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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2012 January 13 at 16^{h} 00^{m}
in the Geological Society Lecture Theatre, Burlington House

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

The President. I have a very difficult duty now to perform. I'm afraid I have the sad responsibility of announcing the death of Professor Steve Rawlings, who was a very close colleague and friend of mine in Oxford. Steve died on Wednesday night. He was an inspirational figure in our Department and led it for five years recently. Much of what happens in Oxford Astrophysics owes itself to Steve's initiative and energy. Probably more importantly, he was the soul of the astrophysics group in Oxford — he had an inclusive leadership style and personal warmth that permeated the whole department. He was a supportive, positive, fair, and fun person, and he engendered those values in all those that interacted with him. He was an outstanding research leader, and an immense figure in world radio astronomy. Much will be said about Steve in the days and the weeks ahead. Today, our thoughts are with his wife Linda, his parents, and his two sisters. Each of us will have his own special memories of Steve. I would like you to stand now and reflect on those for a moment. Thank you.

I have another important duty today, which is related to the Executive Secretary of the RAS. I have to announce that David Elliott is retiring this year as Executive Secretary. David has made a huge contribution to the development of the Society, and there will be an opportunity later to thank him properly. We are now seeking a worthy successor, which is going to be very difficult.

Moving on now to the 2012 RAS awards. I have a list of awards to announce, and this is one of the President's most enjoyable duties, in contrast to my earlier remarks. We have some wonderful scientists to recognize: the Gold Medals of the Society this year go to Professor Andy Fabian from Cambridge, and Professor John Brown from Glasgow; the Chapman Medal goes to Professor Andrew Fazarkerley from UCL, and the Herschel Medal to Professor Mike Irwin from Cambridge; the Jackson-Gwilt Medal goes to Professor Joss Bland-Hawthorn from the University of Sydney. Now for the early-career awards: the Fowler Awards go to Dr. Hiranya Peiris from UCL, and Dr. Matthew Owens from Reading;

the Winton Capital Awards go to Dr. Tom Kitching from Edinburgh, who is of course one of our speakers today, and Dr. Juliet Biggs from Bristol. The Group Achievement Award was given to the UKIDSS survey team, and the people that we viewed as contacts for that are Professor Steve Warren from Imperial College and Professor Andy Lawrence from Edinburgh. The award for Service to Astronomy goes to Professor Paul Murdin. Honorary fellowships this year are awarded to Dr. Karl Menten from the Max-Planck Society, Professor Hiromoto Shibabashi from the University of Tokyo, Professor Robert Williams from the Space Telescope Science Institute in Baltimore, Professor Robert Lin from the University of California at Berkeley, and Professor Hermann Opgenoorth from the Swedish Institute of Space Physics.

On to the lectures: the George Darwin Lecturer will be Professor Andrew Collier-Cameron from St. Andrews, the Harold Jeffreys Lecturer, Professor Bill Chaplin from Birmingham, and the Gerald Whitrow Lecturer, the lecture to be given in 2013, will be Professor Andrew Liddle from Sussex. I'd like personally to congratulate all of them, and I'm sure we'd all like to do the same. [Applause.]

On now to the normal part of the programme: we have four talks, and the first is by Dr. Ben Davies from Cambridge. Ben Davies is an RAS Research Fellow, and so we are looking forward to hearing him talk about 'Using massive stars to study the chemical evolution of their host galaxies'.

Dr. B. Davies. The fundamental goal of astrophysics is to understand how galaxies such as our own Milky Way came into being: how they were first formed in the early Universe; how they grew by accreting smaller satellite galaxies and by merging with other large galaxies; and how they evolve on their own as huge amounts of gas and stars swirl around their central super-massive black holes.

As galaxies evolve, they form stars — either at a smooth, continuous rate such as in our Galaxy today, or in violent bursts as seen in galaxy mergers. During their lifetimes these stars fuse light elements, such as hydrogen and helium, into heavier ones such as oxygen and iron. Therefore, by studying the relative abundances of certain elements within a galaxy, we can infer the star-forming history of that galaxy.

Indeed, chemical-abundance studies of galaxies have been a powerful tool with which to test the theory of galaxy formation within the current favoured cosmological model of the Universe. These models make predictions of, for example, how a galaxy's chemical abundances should be related to its total stellar mass, which can then be tested quantitatively by observations. Unfortunately, in the last few years it has become apparent that the usual way in which chemical abundances are determined — by measuring the strengths of bright emission lines from ionized gas located within the galaxy — is highly problematic. This results in large systematic uncertainties in the data, and means that powerful diagnostic information on the evolution of galaxies is lost.

To address this problem I am currently developing a new way in which to measure the chemical abundances of galaxies — by observing a galaxy's most luminous stars. These stars are around 20 times more massive and up to 500 000 times brighter than our Sun, and can be observed at distances of many megaparsecs by using large ground-based telescopes. The revolutionary aspect of this new method is that it requires much lower exposure times for a given star's brightness than was previously thought possible. This then means that, for a given exposure time, one can push out to much larger distances — in fact, using the *Very Large Telescope* in Chile with this method, one can measure the chemical abundances of single stars out to distances of over four times the distance to the Andromeda galaxy!

By analysing the light from those stars, and by comparing to sophisticated state-of-the-art models, I can then determine their chemical make-up. Since they are only a few million years old, a blink of an eye in terms of the age of the host galaxy, they provide an accurate indicator of the galaxy's chemical abundances at the location of the star observed. Then, as there are many thousands of such stars in a galaxy such as ours at any one time, those stars can be used to make a two-dimensional chemical 'map' of the host galaxy.

I have studied our Galaxy and the two Magellanic Clouds in this way, and will eventually expect to provide a census of the galactic chemical evolution in the local Universe.

Professor I. W. Roxburgh. You are using the stellar spectra to get abundances, but I'm reminded of a flippant comment by Ray Lyttleton many years ago, that if you think you can determine the internal composition of stars by looking at their surface layers, you might just as well think a chimney sweep is made of solid carbon! But it's a serious issue, because there are two things happening: there's mixing internally, these are massive stars that are rapidly rotating, and the mixing can change the composition; and there's also diffusion and gravitational settling in the surface layers. So, how confident are you that measuring the surface abundances is actually giving you a true measure of the composition?

Dr. Davies. That's a fair point. It is true if you are looking in the *H* band, where you see a lot of molecular lines and there you are sensitive to changes in the C, N, and O abundances; and C, N, and O are changed by stellar nucleosynthesis in red supergiants. As you said, they rotate, but they also have deep convection zones, so anything that happens lower down will show its face at the surface as well. In this project we are not interested in C, N, and O, which will tell us about stellar evolution and stellar structure, and nucleosynthesis in stars; we're dealing with the atomic, metallic lines like iron, silicon, and magnesium, which are not really affected by the nucleosynthesis of red super-giants. For this reason, we can be confident that the abundances of these elements are uniform throughout the star.

A Fellow. May I ask a rather simple-minded question? Why study the stars after they have gone red giant, and you've got all these horrible molecular lines in the spectrum — why not look at the same stars when they're still on the main sequence? The spectrum is much simpler!

Dr. Davies. They're much brighter as red supergiants and they're easier to find, especially in the infrared. If you look at a galaxy in the optical, you see blue stars and red stars, the hot massive stars and the cool massive stars. If you look at that galaxy in the infrared, you see bright stars and faint stars. The bright ones are the red supergiants, because they are so much brighter than anything else. This means that they can be observed at much greater distances, which is crucial for extragalactic work.

The President. Ben was amongst the first tranche of recipients of RAS Research Fellowships. We do need to try and carry this programme forward, and the Society has only limited means to do this; hence we are looking for other donors to help us in this activity. So if anybody in the room has ideas about how we might raise some money to continue to support excellent young scientists like Ben, and Tom Kitching, who is going to be the next speaker, please get in touch with the Treasurer, or me. Let's thank Ben again. [Applause.]

Tom Kitching was also in the first tranche of RAS Research Fellows and, as you've heard, from my remarks earlier, we clearly chose well because he is now the Winton Capital Prize winner for this year; also, he is now sponsored by the Royal Society, and is a University Research Fellow of the Royal Society, based in Edinburgh. Tom's topic is 'The dark-universe cosmology with gravitational lensing'.

Dr. T. Kitching. The Universe is dominated by two components whose nature is entirely unknown. Dark matter accounts for approximately 26% of the mass–energy of the Universe, and dark energy 70%. Simulations predict that dark matter should form in hierarchical structures that form a filamentary cosmic web, but only now has dark matter been mapped on a large scale with data. By using the technique of gravitational lensing, whereby the light from galaxies is slightly distorted by the effect that the intervening dark-matter web has on space–time, we can map the dark-matter distribution on cosmic scales. However, in the weak-gravitational-field limit that is applicable for the lines of sight to most galaxies, the distortion that occurs is a small change in the ellipticity of a galaxy's image, approximately a 1% effect — a change far too small to be discernible with the human eye. Furthermore, galaxies are convolved with a spatially varying point-spread function induced by the atmosphere and the telescope optics, and are observed using CCDs that pixellate the images. This makes the task of mapping dark matter particularly challenging.

The largest deep optical-imaging survey observed that has the resolution required for lensing measurements is the CFHTLenS survey that covers 155 square degrees of sky with a limiting magnitude of 24·7 (in the *r* band, 7-σ extended source). The results analyzed in 2006 were found to have significant systematic effects as a function of depth and position and so over the past five years a team of over 20 scientists has reanalyzed these data with an unprecedented accuracy. All parts of the data processing were redesigned, from the raw-image reduction to the way that the gravitational lensing was measured (using a technique called LENSFIT), and the way the redshifts of galaxies were inferred. Systematic tests in the redesigned analysis were strictly cosmology-independent such that an effect called confirmation bias, where results can be inadvertently tuned to match expectations, was removed. Now that this analysis is complete the final gravitational-lensing science can be done with a confidence that the data are free of systematic effects to the level required. The team has now produced the largest dark-matter map of the Universe ever constructed, revealing the way dark matter and galaxies trace each other across cosmic scales.

Gravitational-lensing data can also be used to measure the nature of gravity, and possible deviations from Einstein's General Relativity. Using the CFHTLenS data and a spectroscopic galaxy-redshift survey, WiggleZ, a change in the observed time-dilation effect from that predicted by General Relativity and a change in the spatial curvature induced by gravity from that predicted by General Relativity can be inferred. Previous measurements of such effects were degenerate, in that neither effect could unambiguously be measured. By combining the lensing data and the galaxy-redshift data the CFHTLenS team have jointly measured these effects — the most stringent test of Einstein's General Relativity on cosmic scales to date. Measurements indicate that General Relativity remains the best theory of gravity to explain the data, but with many theories of gravity available, some of which naturally explain the observed dominance of dark energy in the Universe, next-generation experiments have the capability to provide decisive evidence for a new theory of gravity.

Mr. M. Hepburn. Am I right in saying that you've re-done the redshifts for something like 10 million galaxies?

Dr. Kitching. Yes, it's more than that, it's about 30 million galaxies.

Mr. Hepburn. That's a hell of a lot of work, isn't it? And is that only 0·3% of the sky that you measured?

Dr. Kitching. That's 155 square degrees, yes. With *Euclid*, say, we will have 1·5 billion galaxies, and the data analysis really becomes an issue that needs to be sorted out. We'll have about ten petabytes of data that we have to analyse. Yes, it's challenging!

Mr. N. Calder. This idea that you can get the shape of something small on one pixel, is remarkable. How you would explain it, say, to a 6th former?

Dr. Kitching. You only have statistically to get the shapes right. On an individual object-by-object basis you can get things wrong, but as long as it's statistically right, you're OK. While the galaxies are smaller than a pixel, they usually don't quite lie on the exact centre of the pixel, and most of the time they're slightly offset. So what you usually see is two or three pixels with light in them. And then you have to regularize the problem somehow — you have to put in information; and if you do that incorrectly, it's garbage in, garbage out. But the advance that we made was to use a Bayesian method, where you can control what you put in and we know exactly the models that we're putting in. And that means that even if you only have three data points, which are the three pixels, then you can still get something meaningful out of it.

Mr. Calder. So crucially, it's that you get hints from neighbouring pixels?

Dr. Kitching. Yes, because it's very unlikely for the image to land exactly on the centre of the pixel. It does happen, but on average it's slightly offset.

A Fellow. Related to that, you've talked about the new method that you and Lance Miller were developing for improving the estimation of ellipticity. There are a number of future systems which you have talked about, including the *Euclid* space mission. What interests me is squeezing the best possible performance out of those that we can. Has your method actually been applied to predicting the performance of those future systems, including *Euclid*?

Dr. Kitching. Yes, I actually coordinate something called GREAT10, which is a series of experiments where we simulate about a terabyte of data, and it's a blind challenge. We inject a true underlying distribution of ellipticities and then people apply their different methods to try and recover that, and we grade them on how well they do. Some of those methods do well enough for *Euclid* data; LENSFIT, in fact, doesn't. *Euclid* will have very highly resolved galaxies — you'll resolve spiral arms — so this type of simple model-fitting method won't actually work. But yes, there is a programme under way to test things in that régime.

Professor P. Coles. Your dark-matter correlation function matches the Λ-CDM predictions pretty well; you've got the parameters from there. But other than that, what surprised me about the map is that the morphology looks slightly different from what you would naïvely expect from Λ-CDM, and the fact that it's less filamentary and more blobby to the eye. So have you looked at higher-order descriptors of that, to see whether they match Λ-CDM as well?

Dr. Kitching. We are looking at higher-order descriptors, yes, like looking at bi-spectrum and galaxy–galaxy–galaxy lensing. We have noticed the same thing, that actually the dark-matter map does look slightly different than what you would expect. There are some people in the team who want to do a topographic analysis of that as well, which should be out in the next year or so. But I think there are some weird things which are not quite as we expect. And I've pointed one out: that there are some dark-matter features which don't have a strong galaxy over-density associated with them, which should be particularly interesting, I think.

Professor D. Lynden-Bell. Many years ago, Zel'dovich pointed out that if you took a column through the Universe to a supernova that was being used, say, to measure the dark energy or whatever, that if it was actually empty then you would not get the averaged gravitational lensing that is put into the Friedmann–Robertson–Walker metric, which has a smoothed-out distribution of matter, because the matter would not actually be in that column. And as a result of this, and the fact that that particular calculation missed out the effect of shear, which also produces gravitational lensing, the question or issue is, can you use your results to predict the increased scatter in magnitude of the supernova data that you would expect from the lensing due to such things as you are encountering? Can you tell how much intrinsic scatter due to brightening by gravitational lensing, on average, will there be in the supernova data?

Dr. Kitching. Yes, you can: there is also the Supernova Legacy Survey, which is being done with the *CFHT*, and yes, you can do that exact same thing. And in fact, we're talking to the Supernova Legacy people as well and how can they use our data to help them with their supernova studies.

Professor Lynden-Bell. Do you know how big it is?

Dr. Kitching. Not off the top of my head, no. Supernovae are quite sparsely sampled. If you have a galaxy right next to one that is sheared then that gives you more information than if it's a long way away.

The President. Thank you very much, Tom. [Applause.]

Our next speaker is Professor Ellen Stofan from Proxemy Research and UCL, and her topic is 'Exploring the seas of Titan'.

Professor Ellen Stofan. The discovery of lakes and seas near Titan's north pole by the *Cassini* radar confirmed the expectation that liquid hydrocarbons exist on the surface of the haze-shrouded moon. Lakes are also present near the south pole, though are smaller in extent and number. Ethane has been detected in the largest southern lake by the *VIMS* instrument on *Cassini*; ethane and methane are thought to be the principal components of the bodies of liquids. The Titan lakes and seas fill through drainage of subsurface runoff and/or intersection with a subsurface alkanofer. Titan is thus the only body besides Earth with an active condensable-liquid hydrological cycle. The nature of Titan's methane cycle, along with the prebiotic chemistry and implications for habitability, make the seas of the highest scientific priority for *in-situ* investigation.

Ligeia Mare is the second largest sea on Titan, approximately 420 by 350 km, with an area of $\sim 126\,000$ km^2, and in excess of 2000 km of coastline. Seasonal evaporation of methane on Titan is likely to lead to either beach-plain/mudflat shorelines as methane evaporates. This type of shoreline is seen at Ontario Lacus near the south pole of Titan, as well as a possibly wave-modified delta. Waves are likely on Titan, but only when wind speeds exceed 1 m s^{-1}.

The *Titan Mare Explorer* mission is an ASRG (Advanced Stirling Radioisotope Generator)-powered mission to Ligeia Mare on Titan that has been selected for Phase-A study under NASA's Discovery Programme. The mission would be the first exploration of a planetary sea beyond Earth, would demonstrate the ASRG both in deep space and a non-terrestrial-atmosphere environment, and pioneer low-cost outer-planet missions. The scientific objectives of the mission are to (*i*) determine the chemistry of a Titan sea to constrain Titan's methane cycle and its prebiotic chemistry; (*ii*) determine the depth of a Titan sea; (*iii*) characterize physical properties of liquids; (*iv*) determine how the local meteorology over the seas ties to the global cycling of methane; and (*v*) analyse the morphology of sea surfaces and, if possible, shorelines.

Mr. M. F. Osmaston. You're talking about hydrocarbons, and you showed pentane as having been detected. Does the detection fade out because of the state of the higher hydrocarbons being solid and not amenable to being detected, or can you rule out higher hydrocarbons being present?

Professor Stofan. The spectrometers that are on-board *Cassini*, the *CIRS* instrument primarily, which is looking at the atmospheric composition, doesn't have the ability to measure exactly what some of the more complex hydrocarbons in the atmosphere are. And certainly the *VIMS* instrument that can get some spectral information of the surface has a really hard time separating the atmosphere out, which is needed to understand the signal one is getting from the surface. So we don't really know what those higher-order hydrocarbons are; we know they are there, but we don't know what they are. The mass spectrometer on *Titan Mare Explorer* would go out to about 500 Daltons, so we hope that we would be able to find out what some of the heavier hydrocarbons actually are.

Professor F. Taylor. Can I ask a question about the current understanding of the total methane budget? You've pointed out that before *Cassini* got there, everybody expected there'd be substantial oceans, and people arguing for that made a very good case. Then *Cassini* got there and found lakes, which is a lot less liquid; and on some of your diagrams, you have a nice big aquifer near the surface where presumably you are draining an excess off, because something like that has to be happening. Then you pointed out also something that we understand quite well, which is that there has to be a supply of methane into the atmosphere, because there is quite a lot there and we understand the chemistry that's destroying it; so you've got another aquifer pumping stuff in. Does anybody have a good model of how these things are related to each other? Are they communicating, and is there methane circulating? What can be observed?

Professor Stofan. If we can get the lake lander on the surface, we'll be able to measure what percentage of the sea is methane as opposed to ethane, and then ask where is the rest of the methane that has to be there to make the rest of the story work? Modellers currently tend to put the methane they can't find, but want for their model, into the crust. So there are multiple models, and they don't have any problems putting the methane where they like, but it's not clear to me that we actually have the data to say that one model is better than another, or what is the actual methane budget. I don't think we're there yet, we need more data, and right now, we just don't have enough actual data. We can make the measurements of the atmosphere, but we don't have any idea of what's going on on the surface, and we really have no idea of what's going on in the subsurface. I've been reviewing a paper that has some really interesting implications about where there could be some more methane, so I feel as if every day the story changes a bit, and gets more interesting. Even with my mission, I'd love to be able to say that we're going to be able to make inferences about the subsurface methane table, and how much is down there, but we really can't.

Mr. Hepburn. In your diagram showing the chemical relations between methane and all the other species, there is not a single oxygen-containing species, and yet presumably oxygen is much more abundant in Titan as a whole, than either carbon or nitrogen. Have you any comments on that?

Professor Stofan. The oxygen is all in the water ice; there is oxygen, but that's where it is. It's all locked up in the water ice, as there is none in the atmosphere.

Mr. Hepburn. But I would have thought that if you are postulating all these complex inorganic compounds, very highly reduced, that most of them are, particularly HCN, which seems to be much the most likely thing to form in a dynamic, ultraviolet-affected atmosphere, then you would incorporate oxygen into these organic species. It seems to me that you have shut off any consideration of a large number of possible species, which are certainly known in interstellar space.

Professor Stofan. Yes, I don't know the answer to that — good point.

The President. Thank you very much again, Ellen. [Applause.] Actually, I wanted to thank you particularly for coming at a critical time in your Phase-A study, so thank you for coming and telling us about your mission.

The final speaker today is Lee Macdonald, from Cambridge, and he is going to tell us about 'From cracked mirror to Nobel Prize: the *Isaac Newton Telescope*, 1944–2011'.

Mr. L. Macdonald. The 2·5-metre *Isaac Newton Telescope* (*INT*) has been a huge success since it opened on La Palma in 1984. But its earlier career at Herstmonceux (1967–79) has long since passed into the folklore of modern astronomy as an expensive failure. The telescope took 21 years to build and Herstmonceux soon proved to be a terrible site for so large an instrument. Two major questions clamour to be asked about the construction of the *INT*: why did it take so long to build, and why was it built at Herstmonceux? And what, you may ask, has a cracked mirror to do with a Nobel Prize?

Why astronomers wanted a large telescope in the years after the Second World War is nicely summed up in a 1936 comment by Edwin Hubble: "The conquest of the Realm of the Nebulae is an achievement of great telescopes." Since 1900 astronomy's increasing emphasis on astrophysics, and particularly Hubble's discoveries that spiral nebulae are distant galaxies and that those galaxies are receding from us, had led to a demand for more and more light and thus bigger telescopes. When in 1944 the Royal Society formed a committee to discuss the post-war needs of British astronomy, one of its principal recommendations was a large telescope.

On this committee was RAS President Harry Plaskett, and in his 1946 February Presidential Address he argued in some detail for a British 74-inch telescope. Plaskett believed that the telescope had to be sited in England, on the grounds that only if it were nearby could theoretical and observational astronomers collaborate effectively. As regards England's climate, he simply cited the work of William Herschel and others as examples of what could be done with large telescopes in this country. In fact, however, Herschel rarely observed with his famous 48-inch telescope, precisely because the seeing conditions at Slough were seldom good enough to justify its use.

