few years later, Abt *et al.*[63] published eight additional measurements obtained with the same spectrograph transferred to the 84-inch telescope and used with a grating giving about twice the dispersion. Those later velocities were attributed internally estimated 'probable errors' in the neighbourhood of 2 km s^{-1} and ranged between -51 and -84 km s^{-1}, confirming the variability. On the basis of the ensemble of 11 measurements, Abt *et al.*[63] published an orbital solution for HD 212827 (as well as orbits for ten other stars for which they had analogous material). Most of those orbits appear to the present writer to be even more problematical than the 24 that were derived from comparable but less-underabundant material from the same source and were explicitly rejected in Morbey & Griffin's statistical investigation[64]; indeed the Abt *et al.*[63] paper is one of the ten mentioned at the conclusion of that investigation as being of the same ilk as the two that had actually been proved to be faulty. In particular, it is asserted here that the orbit given[63] for HD 212827, and accorded quality *e* (the lowest quality, "very poor orbits") in the orbit catalogues[65,66] by Batten *et al.*, is not trustworthy; it nevertheless seems likely that the velocity of that star does vary, and there is no reason why the published velocities should not contribute to an orbital solution when adequate additional material is to hand.

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CORRESPONDENCE

To the Editors of 'The Observatory'

Organizations and Strategies in Astronomy

As a contributor to the series *Organizations and Strategies in Astronomy* (*OSA*), in which this *Magazine* was reviewed[1], you will certainly be interested to hear that those *OSA* volumes have recently been distinguished by the prestigious Stroobant Prize of the Belgian Royal Academy. The international jury recognized the appropriateness of pioneering new astronomy-related fields, such as organizational, strategic, and sociological issues[2].

The range of subjects tackled in the seven volumes published so far (some 150 review chapters) has been quite broad: characteristics and strategies of astronomy-related organizations (globally and specifically, nationally and internationally), with a planetary sample including even Antarctica; recruitment and promotion policies; economy of activities; evaluation processes (proposals, individuals, institutions, *etc.*); policies for professional publications; bibliometric studies; evolving sociology of scheduling and coordinated observing; communication, and its diverse facets; series of astronomy-related conferences; interactions with other communities and society at large; and a long list of matters covering the astronomy-related life and context, in the spirit of sharing specific expertise and lessons learned. Rather than being devoted to the publication of hard-science results, the *OSA* volumes describe how astronomy research lives: how it is planned, funded, and organized, how it interacts with other disciplines and the rest of the world, how it communicates, *etc*. They went much beyond what some call "scientometrics", generally reduced in practice to bibliometrics[3].

Thus the *OSA* series has been a unique medium for scientists and non-scientists (sometimes from outside astronomy) to tell of their experience and to elaborate on non-purely-scientific matters — often of fundamental importance for the efficient conduct of our activities. As the initiator and catalyzer of the series, it has been a privilege for me to interact with the various contributors. They have also done their best to write in a way that is understandable to readers not necessarily hyper-specialized in astronomy while providing specific detailed information on their expertise and sometimes quite enlightening 'lessons learned' sections.

The independent readers ('referees') must also be praised as they ensured prompt and constructive reading of the contributions. Their task was essentially to get the best out of chapters solicited from authors who did not always realize the extent of their own expertise, especially in the context of an unusual series in astronomy such as the *OSA* volumes. Some of them had to be encouraged to go as deep as possible. Remarkably, very few contributions — to be counted on the fingers of one hand — had to be turned down, mainly because their authors did not comprehend the purpose and level of the *OSA* series, or, in only one specific case, because the author refused, with an unrelated argument, to compromise with the reasonable requirements from an independent referee seconded by the editor.

Each of the *OSA* volumes offers an updated bibliographical list of papers (from 1990 onwards) on socio-astronomy and on the interactions of the astronomy community with society at large. Authors in the field were encouraged to submit for inclusion the contributions they believe relevant. As illustrated by a histogram included in *OSA 6*'s Editorial, the global number of astronomy-related papers on organizational, strategic, and socio-dynamical issues is growing more than steadily, reflecting increased interest. Years ago, the term 'sociology' carried a negative connotation in hard-science circles where only bibliometric counts were barely accepted. As exemplified by the above diversification, the overall approach has now evolved and matured. The *OSA* volumes have contributed to this and the distinction of the Stroobant Prize should be reflected on all contributors.

Yours faithfully,

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Comets

The purpose of the present note is not to assert that certain assumptions about comets are necessarily true, indeed science is not in the business of asserting absolute truths, but to show by example that a self-consistent picture can be built up without invoking a number of assumptions which have become endemic in the study of comets. It will become apparent that certain common assumptions each necessitate a consequent assumption and that simplification is possible by dropping both.

Our perception of comets has been significantly advanced in recent years by two developments. One of these is the recognition that there must be two populations of comets of distinct nature and origin. The other is the development of chaos theory applied to orbits within the Solar System.

A classical long-period comet is first seen as a fuzzy coma; comet-seekers use not star charts but catalogues of nebulae. It has been asserted that in the centre of the coma there is a star-like image representing a solid nucleus, but this may be an optical illusion. It has been identified as a 'dirty snowball' which gives rise to the coma by out-gassing. However, it seems simpler to assume that the coma is just what it appears to be, a cloud of particles orbiting the Sun independently.

Against this, it has been urged that a gravel-bank could not contain enough volatile material to sustain the coma. However, the idea that an extended object of this kind needs to be made up of gravel-like components is sheer assumption. It could more plausibly be a dirty snowstorm. This image is consistent with a number of observations. A comet does not follow accurately the orbit of a gravitating particle, which is not surprising if it is not in fact a gravitating particle. The apparent optical centre of a comet may be where there is the most mass, or it may be the locus of collisions generating gas and dust, or perhaps a combination of the two. It has been reported that the coma contracts as the comet approaches the Sun (Russell, Dugan & Stewart, *Astronomy*, Ginn and Co, 1926 and 1945, ¶ 501), which is exactly what is expected of a group of independently orbiting particles. If the coma were the product of out-gassing of a central nucleus, it would be expected that this postulated nucleus would be relatively most conspicuous when the comet is distant from the Sun, whereas the opposite is true.

Several mechanisms seem to be available for the production of a coma by outgassing. There is direct solar heating, augmented by the solar wind. There is abrasion resulting from a cluster of independent particles turning itself inside out twice in each coordinate during each perihelion passage. There is the possibility that ice at low-temperature could be formed in an amorphous phase which could be triggered to crystallize by mutual collisions. This phase-change would be expected to be autocatalytic. The change in crystal structure would liberate volatile impurities in the ice.

As the comet approaches the Sun, a cloud of snowflakes would evaporate giving rise to the 'streak' appearance reported for Sun-grazing comets. The orbital velocity near perihelion is so great for such a comet that the material would recondense without appreciable spreading by thermal velocities. When a comet consisting of an extended swarm encountered the atmosphere of the Earth, a meteor shower would be produced. It is often said that such a shower is generated by the debris of the comet, but this is to introduce a further hypothesis; the shower is simply a manifestation of the comet. The image of the comet as a dirty snowstorm is also consistent with the Tunguska event of 1908, particularly the absence of evidence for a central body creating a crater. Each component entering the atmosphere at more than escape velocity would have more energy per unit mass than TNT. For example, the collision of Comet Shoemaker–Levy with Jupiter reveals a more complex structure with a number of concentrations of mass.

It was long supposed that short-period comets arose by perturbation of originally long-period comets by the gravity of planets, particularly Jupiter. However, more recent detailed calculations show that, although this mechanism presumably operates, it cannot produce as many short-period comets as are observed. Some other source is needed, and is provided without further hypothesis by the Edgeworth–Kuiper objects. At the temperatures these objects would assume at their distance from the Sun, even nitrogen would be an ice and it is plausible that such objects would out-gas when warmed by coming nearer to the Sun. This would produce an object having a central nucleus and giving rise to cometary activity.

The Edgeworth–Kuiper belt is now increasingly attested by direct observation. The same cannot be said of the so-called Oort cloud. Professor Oort introduced this in his Halley Lecture at Oxford as a shell. This has been bitterly denied, but I can attest it by direct experience, having been present at that lecture. It is also so reported in *The Observatory Magazine* (**106**, 186). He had drawn a histogram based on equal intervals of reciprocal aphelion distance. This was the origin of the notion of a shell, but a similar argument would lead to the conclusion that my house is surrounded by a shell of capital cities! The audience understood that this was just an artifact of the coordinate system, and when the Oort School also understood this, the image was modified to that of a cloud which was supposed to constitute a reservoir of these comets, for which there is no evidence. Comets of this category occupy a very restricted region of phase space, and line accretion (which is not an *ad-hoc* assumption, but is predicated on dynamical grounds) seems very suited to delivering comets within this phase space; some comets indeed become Sun-grazers.

It may be said that Oort was partly right for the wrong reason. Since a comet is observed to lose mass at every perihelion passage, it can survive for only a limited time. It can become observable if its perihelion distance is relatively small. Thus we can observe only comets occupying only a limited region of the phase space, and we observe exactly what we could in principle observe; it is just epistemology and gives no information about the actual distribution of cometary orbits. It certainly does not justify the assumption of a reservoir of comets, for which there is no evidence.

Chaos theory now provides a mechanism for Edgeworth–Kuiper objects to be fed into short period orbits. It is known that some asteroid orbits having periods harmonically related to that of Jupiter are stable, notably the Trojan group, whereas other such orbits are chaotic and the gradual emptying of these orbits by chaotic motions has given rise to the Kirkwood Gaps. It is plausible that some orbits harmonically related to the orbital period of Neptune are similarly chaotic but their evolution would be slower because of the smaller mass of Neptune. These chaotic motions would be expected gradually to deliver ex-Edgeworth–Kuiper objects into short-period cometary orbits. The theory predicts that such motions pass through a phase of high orbital inclination, similar to that of 2003 UB 313.

The motions of molecular clouds relative to the Sun are quite rapid, and since no markedly hyperbolic comets have ever been observed to approach the Sun it follows that classical comets do not exist, or are rare, in such clouds, but have been generated by interaction with the solar gravity. Again, a mechanism for this is available without *ad-hoc* hypothesis, namely line-accretion. Objections to this mechanism have mainly taken the form that not all parts of all clouds meeting the Sun have the right dynamics to deliver comet-like objects in parabolic orbits relative to the Sun, and not all the objects so accreted will escape falling into the Sun at first perihelion passage. However, this does not matter so long as some of the conditions can give rise to comets.

In my opinion, a discussion of this picture of comets — which is quite similar to that described by Russell, Dugan & Stewart (*ibid*, ¶ 519) with some clarifications and refinement due to progress in our understanding particularly of chaos theory and line-accretion, setting aside prejudices and customary assumptions, seeking out inconsistencies or contradictions with observations — would form a worthwhile project for a research student seeking a thesis subject.

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REVIEWS

The First Copernican: Georg Joachim Rheticus and the Rise of the Copernican Revolution, by Dennis Danielson (Walker & Company, New York), 2006. Pp. 263, 24 × 16 cm. Price $25·95 (about £13) (hardbound; ISBN 0 802 71530 3).