Plaskett's proposals received an enthusiastic response. In the 1940s it was possible to fund a major scientific project by applying directly to the Treasury through the Royal Society, and a 'telescope committee' was quickly formed for that purpose. In the spring of 1946 this committee decided to build the new telescope at Herstmonceux Castle, where the Royal Observatory was shortly to be moved from Greenwich. The committee also settled on a name for the telescope. Celebrations of the tercentenary of Isaac Newton's birth in 1943 had been deferred to 1946 because of the war, and an international conference on Newton was being organized by the Royal Society for July of that year. The telescope committee agreed that if it named the telescope after Newton, it might be possible to obtain funding in time for the project to be announced at the tercentenary conference. This meant getting the proposal approved by the RAS Council, then the Royal Society, and then sent to the Treasury — all in time for July. An RAS discussion meeting on the telescope in 1946 June was held long after the proposal had been sent to the Royal Society and allowed no time for a democratic debate on its wisdom. The science-conscious post-war Labour government quickly approved funding for the project and it was duly announced at the Isaac Newton conference — a mere five months after Plaskett had first publicly aired the proposal. The size of the proposed telescope was increased to 100 inches at the last moment, at least partly for reasons of prestige.

To design the telescope, a 'Board of Management' was set up, chaired by the Astronomer Royal, Sir Harold Spencer Jones, and consisting of Fellows of the Royal Society as well as of the RAS. The Board agreed to employ the glass-maker Pilkington to cast the blank for the 100-inch mirror; Pilkington said they could do the job for £8000 to £10 000. However, in 1948 Spencer Jones received an offer from Michigan University in the USA to donate, free of charge, an unused 98-inch blank. Spencer Jones asked the Treasury whether the Board should accept this offer or honour its 'gentlemen's agreement' with Pilkington. The Treasury's predictable response was: "All that we would say officially would be that it is a pity to spend £10 000 of the taxpayers' hard won money if you can get the same thing for nothing...", and so the donation from Michigan was accepted. The contract for grinding the mirror was awarded to Grubb Parsons, the well-known Newcastle firm which had made many large telescopes. When grinding began, the mirror was found to have many cracks and irregularities, and they became worse as grinding proceeded.

In the meantime, the Board had decided to make the telescope an interchangeable Schmidt and Cassegrain instrument. The idea was to use it as a conventional Cassegrain for spectroscopy of individual objects when the Moon was in the sky and then to take advantage of the wide-field capabilities of the Schmidt design during the dark of the Moon. This duplex design involved making an enormous 80-inch corrector plate — a difficult enough item to make in smaller sizes and never attempted at this size — and carefully inserting it in the telescope whenever the instrument was to be used in Schmidt mode. The project became bogged down in technical difficulties. The well-known aircraft designer Barnes Wallis was called in to design a tube that would stand up to the stringent collimation tolerances required by the duplex design, but produced no practical solution. A corrector plate that was big enough and optically accurate proved hard to obtain. An experimental 80-inch blank was obtained from Pilkington, but was never used in the telescope; apparently it was later used as a coffee table at the RGO in Herstmonceux! And because of the cracks in the glass, the mirror took a long time to grind — though it was finally figured to an *f*/3 spherical figure in 1954. Eventually it was agreed to wait for the arrival of Spencer Jones's successor as Astronomer Royal, Richard Woolley, in 1956 before deciding whether to go ahead with the duplex design.

Woolley has sometimes received flak for building the *INT* in the wrong place, but in fact a 1955 letter from Woolley in the RGO Archives reveals that he believed that "the English 100-inch should be in the Southern Hemisphere where it could be guaranteed to do work of the highest significance...". But his views were rejected by the Board and he realized that he was committed to building the telescope in the UK. However, when he became Astronomer Royal Woolley rejected the duplex design and secured the Board's agreement to make the *INT* a conventional Cassegrain. The mirror was re-ground to a parabolic figure, but because of the cracks it was left at *f*/3. This was the origin of the unusually short *f*-ratio the *INT* has today — in the words of Fred Hoyle, "a little runt of an instrument".

The project was then delayed for a further two years, owing to the weakened state of the British economy after the Suez crisis. A contract to build the telescope was finally placed with Grubb Parsons in 1959 and construction proceeded apace in the early 1960s. In 1960, a conspiracy of Cambridge astronomers tried to halt the project by asking Harry Hinsley (a Cambridge don with contacts high up in the Admiralty) and Enoch Powell MP (later a controversial figure but then a rapidly rising star in British politics) to use their influence. But the Admiralty quickly saw through the conspiracy and it had no effect. The *INT* was finally opened by HM The Queen on 1967 December 1 — a foggy day, so no-one saw anything.

Soon after the telescope went into regular use in 1968, the bad observing conditions in Sussex became apparent and questions were officially asked about moving it. By 1979 it was agreed to make the *INT* part of the new Roque de Los Muchachos Observatory on La Palma in the Canary Islands. It was dismantled at Herstmonceux in 1979 and reopened on La Palma in 1984 February. It was agreed to replace the original, problematic mirror with a new one made of Zerodur, but the same tube and mounting (modified for the lower latitude) were used, so the *INT* remained an *f*/3 telescope.

At *f*/3, the *INT* has an unusually wide field for a 2·5-metre-class telescope. The *INT*'s arrival on La Palma coincided with the start of the CCD era, but CCD detectors were small in their early days, so the wider the field of the telescope, the better. The *INT*'s wide field was helpful to Saul Perlmutter and his team in searching for high-redshift type-Ia supernovae in the 1990s, in the effort to determine the cosmic deceleration parameter. They quickly discovered SN 1992bi, a type-Ia SN at an unprecedented $z = 0{\cdot}458$. This and other discoveries were analysed in detail with larger telescopes and, as is now well-known, instead of a deceleration, they discovered a cosmic acceleration — a discovery for which, along with Brian Schmidt and Adam Riess, Perlmutter was awarded the 2011 Nobel Prize in Physics.

To go back to our original two questions: why did the *INT* take so long to build and why was it built at Herstmonceux? — we can identify four broad reasons. Even within civilian science, the government's priority was prestige, not scientific utility. Spencer Jones, who was busy with many other priorities, was the wrong person to lead the project. The generation of astronomers who conceived the *INT* in the 1940s was not used to travelling overseas for short observing runs, as are today's astronomers, so they preferred the telescope to be in the UK. And what was the alternative to a UK site in 1946? As for putting the *INT* in the Canary Islands, it was then politically out of the question. A country that had just defeated fascist régimes in Germany and Italy was hardly going to build a telescope in General Franco's back garden!

But the *INT* did become a world-class telescope in the end. We should not forget that even at Herstmonceux, it was used by Paul Murdin and Louise Webster to discover the first stellar-mass black hole. The *INT* went on to produce many important results on La Palma, but its essential early rôle in the success of Perlmutter's Supernova Cosmology Project may be its crowning achievement, and its *f*/3 mirror made it especially useful as a wide-field survey instrument — from cracked mirror to Nobel Prize.

The President. I'm sure there will be questions.

Professor Lynden-Bell. I would just like to add a beautiful thing that I heard, which I believe is correct, that the reason why Herstmonceux was chosen was not only that Lady Spencer Jones wished to live in a castle [laughter], which is true, but also that the person who was in charge of the daily Sun exposure in Eastbourne happened to be in league with a local travel agent. And he cooked the books; and you will find that there is a certain circle around Eastbourne where the sunshine is slightly better than the rest of England. [Laughter.] And the result of this was that they hadn't got night-time records of moonshine, but they had got records of sunshine, and there was obviously a good reason for putting it near Eastbourne!

Mr. Hepburn. Couldn't they put it on Ascension? Ascension seems the ideal place, and it was thoroughly British.

Mr. Macdonald. It was thoroughly British, but again, it was a long way off by the standards of 1946. Plaskett wanted people to go down there for a week or something, but to get to Ascension Island, you went there by ship, generally.

The President. Any more questions? I can't resist a couple of comments myself, having worked on telescope programmes. One thing to note is that astronomers still have, I think, more than their fair share of the ear of high officials in the government. That's something that we have on our side; however, we don't do ourselves any good when we argue with each other. The remark is that before hearing your talk, Lee, and even though I used the *INT* on both sites, I had no idea about the *long* period of construction, and the very *short* period for approval. And that of course contrasts very starkly with the current situation, where it takes us typically the best part of a decade to get a project of that scale approved, and a not dissimilar time, shorter usually, to get it constructed. And so, actually, it is a bit of a lesson, that we did perhaps learn something: that if you rush, and make what turn out to be rather silly decisions, when looked at with hindsight, you do end up with folly, and that is unfortunate. It clearly was a very unfortunate 21 years with the construction, and so we have at least learned something from that. So, Lee, thank you very much! [Applause.]

I can now remind you there is a drinks reception in the library, and the Society will meet again, for the next open meeting, on February 10.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2012 February 10 at $16^{h}\ 00^{m}$
in the Geological Society Lecture Theatre, Burlington House

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

The President. Welcome to the Open Meeting of the Society. I have a few announcements before we get the scientific programme underway. First we are going to revise slightly the arrangements at the Annual General Meeting, in May this year, in order to try and encourage larger participation and more efficient running, and also to give ourselves the opportunity to have full-length Specialist Discussion meetings in May. Because we've had the AGM at 2 pm, we've had to truncate the Specialist Discussion meeting and it wasn't very satisfactory. So now we are going to have the normal Specialist Discussion meetings, followed by the Annual General Meeting at 4 pm, for one hour, and then we will have the Presidential Address at 5 pm. The Council believes there is every possibility of doing all the business for the AGM in that hour.

I am very pleased to announce that Dr. Becky Parker, the Head of Physics at Simon Langton Grammar School in Canterbury, is the first recipient of the Society's Patrick Moore Medal, for outstanding contributions to teaching astronomy and geophysics at secondary-school level. Well done Becky Parker! [Applause.]

I have more good news. The RAS Fellowships have just been settled, and I can tell you the three Fellowship awardees: Dr. Leigh Fletcher, from Oxford, Dr. Benjamin Joachimi, from Edinburgh, and Dr. Nick Wright, from the Harvard-Smithsonian Center for Astrophysics, who is going to hold his Fellowship at the University of Hertfordshire. (The others will stay where they are!)

It's now time to move on to the scientific programme, and the first speaker is an RAS Norman Lockyer Fellow, Dr. Adam Christopherson from the University of Nottingham, and his topic is: 'Vorticity generation in cosmology'.

Dr. A. J. Christopherson. Vorticity is important in many areas of physics yet, to date, it has not been studied in much detail in early-Universe cosmology. In this talk I will very briefly review cosmology and present some work I completed during my PhD on vorticity in the early Universe, and suggest future directions in which this can be extended.

First, a review of standard cosmology. The Universe is known to be expanding and well described by the Big Bang model; that is, previously the Universe was hotter and denser and much smaller than it is today, and it has expanded from that state. If we rewind this model back even further, we obtain an initial singularity. This is the mathematics telling us that the model is not valid at this time, and indicates our need for a quantum theory of gravity.

So, while the model is not valid at $t = 0$, we can model the dynamics very well from when the Universe was a fraction of a second old. The Universe expands, going through a period of accelerated expansion, or inflation, driven by a fluid with negative pressure or, in the language of particle physics, a scalar field. As it expands, the Universe cools. When it has cooled enough, the photons decouple from the electrons and are able to stream freely, producing the cosmic microwave background (CMB). This is the furthest back in time we can probe (with light), and observations of the CMB enable us to determine details of the physics of the very early Universe. The Universe then keeps expanding and cooling, and re-ionizes, enabling the first structure to form. Recent observations tell us that the Universe is currently accelerating in its expansion today, attributed to the so-called 'dark energy' — another negative-pressure fluid, or scalar field, but on a different energy scale to inflation.

The standard cosmological model is well tested by the data, specifically from observations of the CMB and type-Ia supernovae, as well as observations of large-scale structure. But how do we build a theoretical model? Well, we know that on sufficiently large scales, the Universe appears to be homogeneous and isotropic. Since the best model of gravity we have at present is Einstein's General Relativity, we use the general homogeneous and isotropic solution to Einstein's field equations: the Friedmann–Lemaître–Robertson–Walker (FLRW) solution.

But this solution is an approximation. Obviously, there exists structure in the Universe, exhibited by galaxies, stars, *etc.*, and there are inhomogeneities as observed in the CMB. So how do we obtain a more realistic model? We adopt a technique that is often used in physics where we have a solution which well approximates the physical system in hand — that is, we use a perturbative technique. Here, we model inhomogeneities by expanding around a homogeneous background solution, the FLRW metric. This is called cosmological perturbation theory. Since General Relativity relates the geometry of space–time to its matter content, perturbations in the metric invoke perturbations in the matter. Thus, for example, we model the energy density as a background quantity that depends only on cosmic time, and a perturbation that depends upon both time and space.

Having now reviewed some standard cosmology, and briefly introduced cosmological perturbation theory, I will now change gear and discuss vorticity.

There are many scenarios in Nature where vorticity plays a rôle. For example, aerodynamics is modelled using fluid dynamics and we can see vortices on the wing tips of an aeroplane during take-off. Vorticity is also important in weather physics, where regions of high vorticity have the potential to form devastating tornadoes. As we expect the early Universe to consist of several fluids, it is natural to think that this is a highly turbulent scenario in which vorticity plays a significant rôle.

If we cast our minds back to an undergraduate fluid-dynamics course, we recall that, in the most simple case where viscosity, other body forces, and gravity are neglected, the evolution of the fluid velocity vector is governed by the Euler equation. One can then simply calculate how the vorticity, mathematically defined as the curl of the fluid velocity or more physically, the circulation per unit area at a point along the fluid flow, evolves. We find that the vorticity is sourced by a term coupling the gradient of the fluid density and the gradient of the pressure — the baroclinic term. For a barotropic fluid this is zero, but if these two vectors are not parallel, equivalent to saying that the fluid equation of state depends upon two independent variables, then it is non-zero. This is a result obtained by Crocco in 1937, who showed that including entropy into a system provides a source term for vorticity.

How do we extend this to our cosmological scenario? We can split the pressure perturbation into a term proportional to the energy-density perturbation, and an entropy perturbation. Like in classical fluid dynamics, if we are considering a barotropic fluid, the latter is zero. But, in order to build a realistic model of the early Universe, which involves more than one fluid, or more than one scalar field, we will have a system with a non-zero entropy perturbation. Then, using General Relativity and energy–momentum conservation, we can arrive at governing equations for the vorticity tensor. (I omit the details for the purpose of this talk.) We find that, when only considering linear perturbations, the vorticity has no source term and decays with the expansion of the Universe, as has been known since the mid-1980s.

However, when we extend the analysis to one order higher in perturbation theory, and consider non-linear combinations, we find that the vorticity has a source, which is similar to that found in classical fluid dynamics: a coupling between gradients of entropy and energy-density perturbations. This novel result was obtained during my PhD and can have important observational consequences.

One such consequence is in the CMB. We know that the CMB photons are polarized into curl-free and divergence-free polarizations, called the E- and B-modes, respectively. When only considering linear perturbations, scalar perturbations only produce E-modes, tensors (gravitational waves) produce B-modes and, while vectors like vorticity can produce both E and B, they decay and are therefore assumed negligible. However, our second-order vorticity will not decay as long as it is sourced, and will produce B-mode polarization. Furthermore, since the B-mode polarization that primordial gravitational waves produce is very small, it could be feasible that second-order B-modes are detected in the not-too-distant future. Or, alternatively, it may extinguish the so-called 'smoking gun' that a detection of B-mode polarization is evidence for primordial gravitational waves. In any case, this will allow us to constrain further the physics of the early Universe.

This is one of the topics I plan to investigate for the duration of my Sir Norman Lockyer Fellowship. I have already made progress into obtaining the source term, through work on entropy perturbations between radiation and matter in the cosmic fluid, with I. A. Brown and K. A. Malik; and work on entropy perturbations in multi-field inflationary models, with I. Huston. Future work involves investigating the amount of vorticity that these sources can produce, as well as investigating the potential for this vorticity to source primordial magnetic fields.

Mr. M. F. Osmaston. You lost me when you were talking about getting vorticity into this stuff. Because nowhere do I see any viscosity mentioned, and if you have two linear motions, the only way they can result in vorticity is by interfering, by having viscosity. Now that's the only way you can get vorticity propagated from one place to another, by having viscosity. So where have we lost it?

Dr. Christopherson. There is a problem when you present this sort of material, drawing from classical mechanics and then putting it across into General Relativity. In order to try and let the audience take something away from it, rather than having lots and lots of equations, you have to simplify things. So, I imagine that the viscosity will be hidden in here somewhere, but ...

The President. Any more questions?

Professor D. Lynden-Bell. Viscosity — you have actually left it out of this equation, so it's not in there, but nevertheless, the thing that I wanted to say was that there was a time, rather before you were born [laughter], when the whole school of cosmology was based around vortical structures being the first ones. Bernard Jones in particular studied this for his thesis, and there was a whole set of people who were wild on vortical structures being the first structures there were. It's a very good idea probably to study it again because there wasn't any microwave background then, but I think you might find that there are things dating back perhaps 30 or maybe 40 years which are of interest.

Dr. Christopherson. Thanks!

Professor A. M. Cruise. Is there any chance that your studies will lead you to understand vorticity on a much larger scale than what you are doing at the moment? What is in my mind, is that in the past I've seen papers about whether the Universe is rotating. And actually, there are some technological advances in very-high-precision accelerometers that could begin to set some limits on this. I just wondered if the whole approach you are taking would completely disappear if vortices occupy a large fraction of the Universe? Are they set by the perturbation theory; are they necessarily small?

Dr. Christopherson. The approximation that we make is that we perturb things around in the Friedmann–Robertson–Walker background. If you have some large-scale vorticity in the Universe, then you can't use the Friedmann–Robertson–Walker background anymore. So you'd be able to do perturbation theory but it would be around some other background.

The President. Last question?

Dr. A. Brandenburg. There was this question about where does vorticity come into play, and you said it must be somewhere on this page, and in fact, it is, because what produces your δP non-adiabatic is entropy. And the only way to produce entropy is through viscosity, viscous heating, so to speak, and that's how it actually comes into this page.

Dr. Christopherson. Thank you for pointing that out!

The President. It seems like a good note on which to thank Adam again. [Applause.]

Our second speaker is my colleague in Oxford, Chris Lintott. His topic is: 'What to do with 500 000 scientists: latest results from the Zooniverse'.

Dr. C. Lintott. I want to speak today about the results from a whole suite of collaborative projects with members of the public which go under the collective name of the Zooniverse. We started some years ago with Galaxy Zoo and now have 550 000 registered users on ten live projects, most of them astronomical.

The first one is the Milky Way project started in 2010 in which we set out to create a census of star-forming regions from the large *Spitzer* survey of the Galactic plane. Specifically we are looking for bubbles around star-forming regions and hot young stars, which is clearly a visual task. The data come from the GLIMPSE survey, which is a random survey at 4·5 microns, and images also include data from MIPSCALE at 4·5, 8, and 24 microns. The raw images split up into smaller areas for identification purposes and each area will have about 50 investigators looking at it. The first paper listing identifications of 5000 bubbles has come out, and compared to the professional work of Churchwell *et al.* (employing one postdoc) we find that the volunteers have found an order of magnitude more bubbles by their efforts. You can get the reduced data from the website milkywayproject.org/data.

Galaxy Zoo did the Sloan survey twice on a total of 250 000 galaxies, asking simple questions to determine the morphology of those galaxies, and in a related investigation we have moved on to examining other *HST* surveys such as COSMOS, GEMS, GOODS, and AEGIS, which are now all complete. Thirty papers have been published on this work and more are coming. I'd like to concentrate on three particular programmes.

First, red spiral galaxies: we find that in some environments they account for more than one-quarter of all galaxies and do not have the star formation we come to expect in late-type spiral galaxies. What has happened there to switch off star formation? There are a number of clues which may help. The redder the galaxy, the more likely it is to have a bar. In fact whilst blue galaxies have a bar in 1 in 5 cases, the proportion in the case of the red spirals is higher than 1 in 2. People have asserted that the bars have played a rôle in changing gas content either by angular-momentum transfer or by channelling gas into the centre to fuel the AGN, and that is what may be going on here. We have also done some tests with the ALFALFA collaboration, which is an H I survey carried out by Arecibo Observatory, with the help of Karen Masters (Portsmouth). We do indeed see a drop in the fractional mass content of gas in barred spirals. There may be AGN at the centres of these spirals, and to constrain the AGN-activity problem we use a different set of galaxies which are essentially bulgeless and have broad-lined AGN at the centre. They are large galaxies and have not undergone major mergers and it is difficult to do this without putting material into a bulge.

A second highlight from Galaxy Zoo is the serendipitous discoveries such as Hanny's Voorwerp, which is an anomalous object. It is connected to the disturbed galaxy IC 2497 and it appears to be gas at 50 000 K with a small amount of star formation which is being heated by a jet emanating from a quasar-scale AGN in the galaxy; but observations with *XMM*, *Newton,* and *Chandra,* for instance, have revealed no QSO in IC 2497. Perhaps it has recently switched to QSO quiescence — possibly recently reacting with two nearby galaxies. We have found a number of small anomalous objects, some of which have known AGN, but several have implied ionization lines from gas which is greater by a factor of 2 to 10, and we think this may be a way to probe AGN variability over time-scales of thousands of years. We have an *HST* project to follow-up on these objects.

Thirdly, looking at exoplanets from transit data supplied by *Kepler*, Galaxy Zoo members have found about 200 candidate planets, most of which had already been picked up by *Kepler* teams, but we did find a number of overlooked discoveries. The visual method does seem more efficacious than the automatic software, especially where we are dealing with multiple planets. People look at the light-curves using the main interface software and are asked the question "Would you like to discuss this star?" Mostly this question goes unanswered but sometimes people do comment and this is picked up by the experts. We find that people with no experience of this area of astronomy can teach themselves the language of planetary transits. Next month we are going to start looking at radio-transit data from the SETI experiment with the *Allen* radio-telescope array. We take live data and need to decide how to pass this material on to the people within three minutes. This is the first system with a very large public to incorporate automatic routines representing the insight put in by professional astronomers and live interaction.

Rev. G. Barber. With the papers you're publishing, are you following the traditions of the LHC and having 50 000 authors?

Dr. Lintott. We tried! Actually with *Monthly Notices*, we've had to negotiate a policy. There are about 40 assistant scientists, who have made discoveries and have contributed, who are on the papers; then at the end of the author list is a link to a very long list on-line, so that people are credited. That seems more important to them than currency of paper publishing; it's something we're used to, but others aren't. But I think giving credit is important. And feeding back what we are doing is important, so we're struggling slightly to make sure that we keep people excited. "Science takes a long time, your results are great, we'll get back to you in nine months", doesn't go down very well. And so that's a struggle.

Dr. B. Gustavsson. What proportion of the results which were published by *Kepler* are done automatically, and what proportion are done in this fashion?

Dr. Lintott. At present, we recover about 70% of their planet candidates independently, and we've found somewhere between four and 20, depending on how the next couple of weeks turn out. They have six-months-to-a-year head start, which is about to vanish. So from November data will be available equally. And I suspect that there's somebody in California, at least, who is going to enjoy publishing. The key point actually is that *Kepler*'s mission is to do the statistics of the planet population: are Earths more common than Neptunes, for example? That relies on a very strong pipeline, and we're a completely independent check on that. And so actually, the agreement is a good thing for both sides, I think.

A Fellow. How complicated a task do you think you can have the public do?

Dr. Lintott. The more complicated the task, the fewer the people. We've just finished a project which had people modelling merging galaxies. They were controlling a 30-parameter fit. And that was partly done on their machine, partly done with their interaction. And the results look great — we've got really good fits for about a hundred merging pairs. We need more mergers, if anyone has any that are worth modelling. So, complicated is fine, you just have to take it to fewer people. Boring is also fine! People will stare at light-curves, people are going to do the SETI stuff. It's not just the pretty images. In fact, there is no correlation between how much people enjoy a project and how beautiful they find the images. So I think it is reassuring for those of us who work in fields that don't produce beautiful, gorgeous maps of the Milky Way.

Dr. B. Barrett. How long are user accounts staying active for?

Dr. Lintott. Lots of people turn up once, and disappear. Of the 550 000, 350 000 have been active in the last year, which is huge. Typically, people will be reminded of the existence of the project, will turn up, will do a bit, and go away until they get another email, or they hear about something else on the news. When we launch a new project, all the other projects get a boost, because people are reminded that they enjoyed doing that. So far, we don't have a longevity problem. There are also lots of people out there — 550 000 people isn't a lot on the internet, and so there is no sign of a shortage of people.

The President. Thank you very much, Chris! There are few things more important to us than third-party validation, and certainly you have shown that we have that in spades, and this is a very inspiring development to see so many people wanting to get involved in our activity. [Applause.]

The President. Our next speaker is David Vaughan, from the British Antarctic Survey, whose topic is: 'Why is it so hard to predict the future of ice sheets and sea-level rise?'

Professor D. Vaughan. In the recent past sea-level change has been the poster child for climate change but there are some serious issues with sea-level rise which the wider public do not understand. Sea-level rise has a very subtle impact on our lives and the media give extremely contradictory statements about the issue. London sits on a tidal estuary, it is prone to flooding, and it is both an enormous economic and human asset. In the past we have been increasingly responsive to sea-level rise but I would rather look ahead and build accordingly. London has so far been lucky. Since the flood in 1953 we have built the Thames Barrier and since completion in 1983 we have closed it more than 100 times to date. The barrier is built to look after a 1-in-a-1000-year flooding event, provided the sea-level remains the same. However, if sea-level increases by 0·5 metre then the barrier can deal with a 1-in-a-100-year event, and if it rises another half a metre it becomes a 1-in-10-year possibility. We need to raise the sea-level defence in London if we want to keep the risk of flooding acceptable.

For Londoners, sea level is increasing by 3 mm per year and is caused by thermal expansion of the oceans (which accounts for 1 mm), local land movement such as subsidence caused by the removal of Scottish ice sheets, and global effects which include the amount of ice being added to the ocean from glaciers. Most glaciers are losing mass and may account for a sea-level rise of 1 mm per year. Then there are the ice sheets. Greenland has enough ice so that if it were all to melt the sea level would rise by seven metres. For Antarctica, complete melting of the ice would contribute 60 metres of sea-level rise. I'm more interested in the turnover — glaciers are equilibrium systems — snowfall going in, melt-water or icebergs or some form of ablation coming out at the other end. Melting glaciers are counteracted to an extent by falling snow but the imbalance still means that sea level rises by 2 mm per year. In Antarctica 6 mm of sea-level rise goes in and 6 or 6·5 mm per year of sea-level rise goes out. In Antarctica the melting that does take place is almost all due to the rise in the sea temperature and not that of the atmosphere. In Greenland the melting is due equally to warmer sea and air temperatures. The ice caps are hugely sensitive to changes in temperature and the major contributor to sea-level rise is Antarctica. The Intergovernmental Panel on Climate Change (IPCC) had identified that the major contributor to sea-level rise lies mainly in Greenland and Antarctica. My personal view of projected sea-level rise is that by 2100 it will amount to somewhere between 50 and 100+ cm and will be rising by 3·5 cm per year at the end of this century.

What we do we know about the ice sheets? There are only two, so we do not have a statistical view of how they behave. The Antarctic Ocean drives the changes and the Greenland atmosphere drives the changes. We can now see which specific glaciers are losing the most ice. Satellites such as *GRACE* are very useful. There are actually two satellites in orbit above the Earth, one following the other, which do little but measure the distance between them very accurately. When the first goes over an area of high gravity it accelerates away from the following satellite. Then the other one catches up as the first decelerates. The distance between them tells you about the density of the planet beneath. *GRACE* has allowed us to measure the weight of both Greenland and Antarctica. Over the past few years we have found that Greenland has lost most mass but Antarctica presents a more complex picture. We can measure the mass loss for each month so why can't we just give the projected numbers expected for the coastal-defence planners?

Firstly, the climate impact is at the end of a long chain of events. Greenhouse gases affect the whole climate and we need to understand the regional climate. The glacier response to that in regional sea-level rises and coastal impacts and so on can only produce projections not predictions. Secondly, instabilities in the ice sheets allow them to collapse without a climate driver. It is not clear why. We have sent autonomous, unmanned submarines under the floating ice shelves to look at the heat delivery to the ice sheet — is it weather or climate? In order to make predictions we need more than the satellite records — they don't go back far enough. We need to accrue geological records as well in order to test our models. By looking at rocky outcrops in the middle of ice sheets we can get an idea of their age so that we can find out when the ice sheet was first exposed. Additionally we need to collect those data for many separate ice sheets, and it is the ice sheets that are relatively predictable but the boundary conditions are not. The Ice2Sea partnership contains 24 different parties in an attempt to contribute to our knowledge of glaciers and ice sheets. The IPCC will deliver its report next year — this July is the submission deadline for papers.

The President. Thank you, David. One can't help making the connection between the last two talks and wondering whether we have some potential there, ultimately. Questions?

Mr. M. Hepburn. You only just touched on it, but the great ocean basins are all filled with very cold water, which is in dynamic equilibrium because it is fed from Antarctica. That must be warming up — surely the high rates of melting we are seeing from Antarctica may be a homeostatic effect, to cool the great ocean basin water, and therefore, since it will be colder, it should also be denser. So will that not work in the opposite direction? And so the sea-level rise would not be so great? And also absorb more CO_2, because it's perfectly clear from isotopic studies that practically all the CO_2 up there doesn't come from fossil fuel. If it did, the proportion of ^{14}C would be roughly half.

Professor Vaughan. I believe that there is strong isotopic evidence from Antarctic ice cores that it is a fossil-fuel source, and that it has increased substantially from 280 ppm to 360–370. To answer your initial question about whether there was a cooling of the deep oceans due to the addition of melt-water: at the moment there is not, because there is a general warming of parts of the atmosphere that's slowly getting into the oceans. The thermal expansion of the oceans is all the way through, and you need only to have a mixed layer that expands to get some sea-level rise, and the taking of the heat down deeper — maybe it will take decades and decades, but eventually it will get there. Yes, you are right that there are effects, perhaps of changing the ocean circulation, that will result from big additions of new amounts of melt-water from Antarctica and from Greenland. Actually, in the Greenland scenario, the amount of cold fresh water is only part of the equation. The rainfall is also an enormous amount of freshwater going in, and so is the rejection of water as sea ice freezes. So, for the oceans, the ice is not the dominant influence, probably in the North; it might be in the South.

The President. We don't have time for any more questions, I'm afraid, because we do have to leave promptly at 6 o'clock today, and I don't want to take time away from the next presentation. But there will be opportunities to talk, I'm sure, after we finish. Let me thank David Vaughan again. [Applause.]

Our final speaker today is Pål Brekke, from the Norwegian Space Centre, and he's going to talk about 'The Northern Lights: from myths to modern science'.

Dr. P. Brekke. For thousands of years people in the northern part of the world have marvelled at the spectacular and fearful displays that occasionally light up the night sky. And there have been hundreds of stories and theories to explain these celestial lights, what we now know as the aurora borealis or Northern Lights. But no one, until about a century ago, suspected a connection with the Sun.

Every northern culture has oral legends about the aurora, passed down for generations. During the Viking period, Northern Lights were referred to as reflections from dead maidens. The phenomenon was often referred to as a vengeful force. In ancient times, most people were afraid of the Lights. Other cultures believed it was a message from the Creator. An old tale from the Nordic countries said that, "God is angry when the aurora flames." In other cultures it was an omen of impending war, disasters, or plagues. The first realistic description of aurorae is found in the Norwegian chronicle *The King's Mirror* from about the year 1230. It was originally written as a text-book, probably for the young King Magnus Lagabøte, by his father. At that time people thought that the Earth was flat and surrounded by oceans. One explanation was that the oceans were surrounded by fire and that aurorae were the light from those fires reflected in the sky. Another possibility was that reflected sunlight from below the horizon illuminated the sky. A third explanation was fires in Greenland.

Swedish scientist Suno Arnelius suggested that solar rays were reflected off ice particles in the atmosphere. But later spectroscopic analysis of the light showed it was not of solar origin and dismissed this theory. A major breakthrough was made by an eccentric Norwegian scientist — Kristian Birkeland, who had a theory in which charged particles from the Sun could ignite aurorae. To prove his theory he built his own world in a glass box, electrified his model Earth with its own magnetic field, and showed how particles from the Sun could ignite aurorae. The particles were captured by the Earth's magnetic field and channelled down towards the polar regions. He also showed that they would be identical and simultaneous at both poles. The front of a Norwegian bank note shows a portrait of Kristian Birkeland against a stylized pattern of the aurora and the terrella experiment. The back of the note shows a geographical map of the north polar regions illustrating the location of the auroral oval.

Looking at the sky with the naked eye, the Sun seems static, placid, and constant. From the ground the only noticeable variation in the Sun is its location and sometimes darks spots occur on its visible surface. This is where the tale of the aurora starts: on the Sun — a star of average size among billions of other stars in our Milky Way. However, it is a very dynamic star changing every second, every hour, every day. Sometimes strong magnetic fields push up through the surface and block some of the energy escaping in these regions and dark areas called sunspots are formed. Sunspots can last from a few hours to several months, and a large sunspot can grow to several times the size of the Earth.

Every 11 years the Sun undergoes a period of high activity called the 'solar maximum', followed about five years later by a period of quiet called the 'solar minimum'. During solar maximum there are many sunspots, and during solar minimum there are few. Thus, one way of tracking solar activity is by observing the number of sunspots. However, the Sun gives us more than just a steady stream of warmth and light. The steady stream of particles blowing away from the Sun is known as the solar wind. Blustering at 1·5 million kilometres per hour, the solar wind carries a million tons of matter into space every second. Using satellites we can image the Sun's dynamic atmosphere called the chromosphere. There we see bright intense regions and how violently the Sun behaves every second of every day. Using different filters in the telescope we can observe the hot corona where one can see hot gas tracing out the magnetic loops coming out of the sunspot areas. Sometimes the strong magnetic fields near sunspots get twisted and snap. This can release enormous amount of energy. Such solar flares are short, violent explosions that accelerate particles and intense X-ray radiation into space. Energy equal to a billion megatons of TNT is released. Sometimes large amounts of gas are also hurled out in space. We call these eruptions coronal mass ejections. They thrust billions of tons of particles at speeds up to 8 million kilometres per hour! When the solar storm reaches Earth, something strange happens. It is deflected by the Earth's magnetic field. The magnetic fields couple together, and the gas in the solar storm streams down towards the poles on the daylight side. This is the daylight aurora. The magnetic fields stretch further rearwards and couple together. The magnetic fields, stretched like rubber bands, break, and gas from the solar storm streams along the magnetic lines towards the poles on the night side. This is night-time aurora. These collisions usually take place between 80 and 300 km above ground. There they cause oxygen and nitrogen to become electrically excited and to emit light in much the same ways as in fluorescent lights and old-fashioned television tubes. The result is a dazzling dance of green, blue, white, and red light in the sky.

The Northern Lights are impressive and different from all other light phenomena by exhibiting an amazing variety of colours, structures, and movements. The best place to see the aurorae is, of course, at high latitudes. Aurorae are present within a zone about 1000–3000 km from the magnetic poles, both day and night during the entire year. However, aurorae are only visible from the ground during clear, dark nights. Daylight will outshine the aurorae. The best period is from September to April. In Scandinavia the strongest aurorae often occur between 8 pm and midnight. You should avoid city lights and find a dark place away from the city on a summit or open country with a clear view of the northern horizon. One should also avoid the full Moon, which makes the sky less dark. In parts of northern Norway you can see the Northern Lights almost every dark and clear night. If there is a gust in the solar wind or a strong solar storm, the Northern Lights will extend further south. Sometimes the aurora can be seen even in the south of Europe. Several satellites are observing the Sun 24 hours a day and scientists can detect eruptions on the Sun that will produce strong Northern Lights. By monitoring the activity on the Sun every day and measuring the speed of the solar wind, scientists can predict the strength and the location of the aurora.

The President. Thank you very much, it sounds remarkable! Questions?

Mr. H. Regnart. Is there any possibility that the phenomena causing the aurora could induce currents in the ground, which could have effects which people might be able to hear?

Dr. Brekke. Yes, I think that's right. Because it cannot be sound from space, it must be something else that generates it. Some people think they hear some crackling sounds, and so on, so I think that's the case. And people have tried to study these things, and even tried to find if there is a correlation between physical or physiological things embodied in the geomagnetic storm. There's actually a paper published, and for those who want the reference I have it in my computer. The author looked at 3500 admissions to mental hospitals, and they found a very good correlation, I mean a significant increase, just after geomagnetic storms. And this change in magnetic fields is probably affecting the gland which produces melatonin, which is a depressive substance. An interesting find in this study was that it was only valid for men, not for women. [Laughter.] In the United States, they took the report even further, because they looked at the stock reports, and found the same correlation, and put it back to the same study, that if you're depressive you will be more likely to sell your stocks.

The President. So to those who want to use this as investment advice, be warned! I believe it is true that the currents induced in the Earth are a much more serious concern for the power networks than for listening to the aurorae.

Professor P. G. Murdin. I watched an aurora once, from the northern United States, in a woodland glade, and it seemed to me that the fireflies all the way around the glade were pulsating in synchronism with the aurora. Are there reports of the effects of the aurora on animal behaviour as well as humans?

Dr. Brekke. I don't know, but the only thing I can think of is that the fireflies enjoy the aurora too! [Laughter.]

Professor Vaughan. One of your diagrams showed there is a day-side particle stream coming in. Is there any effect on that day side that we note?

Dr. Brekke. The day-side aurora is interesting, and the reason they have the observatory in Svalbard is because they can observe the day-time aurora. That's where the particles arrive from the Sun. It's more like a cusp. Here you can really measure the direct solar wind; the night side reflects more the old particles that have been stored for a while, and then they're being pumped back during the Earth's geomagnetic storms.

A Fellow. Whilst it's great to know that we've got a CME going off, and we're probably two days away from getting a magnetic storm, can we effectively do anything on Earth to minimize this effect, or do we just have to say in two days' time we're going to get hit. I mean you can't turn off GPS systems and things like that, can you?

Dr. Brekke. No, you cannot do anything for the GPS system, but for power grids you can do a lot. They can mitigate, they can lower the load in the system, they can reroute power, and shorten the stretch in which currents can be induced, and they do that now — they can keep a track of this thing.

The Fellow. They had a big one in Canada a few years back, didn't they? A power outage.

Dr. Brekke. Yes, and after that the power industry, and the world basically, learned a lesson. That is why we don't have these big effects anymore, because they know about these things. They can even put a filter in front of the transformer. In Norway we have a more robust system, because we have many small power plants, and small grids. But for the United States, which has huge power plants, maybe only connected with one transformer, those are more prone to go. Satellite operators can also prevent problems with the satellites, such as turning off some instruments and avoiding any manoeuvring during storms. There is lots of stuff we have learned not to do.

The President. Let's thank Pål again. [Applause.] May I remind you that we have drinks in the RAS library, and we meet again on Friday the 9th of March.

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 225: HR 1313, HR 3567, HR 3907, AND HR 6239 WITH A NOTE ON δ BOO, HD 146815, 64 AQL, AND 75 DRA

By R. F. Griffin
Cambridge Observatories

The four stars all have orbits with periods of a few years and modest eccentricities of about 0·2–0·3. The mass functions are small apart from that of HR 6239, whose companion (if not itself a binary system) must be an F or even late-A main-sequence object; no evidence of it has been seen, but experience indicates that such a spectrum is difficult to recognize in one dominated by a late-type giant.

The four objects were placed on the Cambridge spectroscopic-binary programme after the variability of their velocities had been detected at Haute-Provence. Certain other stars adopted in the same way from the same list appear not to vary in the manner implied (or, in most instances, at all); in nine cases a particular Haute-Provence measurement has been suspected of error, probably in the identification of the star concerned. In three cases it has been possible to suggest the identities of the objects to which the suspect measurements may really belong. The errors are pointed out simply to save other observers repeating the fruitless investigation of the stars as if they were binaries. Cambridge observations have already been given for three of the objects concerned, and are listed in this paper for four of the others. Of the remaining two, one is not accessible to the Cambridge telescope and the other is a long-period binary system whose orbit is not yet ready to be written up.

Introduction

The four 'Bright Stars' were all found to be spectroscopic binaries in a survey that was published in 1999 by de Medeiros & Mayor[1], who in 2002 made available, through the Centre de Données Stellaires (CDS), their individual radial-velocity observations. Some of the stars whose velocities appeared to change were selected from the CDS listing for inclusion in the Cambridge spectroscopic-binary observing programme. Orbits have already been given in this series of papers for 25 of them; observations of five others (HDE 276743, δ Boo, HD 146815, 64 Aql, and 75 Dra) have been discontinued after the source paper[1] and the CDS list were suspected of errors. The case of HDE 276743 has already been discussed[2]; evidence concerning the other four stars is offered below, in the final section of this paper. About 30 additional stars that were adopted from the CDS list, not counting the four whose orbits are given here, remain on the Cambridge observing programme. The observations transcribed from that list are routinely adjusted by +0·8 km s^{-1} in the present writer's orbital solutions (including those obtained below) in an effort to take account of differences in zero-point.

HR 1313 (HD 26755)

HR 1313 is in a region of the sky in Camelopardus that looks rather barren to the naked eye; the star is to be found nearly two-thirds of the way from α Per towards β Cam. *Simbad* seems to imply that no broad-band photometry of it has ever been published in the normal way, as it reports only a private communication by Häggkvist giving $V = 5^{m}{\cdot}71$, $(B - V) = 1^{m}{\cdot}09$ — values that appear also in the *Bright Star Catalogue*[3]. They do in fact feature in a paper[4] published by Häggkvist & Oja in 1987, which could, however, throw the searcher off the scent by its title which refers only to *narrow-band* photometry. That is not the whole solution to the puzzle, however, inasmuch as the *Bright Star Catalogue* ante-dates by five years the paper by Häggkvist & Oja and includes no acknowledgement to them, so possibly there is an earlier publication by them or others that gives the same or similar data and has escaped the present author's attention.

A spectral type of K1 III was found by Yoss[5] from an objective-prism spectrogram taken with the University of Michigan's *Curtis Schmidt*, and one of K2 III was given by Appenzeller[6] from a 120-Å mm^{-1} slit spectrogram taken (surely very quickly!) at the Cassegrain focus of the McDonald 82-inch reflector. The (revised[7]) *Hipparcos* parallax of HR 1313 is 12·53 ± 0·41 arc-ms and corresponds to an absolute distance modulus of $4^{m}{\cdot}51 \pm 0^{m}{\cdot}07$. That implies $M_V \sim +1^{m}{\cdot}20$ if interstellar absorption may be neglected; such a luminosity is very characteristic of a 'clump' giant (one that is powered by a helium-burning core). It is hard to know how Mishenina *et al.*[8], starting from very similar data, could have obtained an M_V of $+0^{m}{\cdot}679$; their whole Table 1, containing the 174 *Hipparcos* giants which are the main subject of their study, appears to be similarly mis-calculated despite the M_Vs being given to a thousandth of a magnitude*. They found the abundances of elements near iron in the Periodic Table to have near-solar abundances in HR 1313; in their later paper[9], where the star is noted as 'RGB' (*i.e.*, as being on the ascending red-giant branch of

*An enquiry made by the Managing Editor to those authors, as part of the process of reviewing the present paper for possible publication, elicited the response that Mishenina *et al.*'s[8] Table 1 is indeed in error throughout, tabulating M_{bol} rather than the ostensible M_V.

the Hertzsprung–Russell diagram, rather than a 'clump giant' as before[8]), the abundances of several rare-earth (neutron-capture) elements are also listed as close to solar. Luck & Heiter[10] found the abundances of six elements in the range of atomic number from Na to Ca to be slightly enhanced with respect to the Sun; Brown *et al.*[11] were unable to detect the red lithium line and concluded that $\log(\mathrm{Li}) < 0{\cdot}0$ on the usual scale on which $\log(\mathrm{H}) = 12$.

The radial velocity of HR 1313 was first measured at the Dominion Astrophysical Observatory (DAO), very soon after the 72-inch telescope was inaugurated there. Six measurements, made in 1918–1920 and in good mutual agreement by the standards of the time, were published by Plaskett *et al.*[12]. In 1938 Christie & Wilson[13] gave a mean of three Mount Wilson velocities, which long afterwards were listed individually by Abt[14]. De Medeiros & Mayor[1] referred to two Haute-Provence (OHP) *Coravel* measurements that were subsequently made available separately, as noted in the *Introduction* above; for the first time there was a major discordance (nearly 10 km s^{-1}), certainly demonstrating the binary nature of the star. The same observations were also referred to by de Medeiros, da Silva & Maia[15].

HR 1313 also features in a paper by Massarotti *et al.*[16], which refers to a computer-accessible table that includes four radial velocities of the star. If one knew the approximate period one could obtain an orbit, of sorts, just from the published observations, but (partly owing to unfortunate phasing) its elements are a long way from the true ones. That is remedied here by the provision of 54 velocities obtained with the Cambridge *Coravel*, which are listed in Table I along with those from the four sources just mentioned.