If a Copernican is someone who thinks that a Sun-centred cosmos is a more accurate representation of reality than an Earth-centred one, then Copernicus himself was very probably the first, since it was Andreas Osiander, not he, who wrote the preface *Ad lectorem* to *De Revolutionibus* that described heliocentrism as a mere calculating device. But Rheticus was second, and without him there would almost certainly have been no others for many years, and the eventual eponym would have honoured some other. When did we all become Copernicans? Not so long ago as you might suppose. Dr. Watson is disapproving, but not disbelieving when, early in *Study in Scarlet*, Holmes expresses previous ignorance of "the Copernican theory" and the intent to forget it as soon as possible.

How well known is Rheticus, who took his name from his place of birth after his father's surname, Iserin, was proscribed (following his execution for swindling his own medical patients and others)? I tried the experiment with a group of cosmologists at a conference the other day. "Oh. That priest who visited Copernicus", was a typical response. Priest he was not, though his rapid departure from Leipzig in 1551 following an accusation of sodomizing a young man was probably no more 'evidence against' then than it would be now. But visit Copernicus he did, from 1539 (when he was 25 and Copernicus was 66) to 1541.

Coming as a student and remaining as something like a friend and colleague (neither of our heroes would be described as clubbable), it was Rheticus who somehow saw to it that the text of *De Revolutionibus Orbium Coelestium* (which Danielson calls *Revolutions*) was committed to paper. And it was Rheticus who carried the pages from Frauenburg to Nuremburg and saw to their publication. Two years later Copernicus was dead, following a stroke. And the folk-tale (well, astronomers are strange folk, with strange folk-tales) that he saw proof sheets on his death-bed appears to be true, though it was an older friend and physician, Georg Donner, who held out the pages to be seen. Rheticus himself then spent much of his life in medical work and had to be dragged back to consideration of the cosmos and its mathematics by the 1574 arrival of Valentin Otto in Cassovia, where Rheticus was then living. Otto, who presented himself as a student, much as Rheticus had to Copernicus 35 years before, was still there when Rheticus died at the end of the year, and then began the process of further dissemination of the ideas of Rheticus and of Copernicus, so that, by about 1600, there had been, Danielson concludes, fifteen Copernicans.

Who should read this book? It is probably not for small children, though the liberal use of "four-letter word, bathroom" in two poems (one by Martin Luther) in the back-matter would surely appeal to a six-year-old who had just learned the word. I had read and thoroughly enjoyed an advance copy before *The Observatory* asked for a review. My old school friend Margie came to dinner last week saying that she had just read the most marvellous book about astronomy, but it was by a professor of English (Margie's own field, though her territory is 20th-Century literature and Danielson is a Milton scholar), and was it OK to take it seriously? Absolutely, said I, knowing instantly which book she must mean. The book might well be described as serious enjoyment, for it is not, in some ways, terribly easy going. A good many folks you may not have heard of (from three Albrechts to Zell & Zwingli) wander through its pages as well as through Feldkirch, Cassovia, Frauenburg, and Olsztyn. A time-line or chronology would have helped. It was, after all, 101 years from the birth of Copernicus to the death of Rheticus!

For a different and more philosophically-orientated view, see the review by Anthony Grafton, in *American Scientist*, **95**, 177, 2007. — VIRGINIA TRIMBLE.

Contact with Alien Civilizations, by M. A. G. Michaud, (Springer, Heidelberg), 2007. Pp. 460, 24 × 16·5 cm. Price £17·50/$27·50/€22·95 (hardbound; ISBN 0 387 28598 9).

As yet, in the fifth decade of SETI endeavours, there is no confirmation of evidence of signals or other emissions from technological alien civilizations. Confronted with this, Michaud points to the limits of our technology as well as to SETI searches limited in their coverage. As well, he argues that there needs to be stretching of assumptions about locales of technological extraterrestrial species.

Adroitly, the author goes on to challenge popular opinion and thought on the existence or non-existence of such species. He is reliant upon trial-and-error suppositions of the whole of origins in the Universe, and on whether technological civilizations other than our own developed before or around the same time as ours, and survived. If they are defunct, he relies also on the detection of surviving emissions and artifacts. This is stretching the barest possibilities to the utmost, and, if suchlike contact happens, might give some idea of the frequency of other surviving emissions.

Despite unpredictable chances of success, Michaud believes humankind should prepare for contact. He is a leading figure in preparations for possible future contact with ETI. The problem is how to advance in that direction when, on our planet, multi-civilizations and conflict among nations exist. Related to these are tribal-like religious beliefs, philosophies, and ideologies. As such, it is unlikely that technological aliens will receive a one-voiced message from the peoples of the Earth.

Whether that occurs or if a single planetary civilization emerges, it is difficult to foresee how our species would manage relations with alien life on intellectual, scientific, and analytical levels vastly superior to its own. Disorientation and for a while a state without norms could result, giving rise to the human species living like a stranger in a new kind of universe. On the other hand, as Michaud points out, technological "civilizations in contact may conceive of great tasks to assure the survival and future evolution of intelligence."

Michaud also gives special attention to other contact issues, among which: the First SETI Protocol, the IAU Position Paper presented to the Committee on the Peaceful Uses of Outer Space of the United Nations, and the proposed basic meta-laws applicable to all sentient beings. He highlights the complexities, difficulties, and disappointments that go with trying to establish a code of conduct for the legal aspects of encountering aliens. Perhaps, with continuing success to find Earth-like planets in habitable zones around nearby stars[1], international policy-makers might give attention to possible future contact and humankind's future rôle.

This is a timely book; there is not a dull word in it. Recommended. — P. Chapman-Rietschi.

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Parting the Cosmic Veil, by K. R. Lang (Springer, Heidelberg), 2007. Pp. 229, 24 × 16 cm. Price £25·50/$39·95/€32·95 (hardbound; ISBN 0 387 30735 4).

Kenneth R. Lang is a well-known author of astronomy textbooks. The annual indices of this *Magazine* over the last thirty years show six reviewed titles to his name, not counting reprints, the most well-known probably being *Astrophysical Formulae*. This book reveals a side of the author not previously seen. In it he aims to describe "the growing realization of the scope and immensity of the Cosmos", and in order to do this he attempts a fusion of art and science by quoting relevant poetry and pointing out paintings which have an astronomical context, so we find that the Russian artist Kandinsky often took space as a subject for his work, and so did Miró and Paul Klee, and less obviously (at least to the reviewer's eye) Matisse. The apparent similarity between a Jackson Pollock and large-scale structure in the Universe is surely no more than a coincidence.

In his preface the author states that *Parting the Cosmic Veil* is written in "a light, concise, and friendly style that will be appreciated by all", and so it proves. The book contains six chapters which essentially work outwards from a discussion called 'Cosmic vision, war and technology', in which a surprising emphasis is placed on the influence of the military on astronomical and computational technology, to 'The fullness of space', in which the concepts of dark matter and dark energy are introduced. The last chapter, 'Epilogue', seems out of place and its contents might have been interwoven into the preceding text in order not to disrupt the natural flow. However, Lang writes well and clearly and this book can be recommended to the lay reader who wants an overall view of our understanding of the cosmos as it is today.

British readers may be wondering who Freudlich [*sic*] is. This would appear to be Erwin Finlay Freundlich, a German astronomer who settled in St. Andrews in the 1940s taking the name Finlay after his wife. The cross representing the mean velocity of 22 galaxies on a distance–velocity plot on p. 84 appears to be missing, whilst the dates for Eddington (1882–1944), the painting of Guernica (1937), and the reign of the Rosse telescope as the world's largest telescope (1845–1917) are given incorrectly. There is an adequate subject index and an additional 'author' index. In it you find Hendrik van der Hulst cheek-by-jowl with Vincent van Gogh and Rene Magritte followed by Geoffrey Marcy. — ROBERT ARGYLE.

The Future of the Universe, by A. J. Meadows (Springer, Heidelberg), 2007. Pp. 175, 24 × 16·5 cm. Price £19·50/$29·95/€29·95 (hardbound, ISBN 1 852 33946 2).

Describing the future of the Universe in 175 pages is a challenge, but by his logical approach and skilful summary presentation of relevant current knowledge, the author has more than risen to it. I say "more than" because, as Professor Meadows points out in his introduction, the best way to forecast the future is to review the past; and so, in dealing with, for example, the Sun, he summarizes our current understanding of its composition, origin, and past development before explaining its likely future. He goes on to deal in the same way with the Earth, the Solar System, the local Galaxy, galaxies in general, and finally the Universe, so the reader benefits from a rapid exposition of the past as well as the likely future and possible ultimate fate of these phenomena.

The book reads easily and the language is simple. Information rather than literary elegance is the keynote, and in essence Meadows is providing a sequence of lectures at the end of each of which the reader is left not only with a good clear picture of the past, present, and future of the relevant subject but also looking forward to the next instalment.

Although the future of life is touched upon, the timescale and size of the events and objects covered in this book are so vast that life, and in particular human life, comes across as trivial, possibly irrelevant, and very probably ephemeral. The author does mention the Anthropic Principle, and the reader can console himself with the argument that reality cannot exist (or is certainly pointless) without somebody or something to observe it, but Meadows does not develop these philosophical concepts, and is largely content to let the consequences of the future events which he describes speak for themselves as regards their impact upon flora and fauna. And unless we can develop the sort of controls over matter which are currently found only in science fiction, our chances of being around long enough to check the accuracy of his predictions are not good.

One advantage of this kind of rapid overview of the cosmos is that it leaves the reader with a clear perception of the dimensional ratios of Earth, galaxies, and the Universe, and of the major factors which are affecting their development. As regards the Earth, the cyclical accretion and break up of supercontinents is well described, along with its consequences in terms of weather, winds, and temperatures.

In reviewing the path of our Solar System within the Galaxy, the inevitability and the disturbing consequences of the Earth's passage through dust clouds and proximity to supernovae is made clear, and in writing of more distant situations the strange alignment of galactic clusters in 'Great Wall' ribbons, the delicate balance between gravitational galactic coalescence and perpetual expansion, and our current need to postulate the existence of dark matter and dark energy are all presented with simple clarity.

In the final pages of his book Meadows mentions the current estimate that the Universe is 70% dark energy, 25% dark matter, and 5% ordinary matter, and expresses a consequent proper diffidence about the accuracy of his predictions for a cosmos 95% of which we do not properly understand. He then briefly discusses theories of everything, strings, branes, and the mathematical need for 11 dimensions if computers are to provide us with what we think are the right answers; and thus, despite his use of simple language, he leaves his readers with much the same sense of awe and wonder as moved Joseph Addison some three hundred years ago to write his ode to "The spacious firmament on high".

In summary, this is a satisfying and thought-provoking review of current cosmology, informative for the layman and capable by its brevity of inspiring some degree of lateral thinking for the serious astronomer. It has nine colour plates, an index, and advice for further reading, and I recommend it unreservedly. — Colin Cooke.

The Cambridge Encyclopedia of Stars, by J. B. Kaler (Cambridge University Press), 2006. Pp. 324, 28·5 × 23 cm. Price £35/$60 (hardbound; ISBN 0 521 81803 6).

This book covers the whole of stellar astrophysics and summarizes this wide field in a lucid and concise style. Topics which interrupt the flow of the description are separated off into boxes. While the book is very readable, the prose occasionally becomes hyperbolic. For instance, when describing the original Hertzsprung–Russell diagram, the author writes "... shows a band of dwarfs climbing up and to the left, while the giants stomp up and to the right ...".