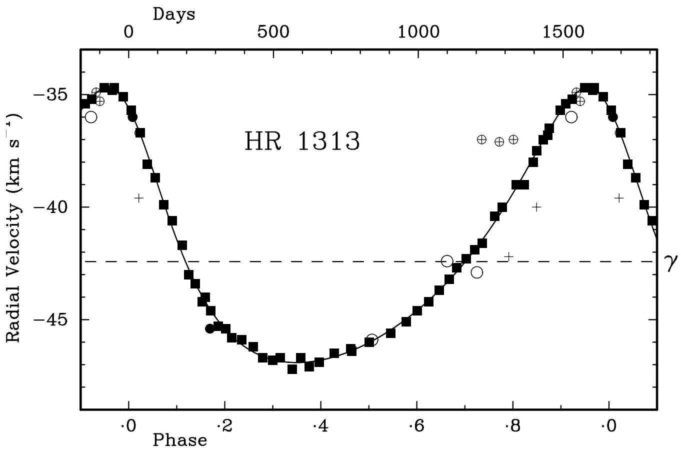

FIG. 1

The observed radial velocities of HR 1313 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. In all the figures, the great majority of the observations, made with the Cambridge *Coravel*, are represented by filled squares, and the few filled circles denote those made with the analogous instrument at Haute-Provence (OHP). Here, other observations (not used in the determination of the orbit) from three published sources are plotted. Velocities determined the best part of a century ago at the DAO[12] and at Mt. Wilson[13,14] are shown as circles with plusses in them and as plusses, respectively. Comparatively recent CfA measurements[16] appear as large open circles.

TABLE I

Radial-velocity observations of HR 1313

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

Date (UT)	*MJD*	*Velocity km s^{-1}*	*Phase*	*(O – C) km s^{-1}*
1918 Oct. 30·37*	21896·37	−37·0	$\overline{19}$·735	+4·5
Dec. 30·22*	957·22	−37·1	·772	+3·3
1919 Feb. 17·11*	22006·11	−37·0	$\overline{19}$·801	+2·3
Sept. 22·52*	223·52	−34·9	·932	0·0
Oct. 5·46*	236·46	−35·3	·940	−0·5
1920 Feb. 18·10*	22372·10	−36·7	$\overline{18}$·022	−0·1
1932 Sept. 15·51†	26965·51	−42·2	$\overline{16}$·791	−2·5
Dec. 20·27†	27061·27	−40·0	·849	−2·4
1933 Oct. 1·54†	27346·54	−39·6	$\overline{15}$·021	−3·0
1988 Mar. 7·82‡	47227·82	−36·0	$\overline{3}$·008	0·0
Nov. 29·84‡	494·84	−45·4	·169	−0·9
2002 Sept. 28·11	52545·11	−45·8	0·214	−0·1
Nov. 2·09	580·09	−45·9	·235	+0·2
Dec. 11·12	619·12	−46·2	·259	+0·2
2003 Feb. 17·95	52687·95	−46·8	0·300	0·0
Mar. 14·88	712·88	−46·7	·315	+0·1
Sept. 18·17	900·17	−46·5	·428	+0·1
Nov. 13·08	956·08	−46·3	·462	+0·1
17·03	960·03	−46·4	·464	0·0
2004 Jan. 17·02	53021·02	−46·0	0·501	0·0
26·21§	030·21	−45·9	·507	0·0
Mar. 29·88	093·88	−45·6	·545	−0·1
Sept. 14·15	262·15	−43·7	·647	0·0
Oct. 11·17§	289·17	−42·4	·663	+0·9
19·17	297·17	−43·2	·668	0·0
Nov. 13·12	322·12	−42·7	·683	+0·2
Dec. 17·00	356·00	−42·3	·703	+0·1
2005 Jan. 13·89	53383·89	−41·9	0·720	0·0
22·24§	392·24	−42·9	·725	−1·1
Feb. 8·89	409·89	−41·6	·736	−0·1
Mar. 24·89	453·89	−40·4	·762	+0·3
Apr. 18·85	478·85	−40·0	·777	+0·2
Aug. 16·15	598·15	−37·5	·849	+0·1
Sept. 8·19	621·19	−37·0	·863	0·0
28·17	641·17	−36·5	·875	+0·1
Nov. 5·11	679·11	−35·7	·898	+0·1
25·03	699·03	−35·4	·910	0·0
Dec. 14·27§	718·27	−36·0	·922	−0·9
17·10	721·10	−35·2	·923	−0·1
2006 Jan. 28·87	53763·87	−34·7	0·949	0·0
Feb. 25·85	791·85	−34·8	·966	0·0
Apr. 3·85	828·85	−35·1	·988	+0·1
July 24·12	940·12	−38·7	1·055	0·0
Sept. 20·19	998·19	−40·6	·090	+0·3
Oct. 25·07	54033·07	−41·7	·111	+0·4
Nov. 17·09	056·09	−43·0	·125	−0·2
Dec. 9·02	078·02	−43·4	·139	0·0

TABLE I (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)* *km s⁻¹*
2007 Jan. 2·07	54102·07	−44·2	1·153	−0·2
11·87	111·87	−44·0	·159	+0·2
31·84	131·84	−44·6	·171	0·0
Feb. 26·80	157·80	−45·3	·187	−0·2
Nov. 9·09	413·09	−47·2	·341	−0·3
Dec. 7·99	441·99	−46·7	·358	+0·2
2008 Jan. 5·96	54470·96	−47·1	1·375	−0·2
Feb. 10·96	506·96	−46·9	·397	−0·1
Dec. 7·05	807·05	−45·1	·578	−0·1
2009 Jan. 13·99	54844·99	−44·6	1·601	0·0
Feb. 21·89	883·89	−44·2	·624	−0·1
Dec. 20·94	55185·94	−39·0	·807	+0·1
2010 Jan. 17·92	55213·92	−39·0	1·823	−0·5
Feb. 17·86	244·86	−38·0	·842	−0·2
Apr. 8·86	294·86	−36·8	·872	−0·1
Sept. 17·19	456·19	−34·7	·970	+0·1
Nov. 15·05	515·05	−35·7	2·005	+0·1
Dec. 17·06	547·06	−36·7	·024	+0·1
2011 Jan. 9·91	55570·91	−38·1	2·039	−0·5
Mar. 8·80	628·80	−39·9	·074	−0·1
Oct. 7·15	841·15	−45·4	·202	0·0
2012 Feb. 11·85	55968·85	−46·7	2·279	−0·1

* Observed with DAO 72-inch telescope[12]; weight 0.
† Observed with Mt. Wilson 60-inch telescope[13,14]; wt. 0.
‡ Observed with Haute-Provence *Coravel*[1]; weight 0.
§ Observed by Massarotti *et al.*[16]; weight 0.

The Cambridge observations, by themselves, yield an orbit with a period of 1658·7±2·9 days. Inclusion of the two OHP velocities refines the period to 1660.4±1.9 days but makes negligible change in the other elements. By an oversight for which the writer apologises, the final orbit which underlies Tables I and V was run with the Cambridge data alone. The other three sources have also been zero-weighted: they are quite compatible with the adopted orbit but cannot contribute usefully to it. Those from long ago[12–14] are really too ragged to be useful, and even the CfA ones, which (being contemporaneous with the writer's data) offer no time-base advantage, have 25 times the variance; the situation is made clear in Fig. 1, where all the velocities are plotted as well as the computed orbital velocity curve. The elements are given towards the end of this paper, with those of the other stars, in Table V.

By way of comment, one can remark that the orbital period is agreeably near to a half-integral number of years (it is 4·55 years), facilitating good phase coverage of the orbit in only two cycles. The mass function requires the secondary star to have a mass of at least 0·6 $M_\odot$ if the primary is taken as 2 $M_\odot$ (a number that is bracketed by different models proposed, *e.g.*, by Luck & Heiter[10]). Thus the secondary could well be a G or K dwarf, which would put it from about three (at G0) to six (at K5) magnitudes fainter than the primary; if it were a white dwarf it would probably have been evident in ultraviolet surveys.

HR 3567 (HD 76629)

This star is to be found in the south-eastern corner of Cancer, about 2½° south of α Cancri. It is slightly fainter than the sixth magnitude: accordant photometry by Cousins[17], Argue[18], and Johnson *et al.*[19] yielded $V = 6^{m}{\cdot}18$, $(B-V) = 0^{m}{\cdot}98$, $(U-B) = 0^{m}{\cdot}76$. Halliday[20] classified the spectrum as G8 III, but derived a somewhat higher luminosity, corresponding to a luminosity class of 2·7, by objective measurement of spectroscopic criteria with his 'oscilloscopic microphotometer' system; he considered the star's absolute magnitude to be $-0^{m}{\cdot}6$, giving a π_{sp} of 4·6 arc-ms. That is indeed close to the astrometric[7] value, which is 3·78 ± 0·38 ms and corresponds to a distance modulus of $7^{m}{\cdot}11 \pm 0^{m}{\cdot}22$ and thus to an absolute magnitude (without allowance for the probably small amount of interstellar absorption) of $-0^{m}{\cdot}93$, with the same uncertainty.

The only other literature that might be summarized here on HR 3567 consists of three publications by Eggen, who included it in papers on samples of 'the old disk population'[21], 'the young disk population'[22], and 'bright stars'[23]. In the 'old disk' paper he reckoned to obtain photometric luminosity estimates by three quasi-independent methods stemming respectively from Strömgren, Geneva, and David Dunlap Observatory (*DDO*) photometry, which all gave accordant values ranging from $M_V = +1^{m}{\cdot}2$ to $+1^{m}{\cdot}4$ and therefore (it seems) all about two magnitudes in error. Along with the proper motion and radial velocity, those data were used to give the space motions of the star in the three Galactic coördinates. In the 'young disk' paper the *DDO* M_V was slightly revised, to $+1^{m}{\cdot}00$, and the tabulated radial velocity had undergone an astounding change, from the −13·6 km s^{-1} given in the *Radial Velocity Catalogue*[24] and in the 'old disk' paper[21], to +26·0 km s^{-1}, without, however, precipitating any significant change in the derived Galactic velocity vectors.

The radial velocity of HR 3567 was first measured at the DDO, from where a mean value of −12·6 km s^{-1} with a 'probable error' of 0·8 km s^{-1}, from four plates with a reciprocal dispersion of 33 Å mm^{-1} at Hγ, was published in 1945 by Young[25]. The value printed in the *Radial Velocity Catalogue*[24] is −13·6 km s^{-1} — it has evidently been 'corrected' by −1·0 km s^{-1}, which the compiler of that *Catalogue* (*cf.* his Table 3) found to be appropriate to K-type stars observed at the DDO; he must have preferred the K0 type shown in the *Henry Draper Catalogue* to the revised type of G8 (for which no correction would be applicable) given by Young. The only other radial-velocity information to be found concerning HR 3567 is that described in the *Introduction* above, by de Medeiros & Mayor[1] and de Medeiros, da Silva & Maia[15], who refer to three *Coravel* measurements discordant enough to have an apparent r.m.s. 'error' per observation of 4·96 km s^{-1} and a mean (given in ref. 1 only) of −8·18 km s^{-1}. The three observations, as they were later made accessible through the Centre de Données Stellaires, actually have a mean of −7·90 km s^{-1} and apparent errors of 4·87 km s^{-1}, not quite agreeing with the values originally published. Be that as it may, they are listed at the head of Table II, rounded from two decimal places to one. The rest of that Table consists of the writer's 35 measurements made with the Cambridge *Coravel*; they have been adjusted by −0·2 km s^{-1} from the 'as reduced' values, in an effort to take account of the dependence of the *Coravel* zero-point on the colour of the observed star.

The Cambridge observations alone yield an orbital period of 878·5 ± 0·9 days (2·40 years, again near to a half-integral value that helps the observer to obtain uniform phase coverage of the cycle). The de Medeiros velocities greatly increase the time base and could be expected to offer appreciable refinement.

TABLE II

Radial-velocity observations of HR 3567

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

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O – C)* *km s*$^{-1}$
1986 Nov. 19·19*	46753·19	−1·5	$\bar{7}$·896	+0·5
1987 May 9·87*	46924·87	−9·9	$\bar{6}$·091	−0·4
1993 Feb. 24·93*	49042·93	−9·9	$\bar{4}$·501	−0·5
2002 Dec. 9·18	52617·18	−7·8	0·568	+0·2
2003 Jan. 5·17	52644·17	−7·2	0·598	+0·1
Feb. 18·03	688·03	−6·1	·648	0·0
Mar. 17·84	715·84	−5·3	·680	0·0
Oct. 28·24	940·24	−2·8	·935	−0·1
Nov. 28·20	971·20	−3·7	·970	+0·1
Dec. 27·23	53000·23	−5·3	1·003	0·0
2004 Jan. 30·13	53034·13	−7·3	1·042	−0·1
Feb. 27·99	062·99	−8·5	·075	+0·3
Mar. 30·92	094·92	−10·1	·111	+0·2
Apr. 25·92	120·92	−11·6	·141	−0·3
Oct. 27·19	305·19	−12·1	·350	−0·2
Dec. 18·22	357·22	−11·2	·410	−0·1
2005 Jan. 9·10	53379·10	−10·6	1·435	+0·1
Apr. 3·92	463·92	−8·8	·531	0·0
May 8·89	498·89	−8·0	·571	−0·1
Oct. 10·22	653·22	−3·9	·746	−0·2
Nov. 5·22	679·22	−3·0	·776	+0·1
Dec. 17·21	721·21	−2·5	·824	−0·2
2006 Jan. 29·10	53764·10	−1·9	1·873	0·0
Mar. 1·97	795·97	−2·0	·909	+0·2
May 10·87	865·87	−4·9	·988	−0·3
Oct. 27·21	54035·21	−12·1	2·181	0·0
Nov. 26·22	065·22	−12·4	·215	+0·1
2007 Feb. 2·96	54133·96	−12·4	2·293	+0·1
Apr. 15·87	205·87	−11·6	·375	0·0
Nov. 24·20	428·20	−6·6	·628	0·0
2008 Feb. 9·05	54505·05	−4·4	2·716	+0·1
Oct. 28·25	767·25	−5·6	3·014	+0·2
Dec. 7·24	807·24	−8·2	·059	−0·1
2009 Jan. 2·10	54833·10	−9·6	3·089	−0·1
Feb. 4·09	866·09	−10·7	·126	+0·1
May 17·87	968·87	−12·6	·243	0·0
Dec. 1·17	55166·17	−9·9	·468	+0·2
2011 Dec. 18·17	55913·17	−12·3	4·318	0·0

*Observed with Haute-Provence *Coravel*[1]; weight ¼.

They do not fit too well and have been weighted ¼; the final period is 878·9 ± 0·6 days. The elements are given in full in Table V below, and the orbit is illustrated in Fig. 2. The mass function is small and requires the secondary

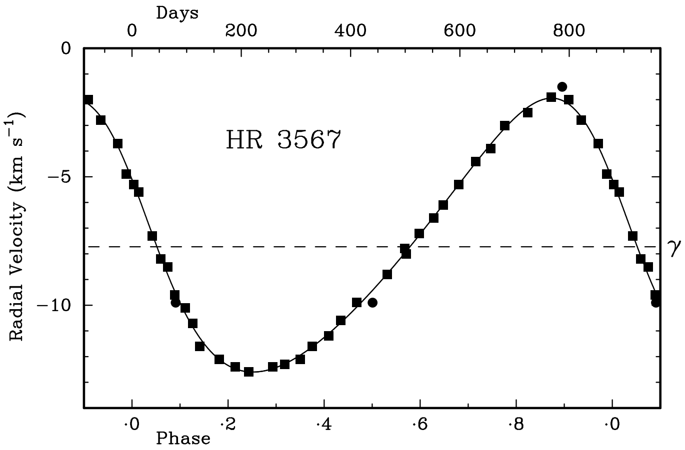

FIG. 2

As Fig. 1, but for HR 3567. In this case there are no other published observations that can be plotted with the Cambridge and OHP ones.

to have a mass little more than 0·4 $M_{\odot}$ if that of the primary is taken as 2 $M_{\odot}$; the companion star could therefore well be a star on the lower main sequence, possibly as far down as about M2.

HR 3907 (*HD 85505*)

HR 3907 is an equatorial star in Sextans, about 4° preceding α Sextantis. At the time of writing this paper it was at a declination slightly more than a single minute north of the celestial equator, across which it will be carried into the southern hemisphere by precession early in 2016. Its magnitude and colours were first given by Cousins[26], as $V = 6^{m}{\cdot}35$, $(B-V) = 0^{m}{\cdot}94$, $(U-B) = 0^{m}{\cdot}68$; results that were identical apart from a $0^{m}{\cdot}01$ difference in V, at $6^{m}{\cdot}34$, were listed by Johnson *et al.*[19]. Rybka[27], however, found a V of $6^{m}{\cdot}38$ and $(U-B)$ of $0^{m}{\cdot}76$, although his $(B-V)$ almost agreed with the previous values. One might think twice before worrying over the discrepancies: Rybka's results were obtained by making corrections to magnitudes previously published by Nekrasova, Nikonov & Rybka[28], which in retrospect clearly lacked their own authors' confidence, and were themselves supplanted, also by Rybka[29], by another set in which they were averaged with other published photometry. The waters are further muddied by a V magnitude of $6^{m}{\cdot}25$ listed by Eggen[23]. There might be a moment's hesitation in dismissing it entirely out of hand as a misprint, because Adelman, Davis & Lee[30] are on record as saying that the *Hipparcos* 'epoch photometry' shows variability with an amplitude of $0^{m}{\cdot}04$. That, however, is the reverse of obvious from the *Hipparcos Catalogue*[31] itself, which records that the r.m.s. spread of the magnitudes from the 130 individual observational epochs was only $0^{m}{\cdot}012$. To put that into perspective, it may be mentioned that only 18 of the 99 other stars listed on the same page of the *Catalogue* as HR 3907 have smaller values. The mean *Hipparcos* magnitude, transformed to the usual (Johnson) V, is $6^{m}{\cdot}34$, in excellent agreement with the values given by those normally unimpeachable authors Cousins[26] and Johnson *et al.*[19] and noted above. Moreover, Percy[32] used HR 3907 as a comparison star in a deliberate survey for photometric instability in K giants and did not identify any variability in it, so it certainly looks as if Adelman *et al.*[30] climbed out onto a limb.

The parallax[7] of HR 3907 — alone among those of interest in this paper — came from an 'acceleration solution' that demonstrates that *Hipparcos* did notice its orbital motion; it also shows that the star has a distance modulus of $5^{m}{\cdot}72 \pm 0^{m}{\cdot}16$ and therefore an absolute magnitude of $+0^{m}{\cdot}62$ with the same uncertainty. Like HR 3567, HR 3907 was observed by Halliday[20], who found a luminosity class of 2·7 by his microphotometer method and classified the star visually as G9 III. The luminosity assessment appears to have been less accurate in this case than in that of HR 3567. Halliday's type appears to be the only real MK classification available. It is disconcerting that *Simbad* attributes a type of K2 III to HR 3907, in obvious conflict with the colour indices. The origin of that error can be identified: the K2 III type actually belongs to μ Leo, whose real *HD* number is smaller by two than that of HR 3907 but was misprinted in Keenan's 1983 revision[33] of MK standard types. In his 1983 list, as well as the 1985 one[34], he attributed the *HD* number of HR 3907 to μ Leo; the latter's number is, however, correctly given in Keenan's other sets of classifications[35–38], both before and after the offending ones, whereas HR 3907 does not feature in any of those listings at all.

Just as in the case of HR 3567, the radial velocity of HR 3907 was first measured at the DDO and published by Young[25]. The mean velocity from four plates is given as +20·1 km s^{-1}, with a 'probable error' of 1·0 km s^{-1}. That result is flagged to indicate that it is "more uncertain than for the general run of stars", and the total range of the four velocities is noted as 9 km s^{-1}. Unfortunately the individual velocities are not given, so it is hard to know how significant a range of 9 km s^{-1} may be. Young's paper gives mean velocities for 681 stars, systematically observed four times each; it was intended to embrace all stars of types A0–M, north of the equator and with $m_{pg} < 8^{m}{\cdot}0$, whose velocities had not been previously published. Of the 681, 36 were definitely regarded as variable in velocity and their individual plate velocities were listed in an auxiliary table; 42 others were flagged like HR 3907, but velocity ranges were given only for 19 of them. HR 3907's range of 9 km s^{-1} is the second-smallest, the only lower one being the 8 km s^{-1} given for HD 59878 (HR 2879, which is certainly a binary, its orbit having been given in Paper 119[39] of the present series). It would be dangerous to presume that the remaining 23 flagged objects had velocity ranges smaller than that. One might learn something by comparing the 'probable errors' of the mean values, but those will naturally depend also on the natures of the spectra, which cover a wide range of types: it is only to be expected that the dispersion of the plate velocities for stars classified as A0n, for instance, will be a lot larger than those of K giants for reasons unrelated to duplicity, so an assessment based on errors of the means would probably be an arid exercise. Without attempting any such detailed and possibly misleading assessment, however, we might charitably acknowledge that Young deserves to be recognized as having made a case for HR 3907 being a binary system.