The book begins with five chapters outlining the subject. First a star must have a unique identity. Originally a name would suffice but, with the development of astronomy, more and more stars were listed and catalogues became larger and larger. Next a star's position must be known and this requires careful definition of the reference frame and proper motion. Another aid to the identification of a star is its magnitude and colour on a defined system. With the measurement of radial velocity and stellar distance, for nearby stars at least, space motion can be calculated and Galactic orbits studied. This section is completed with a description of the structure and rotation of the Galaxy.

The more detailed part of the book follows with a description of stellar spectra and how they reveal the temperature, pressure, and chemical abundances within a star's atmosphere. This leads to the observed H–R diagram. The most important radiation laws are described: inverse-square, black-body, and those of Rayleigh, Jeans, Wien, and Kirchoff. The coverage of atomic physics includes the Maxwellian velocity distribution, the Bohr model of the atom, and Grotrian diagrams.

Double stars are important, not only because they occur so frequently, but also because they are the only way to determine stellar masses. Eclipsing binaries also reveal the radii of stars and, in some cases, the structure of extended atmospheres. Some binaries are so close that the two stars are distorted and may interchange material. Some star systems are triple or quadruple, which lead naturally to a discussion of clusters of stars. Star clusters can be open, globular, or associations, which are only weakly bound gravitationally, if at all. Their stellar content is usually assumed to be coeval, and with the same chemical composition from star to star. However, there is good evidence that ω Cen, one of the most spectacular clusters in the sky, breaks these rules.

Variable stars are important because the time scales on which they vary are a valuable diagnostic. In particular the period of a Cepheid is closely related to its luminosity. Some pulsating stars are not strictly periodic but their mean period is still important. Cataclysmic variables embrace an enormous range between dwarf novae and supernovae.

The book concludes with star formation and evolution, and these chapters pull together much of the material in the earlier part of the book. The birth of stars is intimately connected with the interstellar medium. As clouds collapse to form discs, planetary systems are formed — a rapidly growing field of research. Evolutionary theory predicts the zero-age main sequence and the theoretical H–R diagram. Comparison of the observed and theoretical H–R diagrams can be used to constrain stellar structure; asteroseismology provides further constraints. In the course of their evolution stars cross and re-cross the instability strip, which is delineated by the boundary between constant and variable stars.

There are many, well-designed diagrams to explain the text, the majority taken from earlier books by the author. The coloured illustrations are mostly good, but a minority very poor. A comparison of the latter with the same pictures in earlier books by the author, and with the same illustrations on the web, clearly reveals that the poor quality arises from production faults by the publisher. There is a finely detailed picture of the Crab Nebula which strangely shows the filaments as green, not the red of Hα. The answer is not revealed in the book but can be found on the web. It transpires that the fine details have been brought out by unsharp masking and that intensities have been colour-coded, unlike other illustrations in the book. — DEREK JONES.

Nightwatch: A Practical Guide to Viewing the Universe, 4th Edition, by T. Dickinson (A & C Black, London), 2006. Pp. 192, 27·5 × 28 cm. Price £29·99 (hardcover spiral-bound; ISBN 0 713 67939 5).

Terence Dickinson commands a large share of the market for popular astronomy books in North America, and with good reason: he is experienced, knowledgeable, and writes well. Old hands will remember him as an early editor of *Astronomy* magazine. *Nightwatch* is one of his best-loved works, but it has inevitably become somewhat dated since its last edition in 1998. This revision not only brings the book bang up to date but is also the first edition to be published in the UK.

Dickinson's stated aim is to provide a complete first book of amateur astronomy. He sets the scene by marching into the Universe in eleven steps from the Earth to the remotest galaxies, then returns home to explain the motions and changing appearance of the sky during each night and throughout the year, which baffles all beginners.

Four all-sky charts introduce the main stars season by season, with arrows to guide the reader from the major constellations to their less-easily-recognized neighbours. These charts have one horizon only. The latitude for which they are drawn does not appear to be stated explicitly, but it seems to be the standard North American value of 40 degrees. Dickinson devotes a chapter to choice of equipment, warning strongly against what he terms 'Christmas trash scopes' and arguing that the closest thing to an ideal beginner's telescope is a 6-to-10-inch Dobsonian.

At the heart of the book is a set of 20 charts with detailed labels picking out major objects of interest for observation. The planets rate one chapter, as do the Moon and Sun, although the Moon maps are cursory by comparison with the star charts. Eclipses, comets, and meteors rate special attention, reflecting their significance to amateur observers. The author is an accomplished astrophotographer so inevitably there is a chapter on that skill, completely revised to incorporate the digital revolution. Seasonal star charts for southern observers round out the book.

Nightwatch is attractively designed and enhanced by excellent illustrations. One of its strengths is the photographs by the author himself. The writing contains telling phrases such as "there are about the same number of sand grains in a typical sandbox as there are stars in the Milky Way Galaxy", and the author involves the reader's imagination with musings such as "When I view the Martian disk now I think of the winds that whip the dunes of the vast deserts and howl down the great Mariner Valley."

This is still clearly an American book. As well as the 40° latitude of the star charts, guide prices for equipment are in dollars and suggested pronunciations are distinctly US ("baytuh" and "zaytuh" for beta and zeta, for example). This aside, there is plenty of information and plain enthusiasm to turn anyone into what he terms "a naturalist of the night". — IAN RIDPATH.

Nebulae and How to Observe Them, by S. R. Coe (Springer, Heidelberg), 2007. Pp. 156, 23·5 × 17·5 cm. Price £19·50/$29·95/€24·95 (paperback; ISBN 1 846 28482 1).

This slim volume is part of Springer's *Astronomers' Observing Guides* series which is aimed at more-advanced amateur astronomers. In general, all the titles in the series have the same basic format: an introductory section which goes through the basics of what the objects are and the tools of the trade required to observe them, and then a second section which covers a variety of the objects in more detail. Steve Coe is a well-known author, having already written one book on deep-sky observing for Springer and having also written a series of deep-sky-related columns for both traditional and electronic media. In this book the first 42 pages are taken up with fairly basic information on telescopes, eyepieces, and filters, along with some information on how to select an observing site. I was surprised by the size of the chapter on computer resources as it was only two pages long, since this is now a very important part of the way that most deep-sky observers prepare for observing sessions. The second section of the book describes some of the author's favourite nebulae arranged by observing season. It is useful that this section also covers some nebulae visible from the southern hemisphere. This method of selecting objects is always fraught with danger but I think that the majority of the objects he has selected will be uncontroversial. I found very few errors of fact in the book, the most glaring perhaps was that the surface temperature of O type stars is given as about 12000 K when of course it is around 30000–40000 K. The usual criticism applies here as with most Springer books in that the images are, in many cases, reproduced too small. There are some colour images but they are bound into a centre section away from the part of the book to which they relate.

I must admit I was slightly disappointed by this book, as it could have been so much more. The main question is, can I recommend this book? I think for the readership it is aimed at, the answer is no, but for beginners who want a basic guide to observing nebulae the book will have a few good tips and a list of targets for them to track down. — Owen Brazell.

The Monthly Sky Guide, by Ian Ridpath & Wil Tirion (Cambridge University Press), 2006. Pp. 64, 30 × 21 cm. Price £10·99/$16 (paperback; ISBN 0 521 68435 8).

This popular guide, the product of collaboration between a writer and a cartographer, is now so well known that just a short review of the latest (7th) edition will suffice. Now updated through to 2011, the monthly sky maps (as well as those focussing upon selected seasonal constellations) have been improved by the addition of star colours, while the outline of the Milky Way has been represented more realistically than before. The stellar magnitude steps are now more refined, with increments of 0·5 mag. The text is almost exactly the same as that of the 5th edition on my bookshelf, except for the insertion of internet references, and a short note about observing artificial satellites. The planetary and eclipse notes, again arranged month by month, will help even the deskbound astronomer to avoid missing any important event.

The label 'Ophiuchus' is somehow unrecognizable on the map on page 39. It is hard to find another fault, but given that the Moon is also a constant celestial object (though often demoted to the status of an uninvited guest), it warrants its own map suitable for users of binoculars or small telescopes. *The Monthly Sky Guide* is highly recommended, and remains good value for money. — Richard McKim.

Galaxies and How to Observe Them, by W. Steinicke & R. Jakiel (Springer, Heidelberg), 2007. Pp. 246, 23·5 × 17·5 cm. Price £19·50/$29·95/€24·95 (paperback; ISBN 1 852 33752 4).

The Springer series *Astronomers' Observing Guides* is a bit of a mixed-bag, with some books much better than others; this is certainly one of the better ones. Both authors are experienced amateur astronomers and visual observers and this comes through clearly in the writing. At 246 pages the book is also the meatiest in the series to date.

It is divided into three sections: 'Galaxies and their data', 'Technical aspects on observing', and 'What to observe'. The first section starts by using the Milky Way as an example to discuss the typical 'construction' of a galaxy before moving on to discuss redshift, magnitude, classification, quasars, galaxy clusters, catalogues, *etc.* The chapter on galaxy catalogues is particularly comprehensive, managing to cram in many details on obscure catalogues in addition to the more common ones.

The target readership is the visual observer, not the imager, so the section on 'Technical aspects' has several pages devoted to eyepieces, the eye and vision, and 'how to see'. This is comprehensive and contains much useful material not normally found in observing guides. How to find objects is discussed, with the authors clearly favouring star hopping over 'go to' telescopes. A star-hopping example is given but, as a star hopper myself, I think newcomers will find this difficult to follow. What confuses most beginners is not knowing the field of their eyepiece relative to the star chart and knowing the orientation of the charts relative to the eyepiece view. Field circles and scales on the charts would have helped make it clearer.

Half the book is devoted to the final section, 'What to observe'. This contains numerous tables of possible targets — not just the obvious ones but interesting lists such as edge-on galaxies, giant ellipticals, and peculiar galaxies, all accompanied by comprehensive notes. Working through these lists would keep most observers occupied for several years.

The book is well illustrated with images of galaxies, many in colour, but surprisingly for a book on visual observing there are no sketches. With the exception of a figure showing the structure of the Milky Way, the images have reproduced well. It appears to be free of errors, although there are a couple of occasions when an incorrect word has been used, *e.g.*, 'sensible' is used instead of 'sensitive' (p. 88) and dawn is used instead of dusk (p. 98). A few sentences are slightly confusing and may need to be read a couple of times for their meaning to be clear. Numerous references are given; in fact there are more references in this book than in many academic ones.

In summary, this is an excellent book which I have no hesitation in recommending. Even experienced observers will find much useful information in its pages. — STEWART L. MOORE.

Dark Side of the Universe: Dark Matter, Dark Energy, and the Fate of the Cosmos, by I. Nicolson (Canopus Publishing, Bristol), 2007. Pp 184, 28·5 × 21·5 cm. Price £19·95 (hardbound; ISBN 0 954 98463 3).

About ten years ago, the community of researchers in cosmology agreed on a 'standard model' of the Universe, which still stands today. The detailed agreement between the predictions of the model and new precise datasets is astonishing, and perhaps better than even optimists might have expected. There is no guarantee that this picture will last forever, but it is clearly mature enough that the general public should learn about the remarkable Universe that we inhabit. A host of semi-popular books have been written, and this offering from the experienced science writer Iain Nicolson is the latest.