After a lapse of more than 50 years, de Medeiros and his collaborators re-asserted[1,15] the duplicity of the object, referring to four OHP *Coravel* velocities that were definitely discordant beyond the bounds of observational error, and placing the individual results at our disposal through the Centre de Données

TABLE III

Radial-velocity observations of HR 3907

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

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O – C)* *km s*$^{-1}$
1986 Nov. 20·17*	46754·17	+24·0	$\bar{3}$·375	+0·2
1987 May 11·87*	46926·87	21·8	$\bar{3}$·477	–0·4
1992 Jan. 30·06*	48651·06	21·3	$\bar{2}$·504	–0·5
1993 Mar. 16·94	49062·94	17·0	$\bar{2}$·750	–0·3
2002 Dec. 9·19	52617·19	15·8	0·867	0·0
2003 Jan. 5·18	52644·18	16·0	0·883	+0·2
Feb. 20·02	690·02	15·9	·910	–0·1
Mar. 19·99	717·99	16·2	·927	–0·2
Apr. 16·90	745·90	16·8	·943	0·0
Nov. 4·22	947·22	22·2	1·063	–0·4
Dec. 8·18	981·18	23·3	·083	–0·1
2004 Jan. 9·13	53013·13	24·1	1·102	+0·1
Feb. 9·07	044·07	24·6	·121	+0·1
Mar. 16·99	080·99	24·8	·143	–0·1
Apr. 13·92	108·92	25·0	·160	–0·1
May 16·87	141·87	25·2	·179	–0·1
Nov. 13·26	322·26	24·8	·287	–0·1
Dec. 18·23	357·23	24·6	·307	–0·1
2005 Jan. 9·10	53379·10	24·7	1·320	+0·2
Mar. 25·96	454·96	23·6	·366	–0·3
Apr. 21·92	481·92	23·7	·382	0·0
May 14·87	504·87	23·5	·395	0·0
Nov. 5·26	679·26	22·1	·499	+0·2
Dec. 17·22	721·22	21·6	·524	+0·2
2006 Jan. 29·11	53764·11	21·0	1·550	0·0
Mar. 1·97	795·97	20·6	·569	–0·1
Apr. 3·98	828·98	20·1	·588	–0·2
May 10·87	865·87	19·9	·610	0·0
Oct. 27·21	54035·21	17·8	·711	–0·2
Nov. 26·22	065·22	17·7	·729	0·0
Dec. 9·29	078·29	17·5	·737	0·0
2007 Jan. 14·17	54114·17	16·9	1·758	–0·2
Feb. 7·10	138·10	17·0	·773	+0·1
Apr. 15·88	205·88	16·2	·813	–0·1
May 12·87	232·87	16·3	·829	+0·2
2008 Jan. 6·20	54471·20	17·9	1·971	+0·1
Feb. 2·08	498·08	18·6	·987	0·0
Mar. 17·96	542·96	20·1	2·014	+0·1
Apr. 17·94	573·94	21·0	·032	0·0
May 8·87	594·87	21·9	·045	+0·2
Dec. 7·24	807·24	25·3	·171	+0·1
2009 Feb. 4·10	54866·10	25·6	2·206	+0·3
Mar. 5·02	895·02	25·3	·223	0·0
Apr. 8·89	929·89	25·3	·244	+0·1
May 8·88	959·88	+24·9	·262	–0·2

TABLE III (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2010 Jan. 31·09	55227·09	+23·1	2·421	0·0
Feb. 27·03	254·03	23·1	·437	+0·2
Apr. 4·94	290·94	22·5	·459	0·0
May 10·86	326·86	22·1	·481	−0·1
2011 Jan. 19·14	55580·14	19·6	2·631	+0·1
Apr. 6·97	657·97	18·7	·678	+0·1
May 9·88	690·88	18·4	·697	+0·2
2012 Jan. 27·10	55953·10	+15·8	2·854	−0·1

*Observed with Haute-Provence *Coravel*[1]; weight ¼.

Stellaires. The four velocities are listed at the head of Table III, which otherwise consists of 49 measurements obtained with the Cambridge *Coravel*. As in the case of HR 3567, the Cambridge velocities have been adjusted by −0·2 km s^{-1}. By themselves, they yield an orbital period of 1674 ± 4 days; when the OHP measurements from the 1980s are brought into the solution, with weight ¼, they refine it to 1678·9 ± 2·8 days. A strict equalization of the variances of the two data sources would require the OHP velocities to be down-weighted further, and the change to the period would then be less, so the quoted formal uncertainty may be a little optimistic. The complete set of orbital elements is given in Table V, and Fig. 3 plots the orbit.

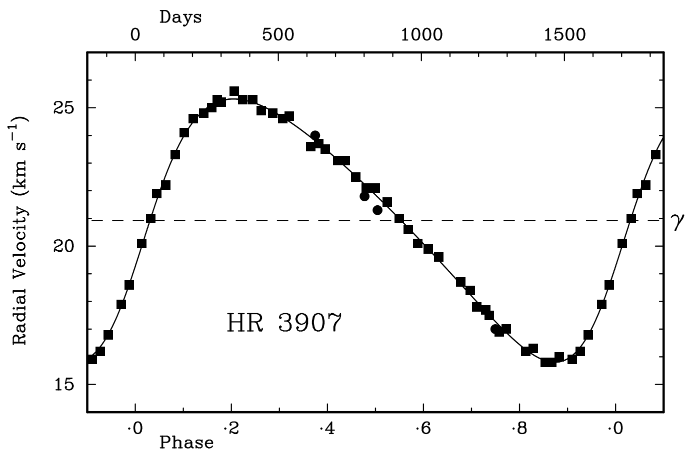

FIG. 3

As Fig. 2, but for HR 3907. Unlike the other three velocity curves shown in this paper, the *rising* side of this one is the steep side: here the longitude of periastron, ω, is near 270° instead of near 90°.

Once again the orbital period, in this case 4·60 years (by coincidence very close to that of HR 1313), is such as to facilitate a uniform phase distribution of observing epochs. The mass function is only a little more than that of HR 3567, and does not require the secondary to be as much as half a solar mass (about type M0 for a main-sequence star) if a 2-$M_{\odot}$ primary is assumed.

HR 6239 (*HD 151627*)

HR 6239 is in the southern part of Hercules, about 7° preceding and 1° south of α Herculis. As in the case of HR 1313 above, the only reference given by *Simbad* for broad-band magnitudes is a 'private' one, but again the magnitudes were in fact published by Häggkvist & Oja[4], whose paper gives $V = 6^{m}{\cdot}35$, $(B-V) = 0^{m}{\cdot}875$. The parallax[7] corresponds to an absolute distance modulus of $6^{m}{\cdot}72 \pm 0^{m}{\cdot}31$, thus indicating an M_V of about $-0^{m}{\cdot}4 \pm 0^{m}{\cdot}3$, without any allowance for interstellar absorption. Both Harlan[40] and Cowley & Bidelman[41] listed HR 6239's spectral type as G5 III. Gottlieb & Bell[42] included the star in a paper, based on spectral scans obtained at the Lick Observatory with the 'Wampler scanner'[43] and published by Spinrad & Taylor[44], that was primarily concerned with elemental abundances in late-type giants. In the case of HR 6239 they appear considerably to have over-estimated the star's luminosity, finding it to have an absolute magnitude of $-2^{m}{\cdot}17$, which in turn led to excessive values for its distance, radius, and mass. Their figure of 4·4 $M_{\odot}$ for the mass is not, in proportional terms, very much larger than the 3·8 $M_{\odot}$ proposed by de Laverny *et al.*[45], but the latter authors arbitrarily fixed so many of their model parameters that their result lacks independence.

Radial-velocity measurements of HR 6239 were published in the 1930s from both the Pulkovo Observatory's outstation at Simeis and from the DAO. From Simeis, Shajn & Albitzky[46] reported a mean velocity of +1·0 km s^{-1} with a 'probable error' of 0·5 km s^{-1}, from four plates taken with a prismatic spectrograph giving a reciprocal dispersion of 36 Å mm^{-1} at Hγ. The individual results were given in a later paper[47] from Pulkovo, and show that all four observations were made within a total span of ten days, so they are not of much help in documenting a leisurely spectroscopic binary like HR 6239. From the DAO, Harper[48] gave four velocities obtained with comparable instrumentation; their mean velocity and its uncertainty was not calculated, as for most of the other stars in Harper's tabulation it *was*, but instead a note says, "Though third plate is weak, binary character is suspected.". The weak plate, taken only three weeks after its predecessor, disagrees with it by more than 14 km s^{-1}; in the orbit presented below, it is seen to be *wildly* in error, and if it is rejected then the velocity range is almost halved, so it is not clear that Harper could validly be credited with discovering the binary nature of HR 6239. De Medeiros & Mayor[1], however, writing 60-odd years after the Pulkovo and DAO authors, found a discordance of 9 km s^{-1}, which was certainly significant, between two OHP *Coravel* observations that they later made available individually, and it was those that galvanized the present author into obtaining another 46 measures with the Cambridge *Coravel*. All the available velocities are listed chronologically in Table IV. The old observations have all been adjusted by +0·8 km s^{-1}, plus, in the case of the DAO ones, the correction of +1·0 km s^{-1} that the compiler of the *Radial Velocity Catalogue*[24] found to be warranted for observations of G-type stars from that source. The Cambridge measures have been adjusted by –0·2 km s^{-1}.

TABLE IV

Radial-velocity observations of HR 6239

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

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O – C)* *km s*$^{-1}$
1923 June 15·24*	23585·24	+3·5	$\overline{20}$·940	+0·1
1930 June 29·89*	26156·89	−0·1	$\overline{18}$·660	−4·4
July 1·85*	158·85	+2·7	·661	−1·6
7·85†	164·85	+3·1	·665	−1·4
9·83†	166·83	+1·3	·666	−3·2
1931 June 6·33†	26498·33	+9·1	$\overline{18}$·888	+2·2
28·28†	520·28	−5·3	·903	−11·5
1933 July 15·27*	27268·27	+0·8	$\overline{17}$·403	+6·2
1986 June 4·97‡	46585·97	−8·7	$\overline{4}$·325	−0·2
1987 May 4·09‡	46919·09	+0·3	$\overline{4}$·548	+0·1
2002 May 29·95	52423·95	−11·8	0·230	+0·1
July 13·98	468·98	−10·4	·260	+0·5
Aug. 27·87	513·87	−9·7	·290	+0·1
2003 Feb. 18·22	52688·22	−5·3	0·407	0·0
Apr. 7·14	736·14	−4·5	·439	−0·5
May 6·06	765·06	−3·3	·458	−0·1
22·07	781·07	−2·6	·469	+0·2
June 24·99	814·99	−1·9	·491	0·0
July 20·94	840·94	−0·9	·509	+0·4
Aug. 26·89	877·89	−0·4	·534	−0·1
Sept. 14·84	896·84	−0·1	·546	−0·3
Oct. 16·77	928·77	+0·8	·568	−0·2
2004 Mar. 1·23	53065·23	+4·3	0·659	+0·1
Apr. 7·15	102·15	+5·1	·684	0·0
May 7·10	132·10	+5·9	·704	+0·2
June 13·03	169·03	+6·6	·728	+0·2
Aug. 12·94	229·94	+7·5	·769	+0·1
Sept. 8·85	256·85	+7·7	·787	−0·1
Oct. 25·75	303·75	+8·1	·818	0·0
Nov. 13·71	322·71	+7·9	·831	−0·2
2005 Jan. 23·29	53393·29	+7·2	0·878	−0·1
Mar. 23·20	452·20	+5·3	·918	+0·1
May 5·08	495·08	+2·7	·946	0·0
June 1·02	522·02	+0·7	·964	0·0
23·02	544·02	−1·0	·979	+0·1
July 16·95	567·95	−3·2	·995	0·0
Aug. 6·89	588·89	−4·9	1·009	+0·1
25·92	607·92	−6·8	·022	−0·2
Sept. 14·81	627·81	−8·3	·035	−0·2
Oct. 4·78	647·78	−9·4	·049	+0·1
25·75	668·75	−10·5	·063	+0·2
Nov. 18·70	692·70	−11·7	·079	+0·1
2006 Mar. 2·22	53796·22	−13·7	1·148	−0·2
Apr. 4·14	829·14	−13·4	·170	0·0
May 11·07	866·07	−12·9	·195	0·0

TABLE IV (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)* *km s⁻¹*
2007 Feb. 4·27	54135·27	−6·4	1·375	+0·1
2008 Jan. 6·29	54471·29	+1·9	1·599	−0·2
Feb. 2·25	498·25	+2·8	·617	0·0
Mar. 5·23	530·23	+3·8	·639	+0·2
Aug. 10·91	688·91	+6·9	·745	0·0
Oct. 31·75	770·75	+7·8	·800	−0·1
2009 Feb. 4·28	54866·28	+7·9	1·864	+0·2
Mar. 27·18	917·18	+6·5	·898	0·0
May 20·07	971·07	+3·7	·934	−0·2
2010 Mar. 23·19	55278·19	−13·9	2·139	−0·4
2011 June 17·06	55729·06	−4·1	2·441	−0·2

* Observed with DAO 72-inch telescope[48]; weight 0.
† Observed with Simeis 40-inch telescope[46,47]; wt. 0.
‡ Observed with Haute-Provence *Coravel*[1]; weight ¼.

An orbit based on the present writer's observations alone has a period of 1495·5 ± 2·2 days; it is reduced by half a day and its standard deviation by about half a day, to 1495·0 ± 1·6 days, by the addition of the two OHP data points, with weight ¼ as for the other stars. The two early sets of data would merit such small weights and are so poorly distributed in phase that they have not been included in the adopted solution of the orbit. It may be mentioned, however, that if they are introduced with weights of 0·01 (which is more than their residuals would entitle them to, even with the 'wild' DAO measure rejected) then the computed orbital period is reduced by a further 0·7 day and there is little effect on the other elements. The orbit is shown in Fig. 4, and its solution is given, with the elements of the three orbits already discussed above, in Table V.

TABLE V

Orbital elements for the four stars

Element	*HR 1313*	*HR 3567*	*HR 3907*	*HR 6239*
P (days)	1658·7 ± 2·9	878·9 ± 0·6	1678·9 ± 2·8	1495·0 ± 1·6
T (MJD)	53848 ± 5	53876 ± 5	54520 ± 6	53575·2 ± 2·7
γ (km s^{-1})	−42·42 ± 0·03	−7·73 ± 0·03	+20·92 ± 0·03	−2·39 ± 0·03
K_1 (km s^{-1})	6·10 ± 0·04	5·33 ± 0·04	4·76 ± 0·03	10·82 ± 0·04
e	0·309 ± 0·005	0·210 ± 0·007	0·279 ± 0·006	0·295 ± 0·004
ω (degrees)	31·2 ± 1·2	66·0 ± 2·2	254·3 ± 1·5	95·9 ± 0·8
$a_1 \sin i$ (Gm)	132·3 ± 0·9	63·0 ± 0·5	105·5 ± 0·8	212·6 ± 0·9
$f(m)$ ($M_\odot$)	0·0336 ± 0·0007	0·0129 ± 0·0003	0·0166 ± 0·0004	0·1718 ± 0·0022
R.m.s. residual (wt. 1) (km s^{-1})	0·17	0·16	0·15	0·18

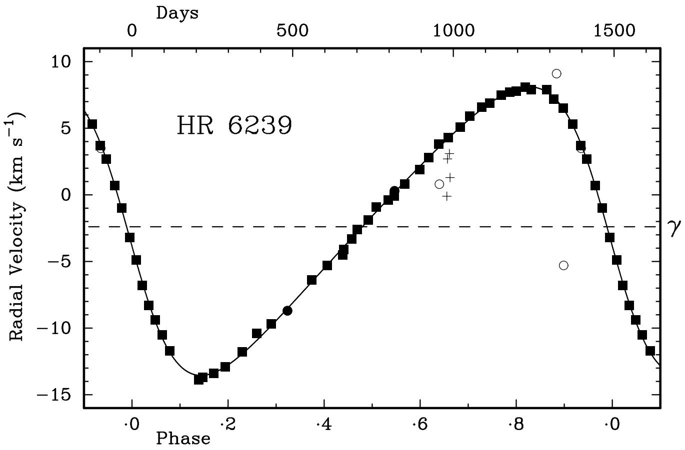

FIG. 4

As Fig. 1, but for HR 6239. Open circles plot the four early DAO observations (one of them, at +3·5 km s^{-1}, is nearly hidden); the very discordant point at –5 km s^{-1} stems from what the observer[48] called a 'weak plate' but he did not reject it on that account. The four plusses plot the Simeis[46,47] observations, which were made very close together in time.

The orbital period of 4·10 years is in this case unhelpful as far as obtaining good phase coverage of the orbit is concerned, but the right ascension of the star puts conjunction near the winter solstice, and despite its modest declination of +13° the star is as much as 36° north of the ecliptic; the only month of the year when it is actually out of reach of the Cambridge telescope is December, and the only evidence of that in Fig. 4 consists of two very modest *lacunae* near phases ·1 and ·35. The most noteworthy feature of the orbit is the mass function, which for a 2-$M_\odot$ primary would call for a secondary with a mass of at least 1·2 $M_\odot$ (so no later than F8 if it is a main-sequence star). If the primary were really near 4 $M_\odot$, then the minimum secondary mass would be 1·8 — about A8. No evidence of any secondary has been visible in the radial-velocity traces and none is visible as any inflection in the velocity curve in Fig. 4. The writer has not seen an actual spectrum of HR 6239, but a recollection of the difficulty of seeing the dF secondary in the somewhat analogous system 73 Leo[49] dissuades him from asserting that the companion to HR 6239 could not be such a star. Even well into the near-ultraviolet where the late-type giant, which at K3 in 73 Leo must be considerably fainter than the primary of HR 6239, it was almost impossible to make out the contribution of the secondary except as a slight raising of the residual light level in deep lines and molecular bands.

δ Boo (*HD 135722*)*, HD 146815, 64 Aql* (*HD 191067*)*, and 75 Dra* (*HD 196787*)

In Paper 186[50] of this series, dealing with 56 Peg, it was shown that one observation that was given of that star by de Medeiros & Mayor in the listing that they deposited with the Centre de Données Stellaires, and correspondingly summarized in their paper[1], is mistaken. A footnote in the 56 Peg paper noted other probable instances of isolated mistakes in the Centre de Données Stellaires listing, in the data on δ Boo, τ CrB, HD 146815, and 75 Dra. A sixth case that has already been brought[2] to attention tentatively identified two observations attributed by de Medeiros & Mayor[1] to HDE 276743 as being really of an adjacent star, HD 29957. A recent paper in the same series as *this* one (no. 222[51]) has characterized one of de Medeiros' & Mayor's observations of HR 8026 (HD 199612) as 'wild'. Still other instances, not previously identified, have been noticed in respect of σ Cet (HD 15798) and 64 Aql (HD 191067). The former, at −15°, is beyond the range of declination accessible to the Cambridge telescope, but the agreement of six Lick[52] velocities obtained in 1899–1923 with five of the six in the de Medeiros & Mayor list makes it practically certain that the isolated value that is discordant by 10 km s^{-1} is a mistake of some sort. 64 Aql joins the four stars that featured in the 56 Peg footnote[50] in being treated more specifically below.

In such a huge listing as that of de Medeiros & Mayor, the number of probable mistakes identified is remarkably small, and no criticism or offence is intended by drawing attention to them, only assistance to others who might well suppose that the objects concerned must be spectroscopic binaries and might initiate unfruitful studies of them (as the writer has in fact done).

Table VI gives, side by side, the radial-velocity measurements in the list supplied by de Medeiros & Mayor[1] to the Centre de Données Stellaires, and those obtained by the writer subsequently. In each case it will be seen that the writer's series suggests that the velocity is constant, and that (allowing for the usual offset of the order of 1 km s^{-1} for K stars and somewhat more for G types) all but one of the CDS measures supports that constancy, and the sole discordant one is a seriously discrepant outlier whose validity may be questioned. It might be surmised that the most likely source of error would be misidentification of the star by the observer concerned, although it is beyond the remit or the power of the present writer to propose revised object identities for all the offending observations, as he has felt able to do in the particular cases of HDE 276743[2] and τ CrB (below). He points out, however, that the anomalous velocity credited to 75 Dra is exactly appropriate to the adjacent object 74 Dra (HD 196925). The certainty of the reassignment of particular observations to other objects could be greatly enhanced if full details of the observation were available, so that the depths and widths of the cross-correlation dips, and the typical photon-counting rates, given by the originally assigned star and the suggested alternative, could be compared with the corresponding parameters of the observation in question.

The final case to be discussed, of errors in the de Medeiros & Mayor list, occurs in the velocities given for τ CrB (HR 6018, HD 145328). The value of –36·51 km s^{-1} that is shown for HJD [actually HJD – 2 400 000] 46997 is far outside the actual range of velocities of that star, which was suspected[52] to be a spectroscopic binary as long ago as 1928, was definitely asserted to be such in the 1953 *Radial Velocity Catalogue*[24], and remains under observation by the writer. In this instance it may be proposed with some confidence that the offending velocity should be ascribed instead to HR 6046 (HD 145849), which the de Medeiros & Mayor listing admits to having been observed 0·009 day (13 minutes) later, when it gave a velocity of –36·98 km s^{-1} with a very weak secondary at a very uncertain −24·53. HR 6046 is a star about half a degree following τ CrB and in almost the same declination; it is a known spectroscopic binary for which a very rough approximation to a single-lined orbit was given[53] in 1934 and a rough one[54] in 1936, both by Christie. The present writer recognized it as double-lined at a nodal passage in 1997, and was a member of the consortium that published[55] a double-lined orbit for it 2007. The false observation attributed to τ CrB fits very neatly into that orbit, just where one in which the weak secondary was not explicitly recognized could be expected to fall.