Nicolson's approach is coffee-table style, with a large glossy format and good reproduction of many colour photographs. But this in no way implies a soft-centred look at the subject, and the text digs quite deeply into details of past and current research. It is this aspect that distinguishes the book from a host of first-year university texts, which can have a similar scientific level and generous serving of pretty colour pictures. But it has to be said that the 'astronomy 101' books do have some educational advantages in the way they concentrate on a few key points. With Nicolson, equations are thin on the ground, which is why it would have been nice to see the ones with key definitions (*e.g.,* Hubble's law and redshift, $v = \mathrm{H}r$, $1 + z = \lambda_{\mathrm{obs}}/\lambda_{\mathrm{emit}}$) displayed or boxed, rather than lost in quite dense text.

From this point of view, the first three chapters on the basics of astrophysical cosmology and the early evidence for dark matter are fairly standard scene setting. This is unavoidable in order to engage the reader who lacks even some basic astronomy background, and is well enough done. I was pleased to see Slipher get more than his usual lack of credit relative to Hubble, even though Hubble's 'discovery' is still presented as having come out of thin air, rather than being one of a number of attempts to find the theoretically predicted linear relation between distance and redshift. Nicolson also treads a nice line in discussing the expanding Universe while avoiding the sort of misleading 'space is swelling up' comments frequently found in basic treatments.

But the book really starts after 48 pages with detailed chapters on MACHOs and particle dark matter in theory and potential observation (with a chapter on the MOND alternative-gravity approach strangely sited in between, and taking more space than it probably deserves). These chapters give a real flavour of research, with details of the experiments and those carrying them out. There is a danger here that a complete approach will degenerate into a flurry of acronyms, but this is just about avoided.

The story then moves to 'dark energy'. Sigh. I wish we had a better name for this, rather than something that is far too similar to 'dark matter'. Better choices would be 'dark tension' or the older 'vacuum energy' — but once popular science writers get hold of something, we are stuck with it. I was pleased to see that Nicolson introduces this topic in an order that I feel represents the real history of the idea in the community. First there is a chapter on inflation, which prompted people to think more seriously about vacuum energy. Nicolson then connects this to the cosmic microwave background, although he has bought some of the publicity from the *Boomerang* experiment to believe that the CMB argument for vacuum energy was only established in 2000, whereas the argument was clearly made based only on upper limits a decade earlier. From this point of view, it is good to have the supernova story told in its correct sequence — as the last piece of confirming evidence. In any case, again these chapters present the story with a nice level of detail.

The closing chapters try to say more about what dark energy might be. This is harder, of course, and the ratio of text to pictures rises. I'm not sure how much this would mean to the reader coming to this material for the first time. They will find equations such as $w = P/\rho$, which takes the sophisticated relativists' convention of $c = 1$ without this being stated. I suspect this material might have been more successful if it had been less ambitious, and retreated to the simpler level of treatment found in the opening chapters.

Overall, then, this is a good attempt to engage a wider readership with the dark universe — but one that I feel falls between some stools. To get over the idea of what dark energy might be would require some more structured pedagogical material; but the coffee-table aspect is not fully exploited either. The text in the book is twin column, and many pictures occupy only a single column — even ones that are so iconic that they cry out for a full page. For example, the magnificent *WMAP* CMB picture is found *four* times, but always at a miserly 3 × 6 cm. This is a pity, since many modern cosmological images are so full of beautiful detail; this book could have captured a market by printing them at the scale they deserve, but this an opportunity that is not always taken. — JOHN PEACOCK.

Calibrating the Cosmos: How Cosmology Explains Our Big Bang Universe, by F. Levin (Springer, Heidelberg), 2007. Pp. 301, 24 × 16·5 cm. Price £19·50/$29·95/€29·95 (hardbound; ISBN 0 387 30778 8).

Calibrating the Cosmos is the first in a series of *Astronomers' Universe* titles that attempt to bring front-line astronomy research to the informed amateur astronomer and scientist. The aim is ambitious — to provide a quantitative yet descriptive account of the evolution of the Universe and its contents — yet the book largely succeeds in this endeavour.

At 223 pages of text, *Calibrating the Cosmos* contains a very broad sweep of ideas, starting with basic concepts in geometry and radiation laws and culminating in an overview of the present-day Universe. The discussion is supported by an impressive array of appendices, chapter notes, a bibliography (including internet-based sources), a glossary, and symbol definitions! This book will serve best the keen amateur astronomer and scientist — those with a good general background of scientific reading who wish to cement their understanding of cosmology into a more coherent whole. However, some background knowledge will certainly help the reader (or prompt them to read more) as the text is punctuated by scientific asides and anecdotes that, to preserve readability and length, cannot always be adequately developed.

Calibrating the Cosmos certainly scores highly by skilfully including hard numbers from recent research (for example, results from the *Wilkinson Microwave Anisotropy Probe* and the Sloan Digital Sky Survey) into the generally clearly written text. This volume will provide readers with a useful link between established concepts in cosmology and the most up-to-date research that is pushing our knowledge of the Universe to new levels of precision and understanding. — JON WILLIS.

Meteor Showers and their Parent Comets, by P. Jenniskens (Cambridge University Press), 2006. Pp. 790, 25·5 × 18 cm. Price £85/$150 (hardbound; ISBN 0 521 85349 4).

That this author wrote this book on this particular subject is no surprise to this reviewer. Peter Jenniskens has been associated with many of the observational and theoretical breakthroughs that have made modern meteor science the thriving field of research that it is today, be it the Leonid-meteor airborne-observing campaigns or tracking down the parent bodies of 'orphan' meteor showers. Being familiar with his work, I expected this to be the long-awaited standard text on the subject. As it turned out, I was not disappointed.

The beginning of the book sees a relatively succinct treatise on the basics, such as the physics behind the meteor phenomenon and the different forces affecting a meteoroid in space. This is by no means comprehensive, but it more than suffices for the purposes of what is to come.

The next four chapters weave an intricate web of association between what we observe 'down here' (meteor showers) and what is 'up there' (cometary parent bodies). Analysis of observational data collected over decades — in some cases centuries — combined with modern numerical methods offers a taxonomy of meteor showers according to the type of cometary parent. The author has made an effort, successfully in my opinion, to present results on each shower as a little detective story in itself, eventually to fall neatly into place next to its siblings as part of the larger picture. This alone justifies the writing of this book and fills what has been to date a yawning gap in modern meteor-science literature.

This book is more than that, however. Throughout the text the reader will find inserts chronicling the author's own experiences in meteor observing, some ending in frustration and disappointment, others crowned by the elation of success. These gold nuggets add a welcome 'novel' quality to the text that is often lacking in efforts of this size and scope.

The final chapter of the book discusses the 'whys', the reasons that the study of meteors is such an essential part of planetary science. I have little doubt that parts of this section, in some form or other, will find their way into many a grant proposal in the future.

Finally, this review would not be complete without mentioning the Appendix: there the reader can find a most comprehensive set of listings on meteor showers — their characteristics, historical observations, and predictions for future activity, complete with mention of possible meteor-shower parent bodies at other planets and opportunities to observe meteoroid impacts on the Moon.

There is little to find at fault in this book. The unavoidable typos are there (*e.g.*, the date in Fig. 19·7 or the misidentified planet in Fig. 20·16) but they do not affect the overall impression of a carefully, lovingly compiled volume of work. References are provided as footnotes at the bottom of each page, obviating the need for rapid flipping back and forth between pages. The style of writing is generally liberal, at times conveying a feeling of ownership of the subject on the part of the author. I expect that not every expert in the field will agree with his interpretation of the facts and suggested parent-body associations, many of which, to my knowledge, are presented here for the first time.

It is equally clear, however, that this book provides the community with something that it has so far lacked: a concise, comprehensive, and up-to-date reference textbook on the subject of meteor showers and their origins. At the same time, the unassuming style of writing makes it accessible to the non-specialist looking to broaden his or her horizons. It is, above all, a good read. — APOSTOLOS CHRISTOU.

Physics of Space Plasma Activity, by K. Schindler (Cambridge University Press), 2006. Pp. 508, 25·5 × 18 cm. Price £45/$80 (hardbound; ISBN 0 521 85897 6).

The plasma environments in our Solar System exhibit a variety of 'active' processes which take place on a wide range of spatial and temporal scales. This volume concentrates on one major topic of considerable theoretical interest, in which plasma systems evolve slowly in a quiescent state over considerable intervals before suddenly becoming unstable and exhibiting major dynamics which involve large-scale topological change in the magnetic-field structure. Two such phenomena are treated in detail, namely the magnetospheric substorm in the Earth's plasma environment, and the solar flare and coronal mass ejection in the solar corona. The book is organized into four sections, the first containing an overview of relevant observations to provide physical motivation, the second and third containing theoretical discussions of 'quiescence' and 'dynamics' which form the core of the book, and then finally a deeper discussion of the observations along the lines of "how did we do?" with regard to the theoretical development. I should also mention that the first section also contains an overview of basic theory, in which in a little over twenty pages the author overviews particle-orbit theory, kinetic theory, plasma-fluid descriptions, magnetohydrodynamics, conservation laws, and plasma discontinuities. This is not a book for beginners!

The two central sections of the book then represent in essence a coherent overview of the theoretical researches pioneered by the author and his colleagues at Ruhr-Universität Bochum over the past thirty years or so, combined with relevant results drawn from the work of others. The section on 'quiescence' is concerned mainly with static plasma equilibria as described in both fluid and kinetic treatments. The chapters in this section provide rigorous theoretical background, illustrated by a number of important exact and approximate solutions, such as the one-dimensional Harris neutral sheet and the two-dimensional extensions that have been used to model the Earth's magnetic tail. The section ends with a discussion of the quasi-static evolution of such systems, such as may occur in the Earth's tail due to its interaction with the solar wind, and in the solar corona due to the differential motion of field-line footprints in the photosphere. A central focus here is on the development within such slowly-evolving, large-scale systems of magnetic field structures which have very small spatial scales in one direction, *i.e.*, thin current sheets. It is within such current sheets that the usual 'ideal' conditions which represent excellent approximations on the large spatial scales (particularly zero effective resistivity) can break down, leading to large-scale changes in the field structure that release stored energy from the magnetic field to the plasma. The section on 'dynamics' that follows then focusses on stability analysis of plasma systems as approached through use of an energy principle, with particular attention being paid to the tearing-mode instability in a current sheet, as a prelude to reconnection. A major chapter on magnetic reconnection in two and three dimensions then follows, as the basic process involved in the topological magnetic-field changes that occur during intervals of strong energy release and plasma dynamics.

Overall, this is a very worthwhile work that coherently draws together a substantial body of research literature. It can be thoroughly recommended to those of a theoretical inclination for its rigorous but clear and physically-motivated treatment of a topic of central importance to the physics of large-scale plasmas in our Solar System, and most probably those beyond. — STAN COWLEY.

Space — 50 Years of Space Exploration, by Piers Bizony (Collins, London), 2007. Pp. 320, 30·5 × 24 cm. Price £30 (hardbound; ISBN 0 007 24222 0).

The race started in earnest when *Sputnik 1* was launched by the USSR on 1957 October 4, at the height of the Cold War. This 58-cm-diameter aluminium sphere bleeped its way around 1440 orbits (initial apogee 939 km and perigee 215 km), finally burning up on 1958 January 4.

Today, about fifty years later, we can look back and assess our progress in the space race. The conclusion from this well-written, lavishly and colourfully illustrated, large-format book is that our 'end of term' report shows rather mixed attainment levels. Piers Bizony, the well-known space author, has concentrated on the exploration of the Solar System. Rockets, and the interiors and exteriors of orbiting space stations, vie for our attention with silver-suited space heroes. On the plus side, we note that 12 astronauts have walked on the Moon, and over 450 have orbited our planet a few hundred kilometres above the surface. Also, we have either landed on or flown by every planet in the Solar System (the demotion of Pluto from planetary status has helped us out here). On the negative side, it is all too apparent that no one has been near the Moon for the last 35 years, and that one out of every 23 astronauts has died in the space endeavour. It is clear that, after our initial sprint, we have recently rather lost the plot. The 'Golden Age' of space travel and exploration is still to come. Hopefully, this wonderful book will help convert the last few decades of space dawdling into a renewed space jog.

Bizony has written a very human book. The magnificent images of the early hardware are balanced by an abundance of portraits and quotations from the pioneers. But the message is clear. When it comes to space exploration, to quote President Ronald Reagan, "the future does not belong to the fainthearted. It belongs to the brave". And to quote Arthur C. Clarke (way back in 1954), "there is one thing that recent years have shown us, it is that anything that is possible is sooner or later done". I live in hope. — CAROLE STOTT.

Soviet and Russian Lunar Exploration, by B. Harvey (Springer, Heidelberg), 2007. Pp. 317, 24 × 17 cm. Price £25·50/$39·95/€32·95 (paperback; ISBN 0 387 21896 3).

More than 30 years have passed since the last Soviet spacecraft to touch down on the Moon brought back 170 grams of lunar soil. Today, it seems hard to believe that the Soviet Union was a serious contender in the race to land a man on the Moon. Details of the barely publicized programmes to send crews around the Moon and eventually deliver a cosmonaut to the lunar surface remained sketchy at best until the early 1990s.

Yet the Soviet efforts to explore Earth's celestial neighbour were not without their successes and achievements, including the first images from the dusty surface, the first robotic rovers, and the first sample returns by automated spacecraft. In this book, author Brian Harvey, a well-known and respected analyst of the Soviet space programme, makes clear his admiration of their dogged determination to succeed, without hiding any of the gruesome details about the many failures — both technical and organizational.

In clear, concise language, Harvey covers every aspect of the Soviet lunar effort, from the pre-*Sputnik* days to the grand finale of *Luna 24*. Along the way, he describes the numerous launch disasters and mishaps which dogged the early Moon launches, the even more spectacular disasters associated with the launches of the giant *N-1* rocket, and the final successes of the robotic programme. The story ends on a high note with a brief mention of Russian long-term plans to return to the Moon.

The book includes many black-and-white illustrations which supplement the text, although the clarity and value of some of these is sometimes questionable. However, as a one-stop reference for the Soviet Moon programme this is hard to beat. — PETER BOND.

The First Men on the Moon: The Story of Apollo 11, by D. M. Harland (Springer, Heidelberg), 2007. Pp. 378, 24 × 17 cm. Price £19·50/$39·95/ €29·95 (paperback; ISBN 0 387 34176 5).

Most people, at least those who had ready access to radio or television, can recall where they were the moment that President Kennedy's assassination was announced to the world, together with the associated shock that accompanied the news. Similarly, but with anticipation replacing shock, and excitement replacing horror, most can also remember the moment that man first walked on the Moon. It is a curious quirk of history that those two events were linked by Kennedy's declaration, on 1961 May 25, that the USA "should commit itself to achieving the goal, before this decade is out, of landing a man on the Moon, and returning him, safely, to the Earth".

I certainly recall the latter event, ensconced in the student TV lounge of Crown College at the Santa Cruz campus of the University of California, at the start of my post-graduate study in astronomy. Armstrong's "small step for a man" heralded that moment when so much science fiction became fact and mankind looked set to embark on a magnificent voyage of discovery. Regrettably, the attention span of the media-dependent 'developed' world is sufficiently short that those in charge of the purse strings soon lost interest and moved off in other directions, wasting the momentum of that first foray away from our home planet. But that is surely no reason to forget what a remarkable achievement the Apollo programme was, in just eight years, to do exactly what President Kennedy had asked. And all this with technology far inferior to that with which we are familiar today. (Even ten years after the Apollo programme, the ground-station hardware used at the outset of the *IUE* project, for example, was amazingly slow and 'clunky' compared with today's PCs, CDs, DVDs, and the rest of it.)

So, with another president of the USA declaring in favour of space exploration, Harland's *The First Men on the Moon* is a timely clarion call to turn our eyes again to the stars (or at least the Moon and Mars). In almost 400 pages, we recall every moment of the *Apollo 11* venture in a text that is densely packed with information and enlivened with many monochrome and colour photographs of the participants, the spacecraft, and the object of their attention — our nearest neighbour in space. Of course, this is by no means the first book on those momentous events, and I found on my shelves a little Penguin book published in 1969 while the dust was still settling back on the Sea of Tranquillity after the departure of Armstrong and Aldrin — Peter Ryan's *The Invasion of the Moon 1969*. I'm not sure the latest offering gives us anything really new, and at times I found myself wondering if the minutiae of the astronauts' wives' activities, the names of many of the more obscure NASA personnel involved in the mission, or the culinary delights presented to the three crew members before their epic adventure adds much to the story. Nor perhaps do a lot of the transcripts of the conversations between the spacecraft controllers and *Apollo 11*, being often quite cryptic (there is a useful glossary at the back to help out here) and occasionally banal. However, some passages, such as the descent to the lunar surface, are really quite exciting to read, and they do capture the spirit of the moment well.

This will be a 'must' for serious space buffs who want to have an archive of all the facts and figures of a pioneering mission, and a nostalgic read for older buffers who fondly remember those heady days. At under £20 it is good value, well produced, and with no errors that I can recall. — DAVID STICKLAND.

Lunar and Planetary Rovers: The Wheels of Apollo and the Quest for Mars, by A. Young (Springer, Heidelberg), 2007. Pp. 305, 24 × 17 cm. Price £25·50/$39·95/€32·95 (paperback; ISBN 0 387 30774 5).

This book relates the story of an 'unsung' technology used in the Apollo missions, namely the *Lunar Roving Vehicle* (*LRV*), and surveys the NASA Mars rovers as well as exploration plans for the future. A large section of the book is dedicated to a detailed description of the *LRV*s operating on the Moon during *Apollos 15, 16,* and *17*. Despite being born in the Apollo generation, *i.e.*, one of those who lived during and watched the actual missions, I had forgotten how essential the *LRV*s were to the science return from the Apollo missions, greatly increasing the number, mass (whole rocks), and types of samples that could be returned due to the extended (geological) survey range the rovers provided in these later missions.

The book also covers the development and subsystems of the *LRV*s. However, this provides a 'minimum' description and left this reviewer wanting to know more about the design and build of the *LRV*s. The book covers the pace of the development programme well, with the Request for Proposals being issued on 1969 July 11 and the first flight *LRV* being delivered two weeks ahead of schedule on 1971 March 14.

During the book, particularly at the beginning, little-known facts emerge, such as Wernher von Braun envisioning rovers in the 1950s, and project HORIZON — a concept for a USA military base on the Moon proposed in 1959; the Saturn rocket design was based initially on such a possible use. The book also relates the development and history of the geological planning behind the missions, which was tied closely to the capabilities of the *LRV*s. Insight is also given into how the astronauts were trained in geology.

As noted by the author during the book-review process prior to publication, he was encouraged to add chapters on the NASA Mars rovers and the 'Vision for Space Exploration', in particular for the latter's requirements for future roving vehicles, which in reality will be similar to the concepts proposed by von Braun in the 1950s. This is done at summary-type level, again making this reviewer want more detail. However, as pointed out by the author and given in the reference list, there are many other excellent books on the NASA Mars rovers, both *Pathfinder* and the *Mars Exploration Rovers*, which detail the design, development, and the 'race against the clock' to get them ready for the 2003 launch opportunity. The Russian rovers are also not neglected, the book having a very short appendix detailing their development problems.

The day-to-day, hour-by-hour, and in some cases minute-by-minute use of the *LRV*s during the Apollo missions are covered *via* transcripts of the radio conversations and by interview with the respective astronauts. Relevant pictures from both the development and missions are distributed (in both colour and black-and-white format) throughout the book.

In summary, the book covers a 'neglected' topic in terms of manned exploration and reminds us how essential the *LRV*s were to the success of Apollo and how essential such vehicles will be in the future exploration of the Moon and Mars. From a personal point of view I would have liked more technical data on the *LRV*s; however, this is a minor grumble with a book that is easy to read and covers a novel subject that has become lost in history. — MARK SIMS.

Space Shuttle Challenger: Ten Journeys into the Unknown, by B. Evans (Springer, Heidelberg), 2007. Pp. 290, 24 × 17 cm. Price £25·50/$39·95/€32·95 (paperback; ISBN 0 387 46355 0).

Evans, whose admitted fascination with space exploration does him credit, is neither astronomer nor space scientist, yet he has amassed an amazing amount of factual information and anecdotes concerning what was arguably the most famous of the team of Shuttle orbiters. He successfully combines technical data, space jargon, and astronaut histories in a way that does keep the story moving despite the amount of sheer information that needs to relayed in order to retain a commendable level of authenticity. All the facts can doubtless be found in sundry technical documents, and all the stories have been told many times from different perspectives, but Evans serves us with a palatable combination that converts batteries of experiments into real-life experiences, puts identities to acronyms, and creates characters out of those who flew, managed, deployed, occasionally mended, and researched with them. Evans had also corresponded by e-mail with many of those persons, thus adding a measure of uniqueness to a book that is only one among many to have been written on this aspect of space exploration. The effect enables the reader to taste the excitement of the many missions they undertook.

However, having reviewed the sister volume, *Space Shuttle Columbia* by the same author, little more than a year ago[1], I did find the newer one less novel and enthralling, though that is not necessarily a valid criticism of the book. All the same, my review of *Columbia* did point out that the non-specialist (and that definitely includes myself) would want to be selective and not need to read all of the large literature on space exploration. *Challenger* broke fewer records than did *Columbia* and made fewer forays into the unknown, yet it was perhaps because of the very dramatic and public nature of its tragic ending in 1986 January that its name became a household word, a shocking demonstration of the risks which the participants absorbed as part of a day's work, and a needle-sharp reminder of humanity's frailty and fragility.

Many of the petty criticisms which I levelled at the *Columbia* review are equally germane here. I found the over-liberal sprinkling of Americanisms decidedly misplaced in a book by an English author (and a school-teacher at that) and published in Europe. Acronyms are handy, but can be baffling when there are too many to commit conveniently to memory — though this book, unlike its sister volume, does have a fairly comprehensive glossary. The rather frequent use of phrases like "sadly, it was not to be ..." did somewhat undermine the author's intention to create a celebration of achievement and not an elegy of loss. The final chapter, reconstructing second by second the events which led to the tragedy, tends to focus heavily on the technical defects at the expense of the moral aspects of the calamity. Carrying the first school-teacher into orbit with the avowed aim of encouraging school-children to pursue careers in teaching (note: not in space science!) must have done its worst in the counter direction by showing fate arriving to that particular teacher — but that question is not raised, nor are the moral issues of the scale of the monetary expenses and losses involved. Several pages are devoted to apportioning blame for the mechanical failures which led to the disaster, but one can read those in the numerous books devoted solely to the matter. That said, the book is generally well-written with remarkably few errors, though occasionally the style grows slightly turgid. The chapters are sub-divided, but with headings that generally give little away and left me confused as to what to expect. A scientific report divides its material between headings and aims to address topics in turn, while a 'story' allows the thread to meander much as it would have appeared to someone living through it. The latter tends to be the style chosen, but what is presented is a technical description rather than a story.