TABLE VI

Radial Velocities of δ Boo, HD 146815, 64 Aql, and 75 Dra

De Medeiros & Mayor[1] *Date (UT)*	*RV (km s*$^{-1}$*)*	*Cambridge Coravel Date (UT)*	*RV (km s*$^{-1}$*)*
	δ Boötis (HD 135722)		
1979 Feb. 19·21	−11·87	2005 Jan. 9·28	−11·0
Apr. 16·04	−21·82	May 11·08	−11·1
18·05	−12·05	2006 July 3·95	−10·9
June 15·93	−11·81	2007 May 31·01	−10·9
July 9·89	−12·14	2008 May 22·00	−10·7
1986 June 5·00	−12·37	2009 June 20·91	−10·9
1987 May 9·99	−12·16	2010 July 31·89	−10·8
1991 Feb. 13·21	−12·10	2011 June 17·00	−10·9
Apr. 29·00	−12·38		
1992 Feb. 14·19	−11·95		
1993 May 28·99	−11·92		
	HD 146815		
1986 May 26·00	+28·16	2002 May 29·95	+30·6
July 16·89	+79·32	July 13·97	+30·7
1987 May 10·08	+29·30	Aug. 14·90	+30·6
		Sept. 10·84	+30·4
		2003 May 6·05	+30·7
		July 20·93	+30·7
		2004 June 16·96	+30·9
		2005 May 29·04	+30·6
		2006 July 3·97	+30·6
		2007 June 27·99	+30·7
		2008 May 22·01	+30·7
		2009 June 1·07	+30·3
		2011 June 27·00	+30·6
	64 Aquilae (HD 191067)		
1987 Aug. 31·89	−3·57	2002 May 31·11	−2·8
1988 July 18·00	+2·17	July 15·02	−2·5
		Aug. 14·98	−2·1
		Sept. 10·89	−2·4
		Nov. 12·78	−2·5
		2003 May 28·09	−2·4
		July 13·03	−2·1
		Sept. 14·91	−2·0
		Nov. 5·79	−2·6
		2004 June 28·06	−2·4
		Sept. 13·88	−2·3
		Nov. 14·71	−1·8
		Dec. 17·70	−2·6
		2005 June 27·08	−2·1
		2006 July 13·04	−2·1
		2007 Oct. 23·83	−2·2
		2008 Aug. 15·01	−2·4
		2009 Aug, 17·99	−2·3
		2010 Sept. 3·94	−2·3
		2011 Sept. 29·84	−2·1

TABLE VI (*concluded*)

75 Draconis (HD 196787)

De Medeiros & Mayor[1] Date (UT)			RV (km s^{-1})	Cambridge Coravel Date (UT)			RV (km s^{-1})
1979	June	25·08	−4·42	2004	Dec.	21·73	−2·6
1980	July	22·99	−4·97	2005	Apr.	22·09	−3·4
1982	Sept.	15·85	−4·42		June	1·10	−3·7
	Oct.	20·85	−4·21		Oct.	29·78	−3·4
1983	July	11·07	−4·41		Dec.	19·77	−2·8
	July	27·98	−4·81	2006	July	4·04	−3·2
	Aug.	7·99	−4·43	2007	Nov.	6·73	−3·3
	Sept.	4·93	−4·52	2008	July	25·06	−3·3
	Oct.	10·79	−4·83	2009	Aug.	30·03	−3·0
1986	Aug.	17·01	−14·79	2010	Sept.	3·05	−3·1
1987	Aug.	9·93	−3·78	2011	Sept.	30·94	−3·0
1989	June	9·06	−4·49				

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COOL DWARFS IN WIDE MULTIPLE SYSTEMS

PAPER 3: TWO COMMON-PROPER-MOTION LATE-TYPE STARS SEPARATED BY OVER 11 ARCMINUTES

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LP 209–28 and LP 209–27 have similar proper motions as tabulated in several catalogues. Using seven astrometric epochs spanning 59 years, we confirm a common tangential velocity by measuring a constant angular separation of $\rho = 666''\cdot62 \pm 0''\cdot09$. Accurate SDSS and 2MASS photometry indicates that they are normal dwarfs of approximate spectral types K7 V and M3 V. However, from their apparent magnitudes, both LP 209–28 and LP 209–27 are located at 200–250 pc, from which one can deduce an astonishing projected physical separation of 0·6–0·8 pc. The system Koenigstuhl 6 AB represents another record among the least-bound systems with low-mass-star components.

Koenigstuhl 6 AB: an extremely fragile system of two late-K/early-M dwarfs

After the discovery of Koenigstuhl 4 AB (two mid-M dwarfs in a loosely-bound common-proper-motion pair)[1] and Koenigstuhl 5 AB-C (a distant M8·5 V companion to a binary solar-type star)[2], now we present the third paper of the series 'Cool dwarfs in wide multiple systems', with the identification of two red Luyten stars sharing the same proper motion and being separated by over 11′. We call it Koenigstuhl 6 AB (abridged: KO6AB) and it is one of the most prominent outcomes of a professional–amateur virtual-observatory search for companions to Luyten stars[3].

LP 209–28 (KO6A) and LP 209–27 (KO6B) are two poorly-known red stars for which Luyten[4,5] estimated spectral types M from B_J and R_F photographic magnitudes. Available information in the literature on the stars is optical and near-infrared magnitudes and proper motions only[6,7]. None of them are tabulated in the *Washington Double Star Catalogue*[8]. We compile in Table I their basic data, including coordinates and proper motions from the PPMXL Catalogue of Positions and Proper Motions[9], optical photometry from the SDSS Sloan Digital Sky Survey[10], and near-infrared photometry from the 2MASS All-Sky Survey[11]. The PPMXL proper motion, of over 200 mas yr^{-1}, is identical, within uncertainties, to other proper motions tabulated in the literature[12,13].

We measured angular separations, ρ, and position angles, θ, for seven astrometric epochs between 1953 and 2012 (Table II). The six first epochs corresponded to the *SuperCOSMOS* digitizations of the Palomar Observatory Sky Survey[14], 2MASS, and SDSS (marked with an asterisk), which was actually the mean of two SDSS observations separated by 24 days. The seventh and last astrometric epoch was taken by us on 2012 February 15 using a dual CCD camera SBIG ST-8XME at the 0·4-m Telescopio del Observatorio de Tacande on La Palma, Spain (Minor Planet Center code J22; see http://www.astropalma.com/), as described by Caballero *et al.*[15]. The camera provided an 18′·1 × 12′·1

TABLE I

Basic data for Koenigstuhl 6 A and B

Datum	*A*	*B*	*Origin*
Name	LP 209–28	LP 209–27	Luyten
NLTT	19878	19858	Luyten
α (J2000)	08^h 37^m 04^s·69	08^h 36^m 37^s·43	PPMXL
δ (J2000)	+39° 07′ 59″·8	+38° 58′ 13″·6	PPMXL
$\mu_\alpha \cos\delta$ [mas yr^{-1}]	−126·2 ± 4·0	−123·9 ± 4·0	PPMXL
μ_δ [mas yr^{-1}]	−194·4 ± 4·0	−182·0 ± 4·0	PPMXL
u [mag]	19·152 ± 0·028	21·111 ± 0·111	SDSS
g [mag]	16·615 ± 0·004	18·841 ± 0·009	SDSS
r [mag]	15·134 ± 0·004	17·456 ± 0·006	SDSS
i [mag]	14·602 ± 0·009	16·787 ± 0·005	SDSS
z [mag]	13·981 ± 0·004	15·339 ± 0·006	SDSS
J [mag]	12·816 ± 0·022	14·009 ± 0·026	2MASS
H [mag]	12·227 ± 0·022	13·478 ± 0·029	2MASS
K_s [mag]	12·054 ± 0·020	13·246 ± 0·031	2MASS
Phot. Sp. Type	K7: V	M3: V	This work

TABLE II

Astrometric observations of the Koenigstuhl 6 AB system

Epoch	*ρ (arcsec)*	*θ (deg)*	*Origin*
1953 Mar. 16	666·72	208·417	POSS I Red
1989 Mar. 09	666·62	208·410	POSS II Red
1991 Feb. 04	666·66	208·423	POSS II Infrared
1994 Jan. 11	666·62	208·413	POSS II Blue
1998 Apr. 04	666·51	208·416	2MASS
2001 Dec. 03*	666·71	208·430	SDSS
2012 Feb. 15	666·48	208·395	This work
Average	666·62 ± 0·09	208·415 ± 0·011	This work

field of view with a pixel size of 0″.71 and first-order adaptive optics (tip-tilt). We took three consecutive exposures of 300 s in the SDSS r' band when the pair culminated (air mass = 1·02) and astrometrically calibrated the stacked image with background 2MASS sources of null proper motion. As summarized in the last row of Table II, both ρ and θ between the two stars kept constant with very small standard deviations, of only 0″·09 and 0°·010, respectively. Thus, KO6A and KO6B share the same high proper motion.

From the SDSS and 2MASS photometry of the two stars in Table I and a simple inspection of a very large sample of cool dwarfs with the same photometry and low-resolution spectroscopy[16], we estimated spectral types K7: V and M3: V for KO6A and KO6B, respectively. Uncertainties are less than two spectral subtypes.

Instead of assuming a spectral-type–absolute-magnitude relation, in Table III we derived the basic astrophysical parameters of KO6AB as functions of distance. Masses and effective distances were obtained from the J-band absolute magnitude and NEXTGEN98 theoretical models of the Lyon group[17]. Derived effective temperatures were too cool and too warm for K7 V and M3 V stars at heliocentric distances d = 150 and 300 pc, respectively. Only at d = 200–250 pc, spectral types estimated from SDSS–2MASS colours and effective temperatures from the J-band absolute magnitudes matched within reasonable uncertainties. Next, we assumed that the pair KO6AB is located at a distance in the 200–250-pc range. A more precise determination of the heliocentric distance shall be obtained by an accurate spectral-type determination together with the use of a reliable spectral-type–absolute-magnitude relationship[18,19,20] or, far better, from a direct parallax measurement, in particular by the ESA *Gaia* mission.

At the assumed distance range, the angular separation translates into a

TABLE III

Basic data for the Koenigstuhl 6 AB system as functions of distance

	A	*B*	*A*	*B*	*A*	*B*	*A*	*B*
d [pc]	150		200		250		300	
s [AU]	100000		133300		166700		200000	
$m–M$ [mag]	5·880		6·505		6·990		7·386	
M_J [mag]	6·936	8·128	6·311	7·504	5·826	7·019	5·430	6·623
M [$M_\odot$]	0·44:	0·25:	0·54:	0·34:	0·61:	0·43:	0·68:	0·49:
T_{eff} [K]	3570:	3380:	3730:	3470:	3940:	3560:	4130:	3640:
M_{total} [$M_\odot$]	0·69:		0·88:		1·04:		1·17:	
$-U^{\star}_g$ [10^{33} J]	−1·9		−2·4		−2·8		−2·9	

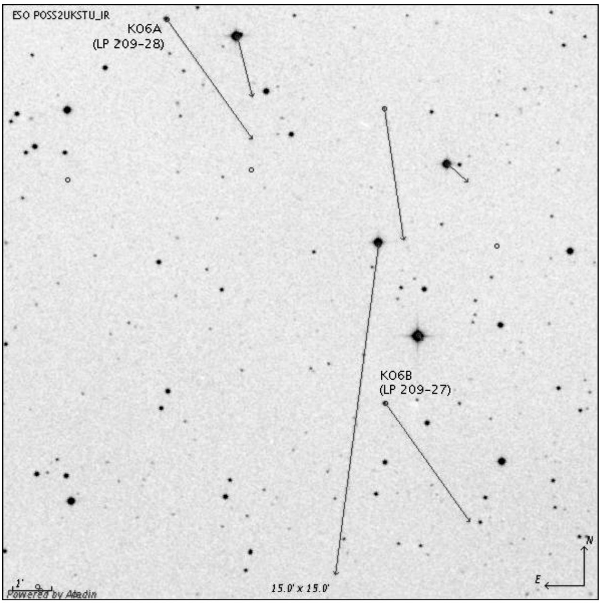

FIG. 1

Colour-inverted *UKST* I_N-band image provided by *The Digitized Sky Survey* from the European Southern Observatory at Garching, constructed with *Aladin* showing Koenigstuhl 6 A (LP 209–28; top left) and B (LP 209–27; bottom right), which are labelled. The long arrows indicate the proper motions of stars as tabulated by *Simbad*. The field of view is 15′ × 15′; the orientation is indicated on the image. The star with the highest proper motion, roughly in the middle of the field, is the foreground M:-type star G 115–11.

projected physical separation of 0·6–0·8 pc. While wider systems have been proposed recently[21,22,23], the total mass of KO6AB is lower than them (*i.e.*, the Zubenelgenubi multiple system[20] has around 6·7 $M_\odot$, in comparison with the approximately 0·88–1·04 $M_\odot$ of KO6AB). This makes our system have a very low binding energy $U_g^\star$ (which is proportional to the product of component masses and inversely proportional to the *projected* physical separation[21]), at the boundary between very wide binaries and stellar kinematic groups, which seems to lay at approximately -10^{33} J (see again works by Caballero[21,22]). The scarcity of data on KO6A and KO6B prevents us from inferring any age, which usually comes from X-ray, spectroscopic (Hα, Li I), or kinematic (*U*, *V*, *W*) data. Few young moving associations are known further than 100 pc (*e.g.*, Torres *et al.*[24] and references therein). A radial-velocity determination for both stars will erase any doubt on their common velocity and help establish their original stellar population in the Galaxy. In other words, we do not know yet whether KO6AB is an old binary in the disc or halo caught in disintegration or a relatively young pair of stars originating from the same parental cloud, from where they were ejected in the same direction, but, in any case, KO6AB represents another milestone in the search for the least-bound binary systems.

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THE RELATIONSHIP BETWEEN CHROMOSPHERIC CALCIUM II AND CORONAL X-RAY EMISSION AMONG MAIN-SEQUENCE STARS

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The relationship between coronal soft-X-ray emission and chromospheric Ca II *H* and *K* emission is studied using data from the literature for a sample of over 200 field dwarfs ranging in spectral type from mid-F to early-K. Emission measures are normalized to the bolometric luminosity and power-law fits are determined with a view to investigating whether the fits are sensitive to the spectral type and luminosity of dwarf stars. Upon restricting attention to stars having absolute magnitudes within $0^{m}\cdot5$ of the zero-age main sequence, it is found that among mid-to-late G dwarfs the normalized soft-X-ray luminosity varies more strongly with *HK* luminosity than for either late-F or early-K dwarfs. The fits are found to be sensitive to stellar luminosity, and the inclusion of stars removed by $0^{m}\cdot5$–$1^{m}\cdot0$ from the zero-age main sequence has the effect of not only introducing more scatter, but also reducing the power-law index of the fit at a given spectral type. When stars within $0^{m}\cdot5$ of the zero-age main sequence are considered, the late-F dwarfs exhibit a greater scatter about the mean emission–emission relationship than G dwarfs.

Introduction

Among the most commonly used observational measures of the degree of chromospheric and coronal activity among main-sequence stars are the emission in the cores of the Ca II *H* and *K* lines and thermal emission at soft-X-ray wavelengths, respectively. Large studies have been made of the correlations between observed emission fluxes (often normalized to bolometric flux) and fundamental stellar properties such as rotation period and/or age. For example, Mamajek & Hillenbrand[1] studied extensively how both Ca II *H+K* emission and soft-X-ray emission correlate with stellar age, rotation period, and Rossby number. Wright *et al.*[2] documented the correlation between soft-X-ray emission and stellar rotation as codified in the Rossby number.

Such studies imply that the chromospheric and coronal emissions from main-sequence stars should correlate, and a number of observational programmes have found this to be the case[3,4]. Previous studies of the relationships between Ca II *H+K* emission and coronal soft-X-ray emission include Schrijver[4,5], Rutten *et al.*[6], Hempelmann, Schmitt & Stępień[7], Piters *et al.*[8], Sterzik & Schmitt[9], and Mamajek & Hillenbrand[1]. Several of these studies, *e.g.*, Hempelmann *et al.*[7], have concluded that there is an optical colour dependence in the X-ray *versus H+K* flux correlation for main-sequence stars. It has been suggested[5,7] that this stems from a non-magnetic-dynamo contribution to the heating of the chromosphere, and that this contribution is a function of stellar effective temperature.

There are many measurements of both Ca II *H+K* emission and soft-X-ray emission for main-sequence stars available in the literature. A widely-used index quantifying the chromospheric activity of late-type dwarfs is the parameter R'_{HK}, based originally on data acquired by the Mount Wilson *HK* survey[10,11,12]. This parameter is defined[13] as the ratio between the summed flux in the chromospheric components of the *H* and *K* emission lines and the stellar bolometric flux, and is corrected for the component of those emission lines that originates within the photosphere. In a previous paper published in this journal, Smith, Redenbaugh & Jones[14] reported upon a compilation of $\log_{10} R'_{HK}$ data from the literature for over 2800 dwarf stars. The same compilation is used in the present paper to document anew the relationship between chromospheric Ca II *H+K* emission and coronal soft-X-ray emission.

The sample of stars

As discussed by Smith *et al.*[14] the majority of values of log R'_{HK} in their compilation come from six sources[15–20], supplemented with a number of other studies that either preceded[13,21,22] or were roughly simultaneous with those sources. There is a significant overlap among stars in the Smith *et al.*[14] compilation and the catalogue of main-sequence stars for which Hünsch, Schmitt & Voges[23] derived soft-X-ray luminosities L_x from observations made by the *ROSAT* satellite. Using distances given in Hünsch *et al.*[23], which are based on *Hipparcos* parallaxes[24,25], together with the bolometric corrections of Flower[26], the values of L_x have been converted to a normalized ratio denoted $R_x = (L_x/L_{bol})$, where L_{bol} is the bolometric luminosity of each star. This ratio can be compared with values of R'_{HK} to define correlations between coronal and chromospheric activity.

The (M_V, $B - V$) colour–magnitude diagram of the stars in the combined (R'_{HK}, R_x) dataset is shown in Fig. 1, and is based on values of V magnitude, ($B - V$) colour, and distance tabulated by Hünsch *et al.*[23]. The solid line shows the zero-age main sequence (ZAMS) from VandenBerg & Poll[27], while filled circles denote stars having an absolute magnitude M_V within $0^{m}\cdot5$ of this ZAMS locus. There are 231 stars represented in Fig. 1, and it is for this sample that we search for any dependence upon either stellar colour or luminosity in the relationship between normalized X-ray and Ca II $H+K$ emission measures.

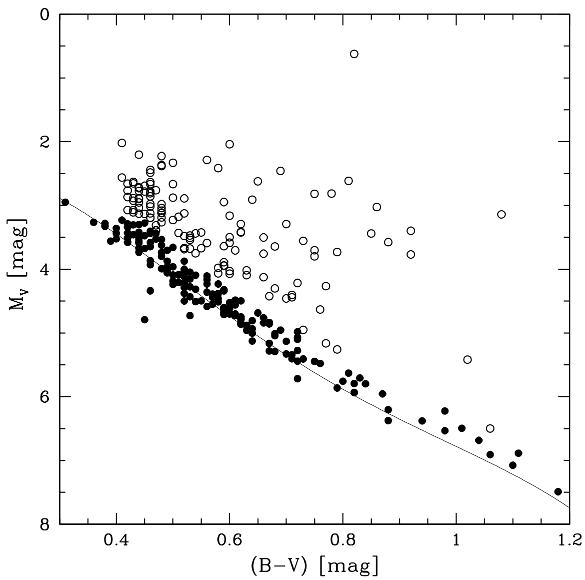

FIG. 1

An absolute visual magnitude *versus* ($B - V$) colour–magnitude diagram for stars with measurements of both Ca II $H+K$ and soft-X-ray emission. A solid line shows the zero-age main sequence of VandenBerg & Poll[27]. Open circles depict stars having an M_V more than $0^{m}\cdot5$ brighter than the ZAMS. Filled circles represent stars which either fall below the ZAMS or are less than $0^{m}\cdot5$ brighter than the ZAMS.

Sterzik & Schmitt[9] determined a correlation between R'_{HK} and R_x for a sample of 92 dwarfs without subdividing the sample according to stellar colour. Mamajek & Hillenbrand[1] used many of the same sources as those drawn upon here to document how Ca II *H+K* emission as well as soft-X-ray emission vary with stellar parameters including age, spectral type, and rotation period. In addition, they correlated the Ca II emission directly with soft-X-ray emission for those stars in their sample having measured rotation periods. Such an emission–emission correlation can also be documented using stars in the Smith *et al.*[14] compilation. Neither Sterzik & Schmitt[9] nor Mamajek & Hillenbrand[1] discussed whether the X-ray *versus* Ca II *H+K* relationship might vary with either stellar colour or advancing evolution away from the main sequence. It is these aspects that are focussed upon in the present paper. A distinction on the basis of stellar evolution is prompted by the finding of Wright[28] that chromospheric activity levels can be systematically reduced among stars as little as $1^{m}\cdot0$ above the ZAMS.

Results

The main results are presented in Figs. 2–6 as plots of $\log_{10} R_x$ *versus* $\log_{10} R'_{HK}$ for main-sequence stars in different $(B-V)$ colour ranges. Filled symbols in these five figures refer to stars having an absolute magnitude M_V that falls within $0^{m}\cdot5$ of the ZAMS plotted in Fig. 1. Stars that are more than $0^{m}\cdot5$ brighter than the ZAMS are shown as open circles. The symbols thus give some idea as to which might be the more evolved stars in each figure.

Correlations between R_x and R'_{HK} are evident among the stars in all colour ranges. The slopes of these correlations, however, appear to vary with spectral type. Linear least-squares fits of the form

$$\log_{10} R_x = A \log_{10} R'_{HK} + B \qquad (1)$$

were derived by using the SUPERMONGO package for the data in each of the colour ranges depicted in Figs. 2–6. Fits were made for both the full set of stars in each figure as well as the more restricted set within $0^{m}\cdot5$ of the ZAMS. The results for the constants A and B are listed in Table I for each colour interval.

TABLE I

Fits to equation (1) for dwarf stars of different colour

$(B-V)$	*fit ID*	A	B	σ_A	σ_B	N
			Full Sample in Each Colour Bin			
0·45–0·49	1f	1·78	2·93	0·33	1·52	54
0·50–0·59	2f	2·23	5·21	0·18	0·82	75
0·60–0·69	3f	2·12	4·88	0·25	1·16	52
0·70–0·79	4f	2·51	6·56	0·24	1·17	30
0·80–0·99	5f	2·11	4·91	0·32	1·48	20
			Restricted Samples Within $0^{m}\cdot5$ of ZAMS			
0·45–0·49	1r	2·31	5·46	0·47	2·13	25
0·50–0·59	2r	2·34	5·69	0·20	0·90	46
0·60–0·69	3r	2·74	7·65	0·17	0·80	31
0·70–0·79	4r	3·24	9·88	0·29	1·37	14
0·80–0·99	5r	2·06	4·59	0·45	2·10	13
0·50–0·79	6r	2·61	6·96	0·13	0·59	91

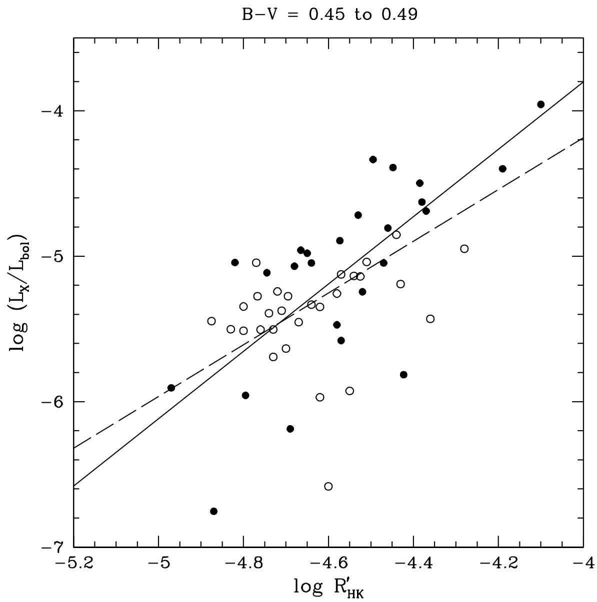

FIG. 2

Soft-X-ray *versus* Ca II *H+K* emission for dwarf stars having colours in the range $0^{m}{\cdot}45 \le (B-V) \le 0^{m}{\cdot}49$. Filled circles represent stars having an absolute visual magnitude less than $0^{m}{\cdot}5$ above the zero-age main sequence. The solid line is a least-squares fit to the data for these stars (fit 1r in Table I). Open circles correspond to stars more than $0^{m}{\cdot}5$ brighter than the ZAMS. The dashed line is a fit to all stars plotted in the figure (fit 1f).

Each fit is designated by an identifier listed in column 2, where *n*f refers to fit number *n* for the full set of stars within a given colour range, while *n*r refers to the fits for the more restricted and less-evolved subsets. The number of stars *N* contributing to each fit is also listed. Long-dashed lines and solid lines are shown in Figs. 2–6, corresponding to fits for the full and restricted samples respectively in each figure.

A distinction with respect to displacement from the ZAMS seems necessary for dwarfs of mid-F spectral type with $0^{m}{\cdot}45 \le (B-V) \le 0^{m}{\cdot}49$. Presumably the higher-luminosity stars in this colour range are of greater mass and are farther evolved from the main sequence. There is considerable scatter in Fig. 2, although a correlation is perhaps more apparent to the eye for the lesser-evolved dwarfs (which encompass a slightly greater range in log R'_{HK}). Fits 1f and 1r illustrate the effect of restricting the stars with respect to absolute magnitude; the more restricted sample evinces a steeper slope *A*, presumably owing to the more luminous dwarfs having a less well-defined relationship between X-ray and *H+K* emission.

If attention is confined to the more restricted samples in Figs. 2 and 3 there is little difference in the value of the slope *A*, *i.e.*, $A(1r) \approx A(2r)$. However, the restricted samples do reveal a trend for the value of *A* to increase towards later spectral type for dwarfs up to a colour of $(B-V) \sim 0^{m}{\cdot}80$, such that $A(2r) < A(3r) < A(4r)$. The data thus reveal that for stars close to the ZAMS there is a modest variation of the index *A* with spectral type.

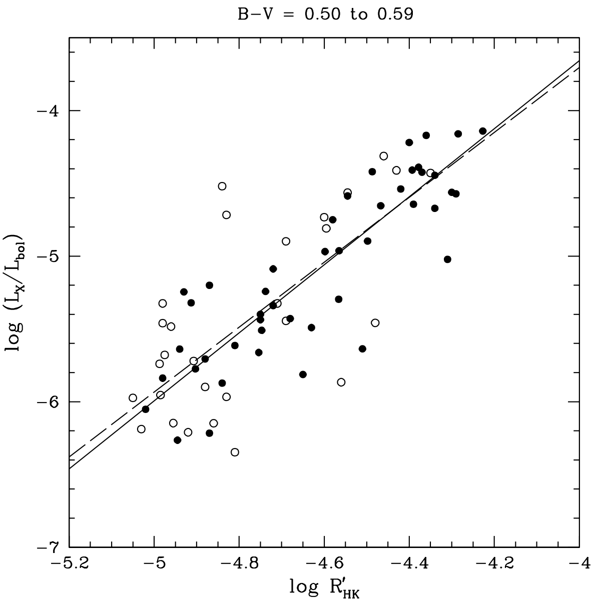

FIG. 3

Analogous to Fig. 2 except that dwarfs in the colour range $0^{m}{\cdot}50 \leq (B-V) \leq 0^{m}{\cdot}59$ are plotted. The solid and dashed lines show the least-squares fits denoted 2r and 2f in Table I. They are very similar.

The steepest relationship between coronal and chromospheric activity is fit 4r found among the restricted sample of dwarfs in the colour range $0^{m}{\cdot}70 \leq (B-V) \leq 0^{m}{\cdot}79$, for which $A = 3{\cdot}2 \pm 0{\cdot}3$. This relationship is shown as the solid line in Fig. 5. By contrast, the most evolved dwarfs in this colour range exhibit a greater dispersion in soft-X-ray emission at a given value of R'_{HK}. As a consequence, the fit for the full sample in this colour range is notably shallower. To provide a comparison with the behaviour for hotter dwarfs, the least-squares fit 2r found for the colour range $0^{m}{\cdot}50 \leq (B-V) \leq 0^{m}{\cdot}59$ is included in Fig. 5 as a dotted line. This latter fit is not an unreasonable match to the open circles in Fig. 5, but fails to match the solid circles depicting the dwarfs closest to the ZAMS. Thus, the inclusion of stars that have evolved away from the zero-age main sequence may tend to obscure any intrinsic dependence upon spectral type of the relationship between R_x and R'_{HK}.

The inclusion of stars more than $0^{m}{\cdot}5$ removed from the ZAMS generally has the effect of reducing the value of A for F and G dwarfs in the various colour bins of Table I. This is particularly true of dwarfs with $0^{m}{\cdot}60 \leq (B-V) \leq 0^{m}{\cdot}79$, but is not the case for fits 2r and 2f (which are plotted in Fig. 3 and are very similar). In both the colour ranges $0^{m}{\cdot}60 \leq (B-V) \leq 0^{m}{\cdot}69$ (Fig. 4) and $0^{m}{\cdot}70 \leq (B-V) \leq 0^{m}{\cdot}79$ (Fig. 5) there is greater scatter among the more evolved stars (open circles), which exhibit a more pronounced spread towards higher X-ray luminosities than given by correlations 3r and 4r for dwarfs closest to the ZAMS.

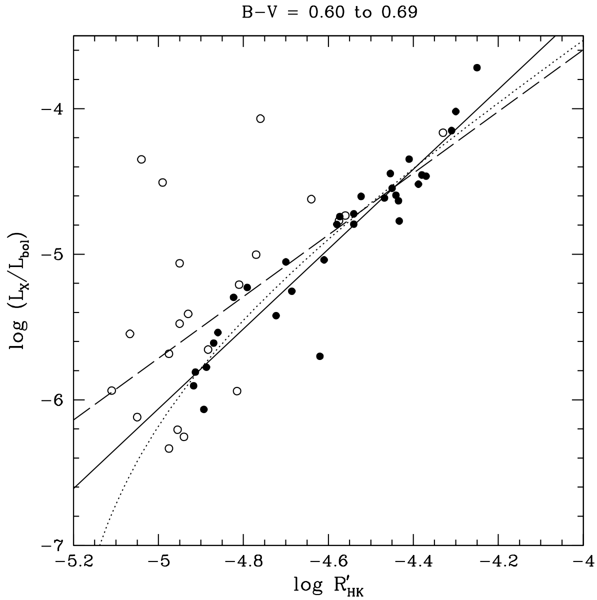

FIG. 4

Analogous to Fig. 2 except that dwarfs in the colour range $0^{m}{\cdot}60 \le (B-V) \le 0^{m}{\cdot}69$ are plotted. The solid line is a least-squares fit to the data for stars having M_V within $0^{m}{\cdot}5$ of the ZAMS (fit 3r in Table I). The dashed line is a fit to all stars plotted in the figure (fit 3f). A dotted line shows a locus of equation (3) that is required to coincide with fit 3r at log $R'_{HK} = -4{\cdot}4$ and $-4{\cdot}9$.

There are relatively few stars with $0^{m}{\cdot}80 \le (B-V) \le 0^{m}{\cdot}89$ (spectral types of K0–K2) in our compilation. While there is still evidence for a correlation between Ca II *H+K* and soft-X-ray emission for these stars, the least-squares fits shown in Fig. 6 are not as steep as for the restricted samples of G dwarfs.

In summary, restricting consideration to stars within $0^{m}{\cdot}50$ of the ZAMS there is evidence that the variation of R_x with R'_{HK} is steeper for G dwarfs than for F dwarfs or early-K dwarfs. Including stars that are more than $0^{m}{\cdot}5$ displaced from the ZAMS tends to add scatter to the R_x *versus* R'_{HK} relationship and to reduce the value found for the power-law index of this relationship.

To illustrate the comparison between dwarfs of different spectral type, stars less than $0^{m}{\cdot}5$ above the ZAMS from Figs. 3–5 were combined into a sample covering the colour range $0^{m}{\cdot}50 \le (B-V) \le 0^{m}{\cdot}79$. A plot of log R_x *versus* log R'_{HK} for these 91 stars is given in Fig. 7. The symbols have been coded according to stellar colour (open boxes for $0^{m}{\cdot}50 \le (B-V) \le 0^{m}{\cdot}59$ [spectral types of late-F to early-G]; filled circles for $0^{m}{\cdot}60 \le (B-V) \le 0^{m}{\cdot}69$ [spectral types of early-G to mid-G], and crosses for $0^{m}{\cdot}70 \le (B-V) \le 0^{m}{\cdot}79$ [spectral types of late-G]). A least-squares fit to all 91 stars is denoted 6r in Table I, is plotted as a solid line in Fig. 7, and has a slope of $A = 2{\cdot}61 \pm 0{\cdot}13$. The greatest scatter in Fig. 7 seems to be among those dwarfs hotter than the Sun, whereas dwarfs with spectral types of G2–G8 follow a tighter relationship and occupy much the same region of the diagram. As such, Fig. 7 suggests that the difference in the value of A found between F and G dwarfs is associated with a less well-defined relationship between X-ray and Ca II *H+K* emission for the F stars.

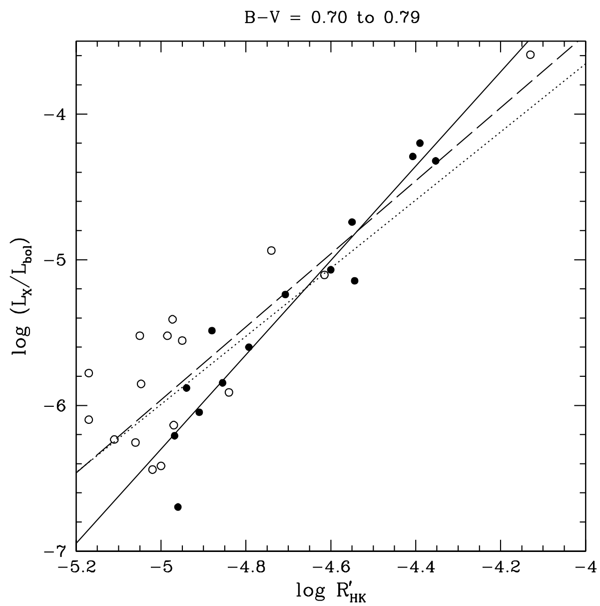

FIG. 5

Analogous to Fig. 2 except that dwarfs in the colour range $0^{m}\cdot70 \leq (B-V) \leq 0^{m}\cdot79$ are plotted. The solid and dashed lines show the least-squares fits denoted 4r and 4f in Table I. By contrast, a dotted line shows the least-squares fit 2r for dwarfs in the colour range $0^{m}\cdot50 \leq (B-V) \leq 0^{m}\cdot59$. The solid and dotted lines illustrate the degree to which the correlation between X-ray and Ca II *H+K* normalized emission may vary with spectral type among F and G dwarfs.

A summary of relationships between emission in a number of chromospheric and coronal lines is given by Schrijver & Zwaan[29]. They conclude that a single parameter "specifies the distribution of radiative losses over the temperature domains in outer atmospheres." They suggest that this activity parameter is in turn a function of stellar rotation, stellar structure, and binarity. Indeed, a number of correlations have been sought between both X-ray and Ca II *H+K* emission and either stellar rotation period or Rossby number[1,2,13]. The differences proposed in this paper between the R_x–R'_{HK} relationships of F, G, and early-K dwarfs may be associated with differences in rotation distribution, differences in the depth of the convection zone, differences in magnetic dynamo activity, and differences in the mechanisms *via* which these factors contribute to heating of the chromosphere and corona.

Comparisons with previous investigations

The fits in Table I are based upon assuming a power-law relationship of the form

$$R_x = 10^B \, (R'_{HK})^A. \qquad (2)$$

A similar fit was made by Sterzik & Schmitt[9] for 92 stars from the *ROSAT* All-Sky Survey. No distinction was made according to stellar colour. Their sample, which is very similar in size to that plotted in Fig. 7, gave a power-law

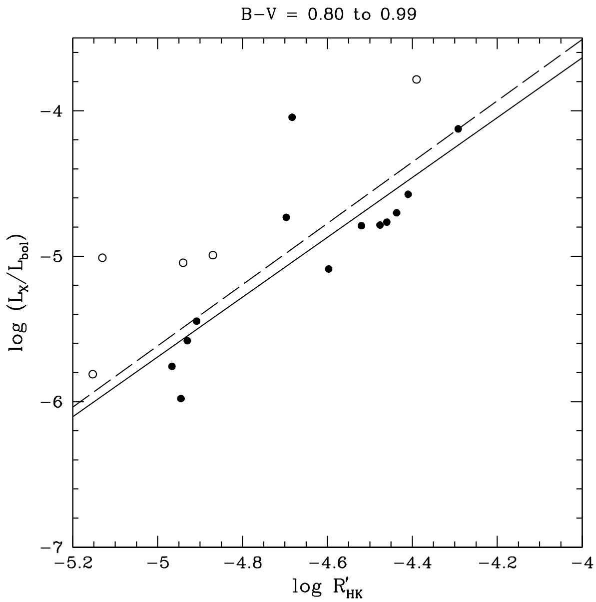

FIG. 6

Analogous to Fig. 2 except that dwarfs in the colour range $0^m{\cdot}80 \leq (B-V) \leq 0^m{\cdot}99$ are plotted. The solid and dashed lines show the least-squares fits denoted 5r and 5f in Table I.

index of $A = 2{\cdot}9 \pm 0{\cdot}4$, which is consistent with results found here for dwarfs of near-solar colour within $0^m{\cdot}5$ of the ZAMS. The fit from Sterzik & Schmitt[9] is included in Fig. 7 as a short-dashed line; it is slightly steeper than fit 6r although the slopes differ by less than 1σ.

A long-dashed line in Fig. 7 shows a fit found by Mamajek & Hillenbrand[1] based on an X-ray-unbiassed sample of 28 stars in the colour range $0^m{\cdot}5 < (B-V) < 0^m{\cdot}9$ for which rotation periods and long-term time-averaged R'_{HK} data were taken from Donahue, Saar & Baliunas[30] and Baliunas, Sokoloff & Soon[31], respectively, with X-ray luminosities again from the *ROSAT* All-Sky Survey. Their fit has a slope of $A = 3{\cdot}46 \pm 0{\cdot}18$ which seems significantly steeper than most fits given in Table I.

Other papers have taken a different approach to the parameters used to quantify chromospheric and coronal activity. Hempelmann *et al.*[7] considered flux densities F of soft-X-ray and Ca II H+K emission rather than normalized flux or luminosity ratios. They found a significantly flatter log F_x *versus* log F_{Ca} relationship for stars with $(B-V) > 0^m{\cdot}90$ than for hotter stars. Among stars with $(B-V) < 0^m{\cdot}8$ it is hard to discern any systematic colour dependence in their plots of log F_x *versus* log F_{Ca}. The fact that the values of A in Table I are lower for dwarfs with $(B-V) > 0^m{\cdot}80$ than for hotter dwarfs may be consistent with the Hempelmann *et al.*[7] finding, although the present work is based on normalized flux or luminosity ratios. Hempelmann *et al.*[7] proceeded to correct their F_{Ca} fluxes for a minimum flux F_{min} that varies with stellar colour according to the precepts of Rutten *et al.*[6]. They found that stars of all colour follow a

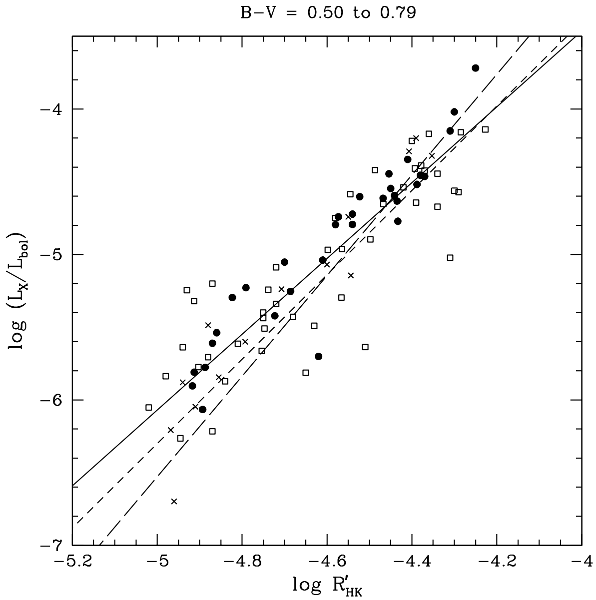

FIG. 7

Soft-X-ray *versus* Ca II H+K emission for dwarf stars within the broad colour range $0^{m}\cdot5 \le (B-V) \le 0^{m}\cdot79$ that have an absolute magnitude M_V within $0^{m}\cdot5$ of the ZAMS. The solid line shows the least-squares fit to these stars, which is denoted 6r in Table I. Linear fits from two other sources are shown: the short-dashed line is the fit to a sample of stars from Sterzik & Schmitt[9], the long-dashed line is for an X-ray-unbiassed sample from Mamajek & Hillenbrand[1].

uniform locus in a plot of log F_x *versus* log ΔF_{Ca}, where $\Delta F_{Ca} = F_{Ca} - F_{min}$. The power-law exponent in the $F_x \propto (\Delta F_{Ca})^{\kappa}$ relationship was found to be sensitive to the chosen relationship between F_{min} and stellar colour, with values of κ falling in the range 1·3 to 1·8. Schrijver, Dobson & Radick[32] had found $\kappa = 1{\cdot}5 \pm 0{\cdot}2$. Such relationships are not as steep as those summarized in Table I.

An analysis by Piters *et al.*[8] parallelled that of Hempelmann *et al.*[7] except for the use of normalized flux ratios. They obtained $R_x \propto (\Delta R_{HK})^{2\cdot14 \pm 0\cdot14}$, where ΔR_{HK} is a normalized Ca II H+K emission luminosity that has been corrected for a baseline term composed of one component due to photospheric emission and a second component due to acoustic heating of the chromosphere, according to the empirical analyses of Rutten[33,34] and Schrijver[5]. The base-level component again varies with stellar colour. The resulting parameter ΔR_{HK} is intended to reflect the heating of the chromosphere by phenomena related to a stellar magnetic dynamo.

The fit obtained by Piters *et al.*[8] corresponds to a function of the form

$$R_x = C\,(R'_{HK} - R'_0)^2, \qquad (3)$$

where C is a constant, and the zero-point term R'_0 is allowed to vary with stellar $(B-V)$ colour. The use of such a function has been explored for the lesser-evolved dwarfs plotted in Fig. 4. Values of C and R'_0 were determined arbitrarily by forcing equation (3) to intersect the fit 3r at both log $R'_{HK} = -4{\cdot}4$ and $-4{\cdot}9$, which leads to $C = 10^{4·52}$ and $R'_0 = 10^{-5·26}$. The resulting equation is shown as a dotted line in Fig. 4. Over the range of log R'_{HK} used to define the linear fit 3r, equation (3) appears to give an agreeable match to stars having an absolute magnitude within $0^{m}·5$ of the ZAMS. This example illustrates that it may be difficult to choose between equations (1) and (3) as to which better represents the data. The curvature in equation (3) is most apparent at low *HK* emission levels close to the value of R'_0. This indicates that dwarfs of the very lowest activity levels are needed to discern whether a function that incorporates a base-level *HK* flux is more appropriate. A caution has to be added in this regard. There are a number of dwarfs with $\log_{10} R'_{HK} < -5·0$ in Figs. 4 and 5; however, they exhibit a considerable scatter in the normalized soft-X-ray luminosity and are mostly more than $0^{m}·5$ removed from the ZAMS. Their low *HK* emission fluxes may be associated with evolution from the main sequence[28], and as such they may not be directly comparable to solar-like stars in states analogous to a Maunder Minimum. Once again this suggests that samples of dwarfs fairly close to the zero-age main sequence are most appropriate for determining fitting functions to relationships between coronal and chromospheric emission.

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CORRESPONDENCE

To the Editors of 'The Observatory'

Why stop at ⅔?

In the discussion of Professor Turner's talk, Rev. Barber states[1] that "the age of the Universe could be derived from any multiple of the Hubble constant from ⅔ onwards." (Presumably he means the Hubble time, not its inverse, the Hubble constant.) In the Einstein–de Sitter universe, with $\lambda = 0$ and $\Omega = 1$, the age is ⅔ of the Hubble time, which is presumably why Barber mentions this fraction. However, this is not a limiting value; except for the fact that there is a region of the λ–Ω parameter space in which the age of the Universe is infinite (*i.e.*, there is no Big Bang), the age of the Universe expressed in units of the Hubble time is a very well-behaved function of λ and Ω with no lower bound, neither at ⅔ nor at any other value (*e.g.*, Fig. 3 in ref. 2). (The value of 0 occurs for infinitely large (absolute) values of λ (which is negative in such cases) and/or Ω (if only one (absolute) value is infinitely large, the other is 0).) To be sure, an age of the Universe of less than ⅔ the Hubble time implies $\lambda < 0$, $\Omega > 1$ or both. Since the discussion is concerned with the possibility to "kick in an arbitrary Λ dark energy", it seems strange to constrain λ to be greater than 0 and Ω to be less than 1. Of course, cosmologists are now reasonably certain[3] that $\lambda \approx 0{\cdot}73$ and $\Omega \approx 0{\cdot}27$ (and these seem to be the result of a real convergence, not just the popular values *du jour*[4]), but in a general discussion of what could be, rather than what is, it is important to remember that there is no theoretical reason to exclude $\lambda < 0$ or $\Omega > 1$.

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The lost sunspot drawings of Humphry Marshall (1722–1801)

In recent years, several papers have been published on the recovery of old sunspot observations for a more precise knowledge of solar activity in the past[1]. The analysis of sunspot drawings made by J. C. Staudacher in the 18th Century[2,3], or the improved description of the onset of the Maunder Minimum[4], are two examples of this type of work. In any case, the largest collection of old sunspot observations was made by Hoyt & Schatten in their reconstruction of the sunspot number[5]. They stated clearly in that work that there were still many missing documents that might be useful for the reconstruction of solar activity.

I would like to draw attention in this letter to the lost solar observations of Humphry Marshall (1722–1801). He was an American botanist and plant dealer with a great interest in astronomy. Marshall wrote a letter to Benjamin Franklin dated 1773 May 3, containing a collection of sunspot drawings. Franklin presented those observations at the Royal Society and an extract of his letter appeared in the *Philosophical Transactions*[6]. More than two years of sunspot records (with about 300 miniature pencil drawings) are described in his long manuscript[7]. Two letters of Franklin to Marshall about those observations were published[8] but we have no more information about the exact location of the manuscript and the solar-disc drawings.