I did grow slightly uneasy about two aspects of this book. It gave very little introductory background to space exploration, astronaut training, and the Shuttle enterprises. That did not bother me much when I reviewed *Columbia* because I had very recently reviewed *Women in Space*[2], which included a broad introduction to the selection and training of astronauts and the dedication and adaptation required, but in *Challenger* I found I missed those necessary preliminaries. The other matter that caused some disquiet was a lack of definite results as spin-offs from the many microgravity experiments carried out on board. The *Challenger* era is a quarter of a century old, and surely it must be known by now whether the experiments performed in microgravity did in fact lead to industrial or other benefits that could (where quantifiable) justify their payload tickets.

I took real issue with just the very last sentence in the book, that "thanks to the sacrifice made by ... in 1986, coupled with the changes NASA implemented in its immediate aftermath, each Shuttle launch that followed was rendered immeasurably safer." *Immeasurably*?? Seventeen years later the writing was on the wall for *Columbia*. In both, there was hindsight evidence of missing maintenance that could (no, should) have prevented the calamity. But if we take no risks we gain nothing, and this raking-over of the *Challenger* loss, which was certainly preventable, did leave me worrying over the balance between risk and reward. If such was Evans' unwritten intention then he succeeded well. Even if not, his book is a scholarly work, and though its strictly astronomical content is very small (as was those of *Columbia* and *Women in Space*) it is a devoted account and excellent value for money at only £25·50. — ELIZABETH GRIFFIN.

References

(1) R. E. M. Griffin, The Observatory, **126**, 304, 2006.
(2) R. E. M. Griffin, The Observatory, **126**, 134, 2006.

Columbia: Final Voyage, by P. Chien (Springer, Heidelberg), 2006. Pp. 454, 24 × 17 cm. Price £17·50/$27·50/€22·95 (hardbound; ISBN 0 387 27148 1).

You probably do not need to be reminded that the oldest of the Space Shuttles, *Columbia*, burned during its 28th re-entry, on 2003 February 1, a little more than 17 years after *Challenger* burned on launch. Author Chien had been following the mission and getting to know its six-man, one-woman crew for some months before launch, talked with them one day during the mission, and afterwards devoted a great deal of time and effort to finding out as much as a NASA outsider could about what had gone wrong.

The book includes brief biographies of the seven who flew and a few who didn't make the cut, and a long description of the preparation and delays associated with *STS-107* (it flew after *STS-113*!). The core is 15 chapters describing the mission day by day: who did which experiments, how they went, and even which musical excerpts awoke them. There is an almost minute-by-minute recounting of February 1 and a "debriefing" on the accident investigation and the associated 'Monday-morning quarterbacking' (note to non-American readers: this means trying to double-guess the guy on a football team who has primary responsibility for choosing the plays minute to minute after the game is over). A bit of the after-match not mentioned is the extra-long period for which three other folk were stuck on the *International Space Station* when Shuttle flights were suspended; but there is another whole book about them (*Too Far from Home*, by Chris Jones).

The author is of the opinion — and says that surviving astronauts, families, and the crew themselves in pre-flight statements have all expressed agreement — that the risk, and so in some sense the deaths, was justified by the importance of the science to be done on the mission, and, to a certain extent accomplished. A real surprise was the extent to which small electronic components, experiment canisters, and even worms survived the crash landing and the subsequent Texas desert heat.

In light of this conviction, I will simply describe some of the sorts of science done by the 85 experiments aboard. It is trivially easy to make fun of the title of a project in someone else's field, and if you are less amused by 'Motions and structure of the filamentary envelope of the Crab Nebula' than by investigations of the rate of hatching of fruit-fly eggs under zero-g, it is only because you are probably also an astronomer.

A large fraction of the experiments dealt with human physiology and biochemistry in space (much involving words you don't generally meet in *The Observatory*), which we must understand if there are to be long, 'personned' missions to Mars and beyond. Another large set concerned other kinds of things that are different in space: flame shapes and propagation, xenon critical-point behaviour, low-power transceivers, filtering by zeolites and several other water-recovery processes, crystal growth, cultures designed to model bone and bone-marrow behaviour and that of the immune system, chemical kinetics, and the behaviour of ants, spiders, fruit flies, bacteria, rats, bees, moss, worms, yeast, silkworms, and snails.

The third class of experiments were various monitors of terrestrial and solar conditions — ozone abundance as a function of height above the Earth's limb; incidence of lightning and dust storms in places not easy to patrol from the ground; small, short-term variations in solar UV flux (which the author says unequivocally are responsible for changes in the Earth's climate); but not a cosmic ray/antimatter experiment, which was moved 'forward' to *STS-108*. For some of these, data were radioed back to Earth during the 16-day mission; for others, some portions of the experiments themselves survived and could be analyzed; though a few were total losses. An early NASA estimate was that one-third of planned scientific results were recovered. That could have risen a bit when some of the surviving canisters were opened and the worms, *etc.*, crawled out, but probably not by much.

Seven may well be very close to the optimum number of deaths to engage public attention. A single one (unless it is Abraham Lincoln) leaves one feeling petty; six million is simply incomprehensible; but the loss of seven individuals, each with a past, family, friends, and now no future, has the human scale essential for tragedy. — VIRGINIA TRIMBLE.

The Seven Secrets of How to Think Like a Rocket Scientist, by J. Longuski (Springer, Heidelberg), 2007. Pp. 167, 24 × 16·5 cm. Price £15·50/$25/€19·95 (hardbound; ISBN 0 387 30876 8).

I quite like to categorize myself as a 'rocket scientist', having worked on a variety of sub-orbital programmes and satellite missions. Also, in an attempt to bring some realism to the experience of 'Physics with Space Science and Technology' students at Leicester, I run a high-power rocket project where, within a limited budget (£450), a complete rocket and payload has to be built and observational data of some kind obtained during a single flight. While this student project has been a great success, one thing that I have always struggled to do is to convey the philosophy and thought processes involved in space projects. Therefore I was intrigued by the title of this book and wondered whether or not it might be a useful read for our undergraduates.

Jim Longuski is a real rocket scientist with a substantial pedigree. He has a PhD in Aerospace Engineering, worked at NASA's Jet Propulsion Laboratory, and now teaches Aeronautics and Astronautics at Purdue University. He has also produced an absolute gem of a book based on this experience. It is divided into seven sections, the 'secrets', that provide basic instruction to the potential space (rocket) scientist (or anyone else who would like to use this approach to life and work). We are invited to: Dream, Judge, Ask, Check, Simplify, Optimize, and Do. Each section is divided into a number of short, easily read chapters, averaging only 2–3 pages in length, which all deliver pointed advice but in an amusing and accessible manner. Longuski's occasional criticisms of NASA and discussion of how the organization lost its way provide a painful illustration of what can happen, in the loss of two Space Shuttles, when the principles he espouses are forgotten. If I had been able to read this book much earlier I might have avoided some of my own difficult, although not tragic, experiences. This book *will* become required reading for all my space-science project students. I recommend it to all aspiring rocket scientists, current rocket scientists who feel the need to reinvigorate their working practices, and, indeed, to anyone who wishes to develop a new way of thinking … like a rocket scientist! — Martin Barstow.

Other Books Received

Numerical Modeling of Space Plasma Flows: Astronum–2006 (ASP Conference Series, Vol. 359), edited by N. V. Pogorelov & G. P. Zank (Astronomical Society of the Pacific, San Francisco), 2006. Pp. 312, 23·5 × 15·5 cm. Price $77 (about £44) (hardbound; ISBN 1 583 81227 3).

The rapid appearance of this volume well within a year of the conference, in 2006 March in Palm Springs, California, is a credit to the editors. Within its pages we find a rich mixture of plasma topics ranging in location from the near-Earth environment to galactic-scale winds. Emphasis is on modelling, with numerical methods, algorithms, simulations, and data handling all discussed. A book to keep the plasma specialist up to date.

THESIS ABSTRACTS

Radio-Source Interactions in Cluster Cores

By Robert John Harris Dunn

Some active galactic nuclei (AGN) emit jets of radio-emitting plasma which travel at relativistic speeds. In clusters of galaxies these AGN are surrounded by a thermal gas — the intra-cluster medium (ICM) — in which the galaxies are bathed. The temperature of the ICM is 10^7 to 10^8 K and so it emits X-rays. The AGN jets plough into the ICM and inflate bubbles of relativistic plasma. These bubbles eventually detach from the central AGN and rise up through the cluster. The high spatial resolution of the *Chandra* X-ray observatory allows a detailed analysis of the morphology of the ICM and the nature of the interaction. Some bubbles do not have any GHz radio emission as the synchrotron-emitting electrons have aged, and these are known as 'ghost' bubbles. From thermodynamic arguments, the energy required to create the bubbles can be calculated, as can their ages, and hence the mechanical power of the AGN can be determined.

The particle content of the plasma within the bubbles was investigated under the assumption that the bubbles are in pressure equilibrium with the surrounding ICM. The energy density of the synchrotron-emitting plasma can be calculated from radio observations and compared to the ICM pressure calculated from the X-ray observations. Limits were obtained on the ratio k/f for a sample of clusters, where k is the ratio of the total particle energy to that in synchrotron electrons, and f is the volume-filling fraction of the relativistic plasma. In some clusters a significant population of particles apart from those assumed to be responsible for the radio emission is required to maintain pressure balance. The evolution of this ratio was investigated in two clusters that have both young and ghost bubbles.

To investigate whether there are any non-relativistic particles intrinsic to the jet or if any are entrained from the ICM, a comparison between the number density on small scales, as calculated from the core radio emission, to that expected from the bulk jet power, as calculated from the expansion of the radio bubbles, was made. Limits were set on the type of jet present in two clusters, assuming that the low-energy cut-off in the electron energy spectrum, γ_{min}, is ~ 1. As previous attempts to determine the matter content of jets have a lower limit of $\gamma_{min} > \sim 100$, the effect of changing γ_{min} on the results was investigated.

The ICM in clusters is expected to cool as it is losing energy *via* the observed X-ray emission. This gas is expected to condense out on the central cluster galaxy where it would form stars. Little cool gas has been found in the X-ray observations and the star-formation rates in the central galaxies are lower than predicted from the X-ray emission. The energy input from the AGN at the centres of some clusters may be enough to prevent the ICM from cooling. The prevalence of radio sources and bubbles in clusters requiring heating was investigated, as was the balance between AGN heating and cooling. The sizes of bubbles expected in clusters that do not show any AGN–ICM interaction were calculated under the assumption that the energy injected into the central regions of the cluster by the AGN in the form of bubbles prevents the ICM from cooling.

Finally, the dynamics of the AGN at the centre of the brightest X-ray cluster in the sky, the Perseus Cluster, was studied. As the bubbles in this cluster are not all at the same position angle, the possibilities of precessing AGN jets, bulk ICM motions, and bubble oscillations were investigated. — *University of Cambridge; accepted 2007 January*.

A full copy of this thesis can be requested from: R.J.H.Dunn.99@cantab.net.

Flares on Active M-Type Stars Observed with *XMM–Newton* and *Chandra*

By Urmila Mitra-Kraev

M-type red dwarfs are among the most active stars. Their light curves display random variability of rapid increase and gradual decrease in emission. It is believed that these large-energy events, or flares, are the manifestation of the permanently reforming magnetic field of the stellar atmosphere. Stellar coronal flares are observed in the radio, optical, ultraviolet, and X-rays. With the new generation of X-ray telescopes, *XMM–Newton* and *Chandra*, it has become possible to study these flares in much greater detail than ever before. This thesis focusses on three core issues about flares: (*i*) how their X-ray emission is correlated with the ultraviolet, (*ii*) using an oscillation to determine the loop length and the magnetic-field strength of a particular flare, and (*iii*) investigating the change of density-sensitive lines during flares using high-resolution X-ray spectra.

(*i*) It is known that flare emissions in different wavebands often correlate in time. However, here is the first time where data are presented which shows a correlation between emission from two different wavebands (soft X-rays and ultraviolet) over various-sized flares and from five stars, which supports the idea that the flare process is governed by common physical parameters scaling over a large range.

(*ii*) As it is impossible to resolve spatially any but a very few giant stars, the only information on spatial dimensions as well as the magnetic-field strength of stellar coronae has to come from indirect measurements. Using wavelet analysis, I isolated the first stellar X-ray-flare oscillation. Interpreting it as a standing coronal flare-loop oscillation, I derived a flare-loop length as well as the magnetic-field strength for this X-ray flare.

(*iii*) The high-resolution soft-X-ray spectra from *Chandra* and *XMM–Newton* allow us to determine temperatures, densities, and abundances of the stellar coronae. Despite a low signal-to-noise ratio because of the relatively short duration of a flare, we find that, if adding up the photons of several flares, certain density-sensitive spectral lines change significantly between quiescent and flaring states. This project led on to investigate the flaring spectrum further, and it is found that the plasma is no longer in collisional ionization equilibrium, but that it is dominated by recombinations. — *University of London, UCL/MSSL; accepted 2007 April.*

The thesis is available electronically at:
http://www.mssl.ucl.ac.uk/www_astro/academic/doctorates.html

T Tauri Stars: Mass Accretion and X-Ray Emission

By Scott G. Gregory

I develop the first magnetospheric-accretion model to take account of the observed complexity of T Tauri magnetic fields, and the influence of stellar coronae. It is now accepted that accretion onto classical T Tauri stars is controlled by the stellar magnetosphere, yet to date the majority of accretion models have assumed that the stellar magnetic field is dipolar. By considering a simple steady-state accretion model with both dipolar and complex magnetic fields I find a correlation between mass-accretion rate and stellar mass of the form $\dot{M} \propto M_{\star}^{\alpha}$, with my results consistent within observed scatter. For any particular stellar mass there can be several orders of magnitude difference in the mass-accretion rate, with accretion filling factors of a few percent. I demonstrate that the field geometry has a significant effect in controlling the location and distribution of hot spots formed on the stellar surface from the high-velocity impact of accreting material. I find that hot spots are often at mid to low latitudes, in contrast to what is expected for accretion to dipolar fields, and that particularly for higher-mass stars, accreting material is predominantly carried by open field lines. Material accreting onto stars with fields that have a realistic degree of complexity does so with a distribution of in-fall speeds.

I have also modelled the rotational modulation of X-ray emission from T Tauri stars, assuming that they have isothermal, magnetically confined coronae. By extrapolating from surface magnetograms I find that T Tauri coronae are compact and clumpy, such that rotational modulation arises from X-ray-emitting regions being eclipsed as the star rotates. Emitting regions are close to the stellar surface and inhomogeneously distributed about the star. However, some regions of the stellar surface, which contain wind-bearing open field lines, are dark in X-rays. From simulated X-ray light curves, obtained using stellar parameters from the

Chandra Orion Ultra-deep Project, I calculate X-ray periods and make comparisons with optically determined rotation periods. I find that X-ray periods are typically equal to, or are half of, the optical periods. Further, I find that X-ray periods are dependent upon the stellar inclination, but that the ratio of X-ray to optical period is independent of stellar mass and radius.

I also present some results that show that the largest flares detected on T Tauri stars may occur inside extended magnetic structures arising from the reconnection of open field lines within the disc. I am currently working to establish whether such large field-line loops can remain closed for a long enough time to fill with plasma before being torn open by the differential rotation between the star and the disc. Finally I discuss the current limitations of the model and suggest future developments and new avenues of research. — *University of St. Andrews; accepted 2007 April.*

Super Star Clusters, Their Environment, and the Formation of Galactic Winds

By Mark Westmoquette

Starbursts and starburst-driven outflows play a central rôle in the evolution of galaxies. However, the paucity of detailed observations of superwinds limits our current understanding of these complex systems.

To this end we have undertaken two intensive ground- and space-based observing campaigns aimed at studying the ionized-gas conditions in two nearby starburst galaxies, M82 and NGC 1569. These two systems host starbursts on different scales: M82 contains densely-packed star-cluster complexes that drive a large-scale bipolar superwind, whereas NGC 1569 exhibits a set of discrete superbubbles powered by only a handful of young massive clusters.

We have used long-slit spectra, obtained with the *Hubble Space Telescope* (*HST*), together with imaging from *HST* and the ground-based *WIYN* 3·5-m telescope, to observe M82 at optical wavelengths. The high-quality *HST* spectroscopy obtained with the *Space Telescope Imaging Spectrograph* (*STIS*) has allowed us to investigate the properties of the gas across the starburst core. By combining high-resolution *HST* imaging with deep *WIYN* observations, we have created the most comprehensive image of the M82 superwind to date, and used it to characterize the outflow morphology.

We also observed the centre of NGC 1569 with the integral-field unit (IFU) of the *Gemini Multi-Object Spectrograph* (*GMOS*) on the *Gemini-North* telescope, and M82 with the *WIYN/DensePak* and *SparsePak* IFUs. We decomposed the observed emission-line profile shapes, and identified an underlying broad (> 100 km s^{-1}) component across the starburst cores of both galaxies. By mapping the spatial variation of each individual line component, we have developed a new model to explain the broad emission and the state of the interstellar medium (ISM) in the central starbursts.

We have also observed the outer-wind environment of NGC 1569 with the *WIYN SparsePak* instrument. We find that the broad line is only found within 500–700 pc of the centre, and speculate that the boundary of this region may indicate the point at which bulk motions begin to dominate over turbulence. — *University of London; accepted 2007 February.*

Dust and Molecules in Interstellar, Circumstellar, and Extragalactic Environments

By Arfon M. Smith

Small-scale-structure variations in diffuse-interstellar-band (DIB) strengths have been detected towards the ρ Ophiuchi complex of stars. Variations of ~ 400 AU in the blue DIBs (λ < 5700 Å) have been detected for the first time with differences detected in eight of the ten bands analysed. Follow-up observations of the yellow/red diffuse bands (λ > 5700 Å) have confirmed the result of Cordiner (PhD thesis, University of Nottingham, 2005 — see also *The Observatory*, **127**, 143, 2007) and shown variations in the majority of the bands analysed. A comparison of the diffuse-band variations over a 10-month timescale has been made with tentative evidence presented for tiny-scale-structure variations of the order of ~ 3 AU. The behaviour of the diffuse bands with respect to each other and with atomic and molecular data from Pan *et al.* (*ApJS*, **151**, 313, 2004) has been analysed; the results presented here conflict, in some cases, with established diffuse-band-family classifications.

An investigation of diffuse-band strengths in a selection of redshifted (z < ~ 0·5) quasar-absorption lines of sight has been made. Using the relationship between $E(B-V)$ and diffuse-band strength in the Galaxy and the Magellanic Clouds, an attempt has been made to detect the diffuse-band carriers at cosmological distances. Preliminary results are presented for five absorbers with a non-detection in all cases. Early analysis suggests that the diffuse-band strengths are significantly lower than predicted, suggesting that local diffuse-band correlations may not be universal.

A search of infrared catalogues has been made for evolved stars with dusty discs. Prompted by the discovery of a selection of exotic evolved stars (Lloyd Evans, *Ap & SpSci*, **251**, 239, 1997), an automated search of the *IRAS* and 2MASS catalogues has been made based upon near-IR and mid-IR colours. By refining the selection criteria used and making use of the digitized-sky-survey images available, the success rate for detecting unusual stars has been significantly improved. Spectra have been recorded for over 90 targets; a summary of the stars and examples of their spectra are presented. — *University of Nottingham; accepted 2006 November.*

The full thesis is available electronically at: http://arfon.org/thesis/

Studies in Lunar Geology and Geochemistry Using Sample Analysis and Remote-Sensing Measurements

By Katherine Joy

This thesis reports the results of an investigation into the geochemistry and petrology of lunar meteorites, and synthesizes this knowledge with research into the calibration and interpretation of lunar X-ray spectroscopy from the *D-CIXS* instrument.

I present the mineralogy, bulk composition, and petrography of two lunar regolith breccia meteorites (DaG 400 and MET 01210), and a launch-paired group of mare basalt meteorites (LAP 02205/02224/02226/02436/03632).

Individual sample geochemistry is interpreted and geological models proposed to account for the meteorites' formation histories and subsequent impact-related processes. These are compared to previously studied Apollo, Luna, and meteorite lunar samples in order to understand how these new samples fit within the context of existing theories of lunar evolution. I have also utilized currently available geochemical remote-sensing datasets to try and constrain possible meteorite-launch localities, thus relating the microscopic perspective of lunar geological processes from the sample collection back to the 'big-picture' of global remotely-sensed datasets.

I review the scientific findings of the UK-built *D-CIXS* X-ray spectrometer, which flew to the Moon on the *SMART-1* mission between 2003 and 2006. An overview of the instrument is presented and I discuss various hardware and software problems that the instrument encountered. Results of laboratory calibration work and of theoretical X-ray-fluorescence modelling are also presented. This thesis introduces the first detailed examination and interpretation of *D-CIXS* data recorded by the instrument during lunar-science-phase activities in 2005. These datasets focus on X-ray flux recorded during periods of strong solar activity (*i.e.*, solar-flare-associated events), with particular attention to an observation of the lunar far-side feldspathic highlands and the South Pole–Aitken Basin, which records an X-ray flux difference between the two lithological terrains indicative of a marked variation in surface composition.

This work will help to lay the groundwork for understanding and interpreting data from the new *C1XS* instrument, which will fly to the Moon in 2008 aboard the Indian *Chandrayaan-1* mission. — *University College London; accepted 2007 May*.

Variation of Earth's Rotation Rate on Seasonal and Quasi-Biennial Time Scales

By Li-hua Ma

Earth's variable rotation, indicating the whole state of the Earth system, reflects interaction among the Earth's fluid layers, including the solid Earth, the atmosphere, and the oceans. Observational research on Earth's rotation has been an important topic in astronomy and geosciences since its non-uniformity was discovered. With high-precision geodetic techniques from space now giving routine observations of the Earth's variable rotation, coupled with advanced data-processing methods, an increasingly deep understanding of Earth's rotation is developing.

In this work, Earth-rotation-parameter series released by the International Earth Rotation and Reference Systems Service (IERS) and atmospheric-angular-momentum (AAM) series released by the National Center for Environmental Prediction/National Center for Atmospheric Research (NCEP/NCAR), together with the southern oscillation index and zonal winds in the equatorial stratosphere, have been studied; emphasis has been placed on analyzing variable characteristics of seasonal change and the quasi-biennial oscillation (QBO) of the Earth's rotation rate (length-of-day: LOD) and their related atmospheric effects.

Preliminary studies of phase relationships among LOD change, axial AAM, southern oscillation, and possible origin of atmospheric QBO are presented. Results show that changes in the semi-annual, annual, and quasi-biennial components of the LOD have obvious characteristics of time-variation: their periodic length and amplitude change with time, and quantitative results of the time-variation in about the last 40 years have been obtained. Such time-variation also exists in the axial component of AAM. Meanwhile, LOD changes on these time scales can be adequately explained by the variation of the axial AAM. El Niño/Southern Oscillation (ENSO) events are centred near quasi-biennial and quasi-four years in LOD change, southern oscillation, and AAM, and consistency of phase exists in these phenomena. Observations of zonal winds in the equatorial stratosphere have been analyzed, and it is found that the variable characteristics of the periodic length of the QBO fluctuations in zonal winds are coincident at every pressure level, while the fluctuation amplitude of the QBO becomes weaker in going from a high to a low pressure level. This possibly results from energy dissipation during the propagation of fluctuations, and strengthens the conclusion that the atmospheric QBO signals come from outer space. — *Graduate University of the Chinese Academy of Sciences; accepted 2006 June.*

OBITUARIES

Michael John Seaton (1923–2007)

Mike Seaton was an unconventional scientist who took an unconventional path into academia. Already taking an intellectually-inspired view of the world in his youth, he joined the Young Communist League in 1938 as a stand against fascism; political activities led to trouble with the police, and thence to expulsion from school. We are lucky that he was still allowed to matriculate, or we may have lost one of the most brilliant, least-assuming physicists of the 20th Century to more mundane pursuits. As it was, his progress to higher education was interrupted by World War II, where he served as a navigator in RAF Bomber Command until 1945. He then came to UCL to study for a BSc; this was followed by a PhD, a staff appointment, and eventually a Professorial Chair.

His first two papers, written in 1949, presented quantum-mechanical calculations of continuous opacities (he would recall how he wore out the elbow of his jacket cranking the handle of a Brunsviga mechanical calculating machine — the computer of its day — in this work); by publishing in *Monthly Notices* he signalled his early awareness of the astrophysical significance of such studies, and he quickly went on to publish papers on the chemical composition of interstellar gas, and on electron densities and temperatures in planetary nebulae. This set the theme for all his subsequent work: the production of the highest-quality atomic data, and their application to astrophysical problems. Although much of his effort went into sophisticated quantum-mechanical calculations, he was amused that his most cited paper is a very simple, short paper in observational astronomy ('Interstellar extinction in the UV'; *MNRAS*, **187**, 73P, 1979).

Perhaps the most strikingly unconventional aspect of Mike's career was the purity of his academic aspirations; never personally ambitious, he eschewed the politics of empire building and grant funding, other than in the direct pursuit of resources to further his research. What always motivated him was the satisfaction of working out the answers; this was particularly evident in his embrace of digital computing, where he not only ensured that the best available hardware was used, but also personally wrote elaborate FORTRAN codes that continue in use today. He attracted many excellent research students, and was generous both as a supervisor and a collaborator, but he always found great reward in doing the work himself; of almost 300 publications, many are single-author works.

He was, nonetheless, a good team player, as exemplified by the Opacity Project, which he led from 1982 (and aspects of which he continued to work on for the remainder of his life). A long-standing problem in stellar astrophysics had been discrepancies between Cepheid masses and luminosities determined from evolutionary models and from pulsation properties. Initially a minor niggle, this was becoming a major issue as cosmological distance-scale determinations, critically reliant on Cepheid calibrations, became increasingly precise. Numerical experiments by Simon showed that revisions to opacities held the key to this problem, whereupon Seaton and colleagues initiated a successful programme for the necessary wholesale production of vast quantities of high-quality atomic data (not to mention revised equation-of-state thermodynamics, and issues of data storage and promulgation). These have not only resolved the Cepheid problem, but have also cast light on many other issues in stellar pulsation, structure, and evolution.

On Mike's formal retirement at the end of 1988, a celebratory party was held at UCL; presented with a retirement gift, he confessed to feeling rather a fraud, as he had no intention of diminishing his research efforts. Indeed, for the next decade, he continued to work on a full-time basis. To escape from the temptations of the office, he eventually moved to the village of Bwlch, in Powys, but this was not to slow down research; he bought a state-of-the-art VAX computer (subsequently upgrading to a linux system) and set himself up as, essentially, a self-contained *Starlink* node, in the days before reliable broad-band internet connectivity. He continued to work vigorously right up to the end, discussing completion and publication of unfinished work with a delegation of colleagues who visited him a few weeks before his death, of kidney failure; his most recent publication in *MNRAS* was accepted at around the same time.

Mike Seaton was born on 1923 January 16, and died on 2007 May 29. He was elected a Fellow of the Royal Society in 1967, and was awarded its Hughes Medal in 1992 for his theoretical research in atomic physics and for his rôle in the Opacity Project. He was President of the RAS 1979–1981, and received its Gold Medal in 1983. His first wife, Olive Singleton, with whom he had a son and daughter, died in 1958; he later married Joy Balchin, with whom he had a second son. He requested an ecological burial, and the funeral was a simple family service at a green burial site; a celebration of his life in science is planned for later in the year. — IAN HOWARTH.

Bernard Ephraim Julius Pagel (1930–2007)

Early on 2007 July 14 British science lost one of its foremost astrophysicists when Bernard Pagel died after a short fight against cancer. I first encountered Bernard when I attended the Student Vacation Course at the Royal Greenwich Observatory in 1967, in the halcyon days when that institution was based at Herstmonceux Castle. It was clear from the lectures he gave to the course that he had a powerful insight into many areas of astrophysics, and it certainly encouraged me to turn to him as my D.Phil. supervisor when, in 1969, I returned from a year at the Lick Observatory with a large quantity of Am-star spectra (collected principally by Peter Conti, who launched my post-graduate studies). Bernard proved to be an admirable mentor, allowing me to plough my own furrow but always being there with impeccable advice when needed.

Bernard Pagel was born in Berlin on 1930 January 4, arrived with his parents in England in 1933 and, after attending Merchant Taylors School, entered the world of professional astronomy at the start of the second half of the 20th Century in Cambridge, having gained a BA in 1950 at Sidney Sussex College; this was followed by an MA in 1954 and a PhD in 1955. His early work was on the solar spectrum, with his first paper (according to the ADS) on 'Centre-limb variations in the equivalent widths of some infra-red Fraunhofer lines' (*MNRAS*, **115**, 493, 1955) — the overture to a prodigious record of publications spanning over half a century and numbering almost 300.

From the Sun to other stars, particularly — but by no means exclusively — late-type stars and especially those of interesting chemical composition, Bernard made many important contributions relating to abundances, relying on insight and sound physics rather than computer modelling. This work flourished at Herstmonceux, where he came to work in 1956, and a delightful account of his time there may be found on the website of the RGO Society[1]. It is still easy to conjure up the sight of Bernard and other 'pundits', such as the late Michael Penston or Donald Lynden-Bell, taking a post-lunch walk in the castle grounds, deep in astronomical discussion. During this time he was, for a while (1961–62), Senior Editor of this *Magazine*. Another important facet of his time at Herstmonceux was the input he made to the success of the nascent Astronomy Centre and its MSc programme at the University of Sussex (as, indeed, did a number of the RGO staff).

Although Bernard had an encyclopaedic knowledge of stars — helped by his remarkable card-index system, regularly updated by visits to the library where new facts would be added in his unmistakable tiny handwriting — his motivation was understanding the chemical evolution of the Universe, and in the company of a host of visitors to Herstmonceux his research extended out to nebulae (H II regions) and galaxies in pursuit of the 'big picture'. This progress is encapsulated in his excellent textbook, *Nucleosynthesis and Chemical Evolution of Galaxies* (CUP, 1997), which I enthusiastically welcomed in these pages (**118**, 314, 1998). It is good to know that a second edition will be brought to fruition by his long-time collaborator, Mike Edmunds.

When RGO moved to Cambridge in 1990 (prior to its eventual closure), Bernard retired and took up a professorial post at NORDITA in Denmark, which he held until 1999, when he returned to the University of Sussex to become again a Visiting Professor. His return to the British astronomical scene was most welcome, and, a true polymath, he could often be found at RAS meetings posing questions to a wide range of speakers. Throughout, he was a regular (and prompt!) book reviewer for this *Magazine*, and, as a true internationalist, an occasional translator of French and German books into English. He received the Gold Medal of the RAS in 1990 and became a Fellow of the Royal Society in 1992.

Bernard is survived by his wife Annabel, daughter Celia, and sons David and Jonathan and by his many friends and collaborators. His presence on the astronomical scene will be sorely missed. — DAVID STICKLAND.

References

(1) http://homepage.ntlworld.com/rogerwood/RGOS/news007.htm

EDITORIAL

After three years of constant prices, the Editors have regrettably been obliged to make some upward revision of prices for 2008. Modest inflationary pressures and the (satisfying) continuation of page numbers in excess of 400, although contributory, are not the main culprits on this occasion; they have been joined by a marked change in the exchange rate between the US dollar and the pound sterling (which affected the revenue from our many subscribers in the USA) together with a significant rise in the cost of postage, in part occasioned by a need to secure a better method of delivery for customers overseas. (For a description of those problems, see **125**, 284; **126**, 308.)

So, for 2008 (Volume 128), the cost will be £70 or US$140 for institutional subscribers and £15 or US$30 for individuals who undertake not to donate or resell their copies to libraries. The cost of airmail delivery will rise to £7 or US$14. It is hoped that this pricing structure will obviate the need for further revision for several years.

Here and There

A PERTURBING STORY

Alpha Centauri A and Alpha Centauri B make up a stable pair. They never approach each other more closely than eleven Earth–Sun distances, less than the distance that Uranus is from the Sun. Therefore planets around either star would not be disturbed by the gravitational field of the other. — *Astronomy Now*, 2006 October, p. 21.

ONE WAY TO BEAT THE SEEING

Sitting on Cerro Paranal in Chile, 2635 kilometres above sea level, [the VLT] is located in one of the driest places on the Earth … — *Astronomy Now*, 2006 November, p. 26.

KEEPING EVERYONE IN THE DARK

The week-long study [to count Orion's stars] is being organised by the Campaign to Protect Rural England and the British Astrological Association's Campaign for Dark Skies. — *The Daily Telegraph*, 2007 January 12, p. 5.

ENERGY-TO-MATTER CONVERSION

Optical and infrared analyses show that only about 2 per cent of [the fossil cluster J1216+2315] is in the form of stars in galaxies, and 15 per cent in the form of hot gas-emitting X-rays. — PPARC's *Frontiers*, no. 25 (Autumn 2006), p. 10.

A LEISURELY MISSION

[*COROT*] will travel 85,471 miles in two and a half years, orbiting the Earth three times. — *The Daily Telegraph*, 2006 December 28, p. 19.

AND SOME

Travelling at 186,281 miles per second [a spaceship with a sail] could reach a nearby star, Alpha Centauri, in 40 years. — *The Daily Telegraph*, 2006 December 28, p. 19.

FOR STUDENTS OF COSMOLOGY

In these straightened times, work on a research topic … is an opportunity that motivated undergraduates would jump at. — *A & G*, 2006 December, inside back cover.

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NOTES TO CONTRIBUTORS

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