The location and study of those sunspot drawings would be of great interest because they complement the drawings made by Staudacher[2,3]. Any information about where, eventually, the drawings might be preserved would be welcome.

I appreciate the interest of Janet Bloom (Clements Library) and the support of Junta de Extremadura and Ministry of Economy and Competitiveness (AYA2011-25945).

Yours faithfully,
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REVIEWS

Using SI Units in Astronomy, by R. Dodd (Cambridge University Press), 2012. Pp. 230, 25 × 18 cm. Price £35/$55 (hardbound; ISBN 978 0 521 76917 4).

As an undergraduate, I learned about electromagnetism largely in the cgs formalism but with the SI system in the background. (Even earlier, I had learned about various Imperial units at school!) Before long, though, most university teaching moved over to SI, and we have been producing physics graduates who are most familiar with that system ever since. It is therefore a bit of a shock to some of them to find that astronomers have a curious attachment to the old ways (even before they have learned about the various magnitude scales). So I welcome the aim of this volume.

There is a chapter devoted to each of the SI base units, with a nicely-judged amount of background information and 'how-we-got-here'. The standards technology does not stay still, but the several possible changes to the formal definitions are most unlikely to result in any crises among the astronomical measurements, whose precision can seldom be said to be challenging that of the standards. There are plenty of worked examples for recalculating astronomical quantities into a common, systematic form (say, from erg cm^{-2} s^{-1} $Å^{-1}$ to Jy). There is a good range of astrophysical topics discussed in this context, though with the emphasis on examples rather than textbook treatment of the topics themselves. I must say that I have never found an astronomical need for the candela!

There are some more contentious suggestions: measure angles in radians and do away with sexagesimal notation; measure declinations from the south celestial pole (plenty of room for confusion there); recast magnitudes so that the scales go the 'correct' way, and maybe change the logarithmic base to 2, 10, or e; and various ways of representing extremely large or small numbers. Many generations of astronomers have had to get used to the magnitude system, although whether there is the same need for a logarithmic system now, I am not sure. Radio astronomers have managed for years with only a brief attempt to invent 'radio magnitudes' (they did not last!). Richard Dodd's suggestions about representing very large and very small numbers include one which looks rather like FORTRAN: 1·49 d 11 for $1{\cdot}49 \times 10^{11}$ works well in tabular material. There are, of course, the approved SI prefixes, mostly for powers of 1000 from 10^{-24} to 10^{+24}, supplemented by a wider range suggested by Mayes using words from languages around the world. The extreme prefixes I find very forgettable, but much less attractive still are the suggested extensions to million, billion, trillion ... and their reciprocals (anyone want to use 'undecillion'?). I find these unpalatable and think that they serve no useful purpose.

There are a couple more styles used in the book which might be improved. According to the booklet published in 1975 by the Royal Society on *Quantities, Units and Conventions*, the units attached to a measurement should be separated by (non-breaking) space or a centred dot, both representing multiplication: thus W m^{-2} Hz^{-1} or $W{\cdot}m^{-2}{\cdot}Hz^{-1}$ — whereas this book uses a standard point on the line: $W.m^{-2}.Hz^{-1}$. I don't remember the raised-point style being used in any publication (though this journal uses it as a decimal point, formerly the standard for UK printing). The form without the point seems less cumbersome all round. The other stylistic issue, also addressed in the Royal Society booklet, concerns the way to display units on graphs, tables, and the like. An axis of a graph showing flux density should be labelled (for example) S/Jy or $S/(10^{-26}$ W m^{-2} $Hz^{-1})$ — and then the labels are pure numbers. Even better for clarity, an expression for the logarithm of some quantity should be presented as, for example, log(S/Jy). I believe that you should only take logarithms of pure numbers, and that expressions like 'log(S) (Jy)' or 'Frequency ($\times 10^{14}$ Hz)' should not be used.

A useful book for the library, though perhaps I would not suggest that every student needs one on the desk. — GUY POOLEY.

Celebrating the AAO: Past, Present and Future, edited by R. Cannon & D. Malin (AAO, Epping), 2011. Pp. 353, 26 × 18 cm. Price AU$45 (about £29) plus overseas shipping AU$15 (about £10) (hardbound; ISBN 978 1 921916 04 5).

I was at a conference on site selection for optical astronomical observatories some years ago where there was a lot of discussion — naturally — on the characterization and measurement of seeing at different sites. The discussion went on beyond the patience of many of the audience. Roger Angel was the first to break. He stood up and said something along the following lines: "We are spending a lot of time discussing a very important property of astronomical observatories. But I wonder why we have not spent time on an even more important property. Everyone would agree that the seeing at La Silla and Kitt Peak is better than the seeing at Siding Spring and they all have 4-metre telescopes. But if you ask the same people which observatory — the Anglo-Australian Observatory, the European Southern Observatory, or Kitt Peak Observatory — has had the most impact on astronomy, the answer will not correlate well with the seeing. There is something else that makes an observatory effective, and we are not discussing it." Everyone went silent. It was a question that was embarrassing, too hard, or awkward (or all of these) because, after a few moments, the discussion returned to talk of the isoplanatic patch and microthermal fluctuations.

The present book is a contribution to a discussion of the occult properties. It is a series of over fifty reminiscences by people who participated in the foundation and early years of running the Anglo-Australian Observatory, the proceedings of a conference in the year in which, after 35 years, it was transformed into the Australian Astronomical Observatory, and irrevocably changed its character, if not its initials. Of course, the papers in the book contain their share of anecdotes, jokes, and informal pictures, but their lasting value is that they address serious issues about the practice of astronomy and science in general, perhaps more by implication than by full-frontal confrontation.

Some things are clear. The telescope was for a time the largest optical telescope in the southern hemisphere. It performed to a very high technical specification, optically precise, mechanically stiff, and the first large optical telescope that was computer controlled. Its instrumentation was built on the cusp between photographic detectors, which had reached a peak of development with the IIIa emulsion, and electronic detectors, of which it had two successful early examples, Joe Wampler's *IDS* and Alec Boksenberg's *IPCS*. The Observatory had a very productive symbiosis with the Australian community of radio astronomers, influential both as instrumentalists and physicists and in the radio data that they brought to the telescope to investigate optically. (The British radio astronomers were also influential in this way, but the Australians played the home advantage.) The AAO was also timely in that in its early years X-ray astronomy burgeoned and that in 1987 there occurred the nearest, brightest supernova for centuries.

The AAO was also an interesting sociological experiment in science, and the conditions that surrounded it must have had an influence on its productivity. Its genesis was in 1963 in an agreement between the Royal Society and the Australian Academy of Sciences to promote the need for the telescope. The scientists agreed on the scientific aim but warred on every issue that would bear on how it was accomplished, including the telescope design, its site, the institutions that should make it and operate it, and the way it would run.

The arguments thrashed through the issues implied in Angel's question. Somehow, the right answers were arrived at and eventually the AAO was set up as an autonomous Australian quango, with a governing board from both countries and its own staff, including scientists, five seconded from the UK. (The AAO is now subsumed as a division into the Australian Department of Innovation, Industry, Science and Research.) The operations staff would be located at the telescope site but the main administration, instrumentation, and scientific activities would be in Sydney. The telescope and the structure of the institute that ran it was implemented by 1974 and 'its aperture was declared open' by Prince Charles at the end of the year.

The AAO at least contributed to, some say caused, the resurgence of British astronomy in the last thirty years. The UK lost some of the staff that it transported to work at the AAO when they stayed on, but it also imported back from Australia skills and knowledge in those that returned. From lack of first-hand knowledge I cannot speak so strongly for the effect of the AAO on Australian astronomy, but at least I can see that it has never been stronger, and never more widely practised in Australian universities. It is clearly true that the AAO had the qualities identified from the conference floor as missing in that discussion from long ago.

This book contains contributions by many of the main players in these issues, although some key individuals are no longer with us, including Ben Gascoigne who died just before the conference and who is movingly remembered in the book. If read together with the complementary account, *The Creation of the Anglo-Australian Observatory*, published in 1990 by Gascoigne, together with K. M. Proust and M. O. Robins [see **111**, 258, 1991, for the review by Sir Bernard Lovell], it will tell you something of the answers to Angel's question about what really matters when you make an observatory, or indeed any scientific organization. — PAUL MURDIN.

Matter, Dark Matter, and Anti-Matter, by A. Mazure & V. Le Brun (Springer, Heidelberg), 2012. Pp. 195, 23·5 × 15 cm. Price £31·99/$34·95/€34·95 (paperback: ISBN 978 1 4419 8821 8).

The advent of high-precision cosmology based on the detailed analysis of the wrinkles in the cosmic microwave background is one of those epoch-changing events that rewrote our view of the Universe. So different was this new view that it has filtered down from the ivory towers to everyone. Who now doesn't know of the existence of Dark Energy and Dark Matter? The aim of this book is, with absolutely clarity and without mathematics, to explain the concepts behind this revolution and allow the layman to understand the phenomena and measurements that enabled this new view to arise. With a layout as clear as the prose and a lavish use of illustrations, colour photographs, and line diagrams, the book is mostly successful in its aim. However, at £32 a copy it is not cheap for quite a short paperback, and because of the caveats mentioned below it is not really a book for the absolute layperson either. It might make an excellent introductory astronomy text at A-level or first-year-undergraduate-level physics or indeed as a useful inspirational book for continuing-education courses.

In spite of the title, the book is primarily (7 of the 9 chapters) about matter and the business of establishing an inventory for the location (stars, gas, dust, *etc*.) in the Universe of regular everyday baryonic matter. The story of compiling this inventory is applied to illustrate and explain the various instruments and techniques used in astronomy and cosmology. Telescopes ranging across the electromagnetic spectrum, both ground-based and in space, theoretical modelling, stellar-population surveys, galaxy-rotation curves, spectroscopy, gravitational lensing, *etc.*, are all introduced and all clearly explained.

As a very small example of the clarity consider the statement that 25% of the mass created during Big Bang nucleosynthesis consists of atoms of helium. In a footnote the authors helpfully point out that in terms of numbers of atoms this actually means that only about 8% of the atoms would be helium. This 'extra mile' of explanation and detail is seldom encountered in more general books aimed at the layman. However, not all the footnotes or the glossary are so layperson-friendly; for example, the definition of the Lyman series includes the statement that "… this is the series of lines emitted by hydrogen atoms" — you need just a little bit of experience to know that atoms do not emit lines but photons, and what the authors mean is that these photons are displayed as lines of light in some spectrographs. It is probably true that the main text is very careful to avoid technical terms without explanation whereas the footnotes and the image captions make few such concessions. For example, although used in captions, neither proper motion nor magnitude are defined, and the caption to a diagram on the formation of structures from the gravitational collapse of a cloud just drops in "virial equilibrium" without explanation — but these are quibbles as in general the sense is clear even if the precise definition is lacking. I should also add that I really appreciated the reader-friendly appendices which, along with the powers of ten, the units of high-energy physics, and the thermal history of the Universe in quite a short table (about half a page), includes a comprehensive glossary. This is another thoughtful element that helps in understanding and the general clarity of this book — and allowed me to look up and remind myself of the definitions of hadrons, baryons, and bosons.

The Copernican revolution that began with the Earth losing its place at the centre of everything and progressed through the sequential demotion of the Sun, and our Galaxy as the principal body, continues with the demotion of matter itself. This ultimate journey of democratization, in which all matter is essentially equal and equally irrelevant, compared to the dominant universal constituents, is an unexpected outcome of modern cosmology. This book provides an excellent account of how the minor component of the Universe, baryonic matter, is distributed and how that knowledge has been obtained. — BARRY KENT.

Cosmic Update, by F. Adams, T. Buchert & L. Mersini-Houghton (Springer, Heidelberg), 2012. Pp. 138, 24 × 16 cm. Price £44·99/$69·95/€49·95 (hardbound; ISBN 978 1 4419 8293 3).

Editor Farzad Nekoogar (whose name appears at the end of the preface and half-hidden on the cover) has produced a remarkable book. Some of the remarks — thanks for a glossary with a simple definition of Anti-DeSitter Space (one with constant negative curvature) — are printable. Some require a bit of censorship — why on earth does the "concordance model" of cosmology (Fig 1.15) look like a half-burned flan cast aside in anger? Was it really the author, a native speaker of English, who gave us the figure caption "At this future time, the biosphere will have long time from being sterilized"? Are you really telling me that multiple members of a multiverse landscape can be in contact (Fig. 2.3)? And much else. In general, the figures are the most puzzling part, but equations and text also differ from what you find in most cosmology books; for instance, a demonstration that nature forbids the existence of a pure cosmological constant.

The demonstration is at least analogous to a thermodynamic argument, and the modified Friedmann equations are full of back-reaction terms.

Is this a book that I will lightly cast aside, or even throw with great vigour? Actually not, because it seems likely to be useful for preparing a somewhat odd talk that I seem to have agreed to give at a somewhat odd conference session later this year, but after that, watch out! Meanwhile, if any critic of the science establishment berates us for not allowing dissident theories a voice, please tell him about *Cosmic Update*! — VIRGINIA TRIMBLE.

Our Future Earth: The Next 100,000 Years of Life on the Planet, by C. Stager (Duckworth, London), 2011. Pp. 284, 24 × 16·5 cm. Price £18·99 (hardbound; ISBN 978 0 71564140 8).

A few years ago, I reviewed in these pages another book with an apparently similar title*. That book looked at all the possible problems that might prevent the human race surviving for at least as long as it has existed so far, and concluded that a stable world order was in principle possible in the long run but that the route to it would be extremely difficult. Bonnet and Woltjer are astronomers, with a lot of political experience in running large organizations, and placed quite a lot of emphasis on external threats (asteroid collisions and so on) as well as on political difficulties. The present book is refreshingly different (and makes no reference to Bonnet & Woltjer's book). The author, Curt Stager, is a palaeoecologist, from a small college in northern New York State, who is used to studying changes on long timescales. He mostly just assumes that human beings will still be around in 100 000 years' time and sets out to review the evidence from past climate variations and use it to paint a range of word pictures of what might happen in the future. Thus the background of the book is the global-warming debate, but extending the phenomenon in time. It has somewhat of an American bias (most noticeable in his use of the Fahrenheit scale, although the temperature in Celsius is usually given in brackets), and is aimed at a general readership as well as at scientists. He has an easy, informal style that is very readable and should appeal to both his target readerships (although occasionally, *e.g.*, when discussing sea-level, he assumes little background knowledge but uses a style more appropriate for the scientists in his readership).

Many people these days are concerned that human-induced changes are leading to the destruction of species at an unprecedented rate. Stager makes an interesting comparison with the much more dramatic species destruction that happened when photosynthesis began about 2 Gy ago, producing corrosive oxygen that wiped out most oxygen-hating bacteria in the process of allowing the evolution of oxygen-dependent species that ultimately led to us. He also points out that without the human-induced increase in 'greenhouse gases', and the associated slow warming of the planet, the Earth would almost certainly have been heading back into another ice age. The total destruction of the local environment when ice sheets cover it is, he feels, much worse than any consequences of global warming — although that warming is still something to be avoided in its most extreme form. He backs this feeling up by using records of past inter-glacials to sketch how plants and animals reacted and survived by

Surviving 1,000 Centuries: Can We Do It?*, by R.-M. Bonnet & L. Woltjer (Springer, Heidelberg), 2008; see review in **129, 157, 2009.

moving north or south as the climate changed, but notes (p. 66) that now "They can't move because we're standing in their way" — our cities, transport links, and cultivated areas have removed many of the corridors by which they might have migrated.

In the most extreme scenario of global warming, where the CO_2 levels get up to nearly 2000 ppm (*cf.* 387 ppm today; his 'moderate' scenario assumes it will reach 550 ppm), temperatures would reach levels not seen since the early Cænozoic, some 55 million years ago, before any ice sheets existed. He makes the uncomfortable point that, even if we managed to stop adding any more CO_2 to the atmosphere after the 2000 ppm level was reached, the 5000 Gtons already there would take some 400 000 years to be absorbed by natural processes. During his later discussion of fossil fuels, he explains how ^{14}C dating works, and makes the point that, because fossil fuels contain no radioactive ^{14}C, burning them actually slightly reduces the radioactivity in the atmosphere (and hence of course in our bodies) — a tiny positive effect from air pollution! However, this effect also distorts radiocarbon dates, making objects that died between the 1800s and mid-1900s date older than they actually are — dating of first-world-war skeletons would suggest that they died some 200 to 300 years earlier! On the other hand, atmospheric atomic bomb tests in the 1950s nearly doubled the ^{14}C concentration, distorting the dates in the opposite direction, by several centuries. We can correct for these effects — but pity any archaeologist in the far future, who isn't aware of these effects and finds that there are no artefacts dating from the 20th Century — there is an apparent gap in the records.

Another effect that is already occurring, although not so well-known to the public as the increase in atmospheric greenhouse gases, is the increasing acidification of the oceans because of the natural absorption of CO_2 by the sea. We can trace the potential future effects of this by looking back at the Palaeocene–Eocene Thermal Maximum (PETM) between 50 and 55 million years ago, during which there was a major acidification of the oceans. A few species survived, but many extinctions occurred — and it took millions of years for the oceans to recover. Temperatures and sea levels do eventually recover, but (p. 117) "Extinction, far more than global warming, is truly forever".

How is sea-level defined? He convincingly demonstrates that this is not as easy a question as you might think, before going on nonetheless to discuss how global warming might affect mean sea-level — but emphasises that rising sea-levels will not happen overnight, even if ultimately the total melting of the ice sheets in Greenland and West Antarctica might together raise the level by some 12 metres. It's not so much the temperature rise itself that will do the melting — even a few degrees will do it if maintained for long enough (50 000 to 100 000 years is likely on current scenarios). Every chapter of the book makes it clear that even if we stopped using fossil fuels tomorrow (which won't happen) it would be many centuries before CO_2 levels returned to pre-industrial levels, and even longer before the temperature dropped back to today's level — hence the title of the book (which is also echoed in the 2012 March 3 special edition of *New Scientist* — 'A Guide to Humanity's next 100,000 years' — so perhaps the message is beginning to get through).

Towards the end of the book, he makes the point again that global warming is postponing the next ice age — and uses that as an additional argument for saving carbon now, to be used in the far distant future, in a more planned way, to smooth out natural temperature fluctuations and also prevent further ice ages. He puts an emphasis on human responsibility and argues that scientists should remain objective, at a remove from politics and campaigning, in order to maintain trust in their predictions — and he follows his own advice in this book, which largely gives information rather than warnings. It's not easy to sum up his message, but perhaps the key points are that timescales for recovery are much longer than is generally realized and that as the Earth cools again in the distant future there will be other equally difficult adjustments to be made by us and by all species. But that is to over-simplify a fascinating and complex tale. I strongly recommend everyone to read this compelling and surprisingly optimistic book. — ROBERT CONNON SMITH.

Adventures in Cosmology, edited by D. Goodstein (World Scientific, London), 2011. Pp. 410, 23·5 × 15·5 cm. Price £57/$86 (hardbound; ISBN 978 981 4313 85 8).

Editor Goodstein has assembled a team of 14 experts (mostly American, mostly male), who have provided 12 chapters about the Universe. Chapter length and level are close to those of *Annual Review of Astronomy and Astrophysics*. Consistency (the Sunyaev–Zeldovich effect is explained; the Wouthuysen–Field process only about half way) and proof reading (my favourite is New Wright's cosmological calculator, which the chapter author says many readers will like; I was OK with the Old Wright) are also at the *ARA&A* level.

The book begins with Andrew Benson forming galaxies and ends with Xiaohui Fan forming supermassive black holes. In between come the microwave background, re-ionization (two chapters separated by clusters of galaxies), cosmic expansion, dark matter (three chapters — two cold and one hot), and cosmic acceleration and dark energy. Lots of pictures and graphs, many in colour, are nearly all well explained, with numbers on the axes, scale bars, and so forth. The number of equations per chapter ranges from zero (cosmic dawn) to many (particles as dark matter), and so too for the references. Only 'Mapping the cosmic dawn' is completely reference-free, a section called "An Unfinished Story" being followed by the completely blank p. 172.

My usual complaint concerns skimping of history, even where it is apparently mentioned. Cosmological neutrinos begin in 2001, with no mention of Cowsik & McClellan's limit on masses from the 1970s or the work of Zeldovich and his collaborators. Spirals rotate, but neither Rubin nor Rots is mentioned, let alone Babcock. Zwicky is credited only for supernovae, *etc*. Nor, of course, can any volume ever be quite current. The launch date for *JWST* has slipped from 2014 to 2017 between pages 130 and 402 (and is probably still on greasy skids), while cosmological supernovae have continued to advance.

Is this book likely to be worth more to you than £57? The answer, I think, is very probably yes, if you have the teaching (or taking) of an advanced course in cosmology in your fairly near future. All authors focus almost entirely on the current, standard, best-buy models, which I think makes sense for a future cosmologist's introduction to the subject. And there may soon be a new generation who do not need to be told that Leavitt's law used to be called the Cepheid period–luminosity relationship. — VIRGINIA TRIMBLE.

Build Your Own Time Machine: the Real Science of Time Travel, by B. Clegg (Duckworth, London), 2012. Pp. 290, 22 × 14 cm. Price £14·99 (paperback; ISBN 978 0 7156 4290 0).

This is a narrative description, virtually free of equations and diagrams, proceeding from the theories of relativity to current discussions about the "kill your grandfather" and other paradoxes involved in time travel. On the way the book does a little time travelling of its own, going back to the Eleatic philosophers of around 500 BC and the paradoxes of Zeno through to calendar reforms. But then it resumes its journey, with straightforward discussions about the arrow of time, the origins of time, the possible granularity of space and time, and suchlike. We are a third of the way into the book before there is a discussion of travel into the future through relativistic speeds or sitting at the bottom of a deep gravitational well. We then enter the realms of entanglement and quantum teleportation, and whether these phenomena could be used to travel back in time, and continue on to Wheeler/Feynman advanced waves and tachyons. We are about 80 per cent of the way through *Build Your Own Time Machine* before we encounter a concept which might, in principle, work: a wormhole. The technology of a wormhole — setting up a time differential between the ends, keeping it open through a massive dose of negative energy and the like — is discussed, and there is a description of a lab-scale machine envisaged by Ronald Mallett in which the frame-dragging effect produced by a tower of laser rings creates a closed time loop for particles fired along it. The closing chapters are mainly concerned with the numerous paradoxes which arise should time travel into the past be realizable (and there seem to be no arguments showing that it is impossible).

A comparison with the very similarly titled *How to Build a Time Machine* by Paul Davies (2002) is interesting. Unlike Paul Davies' book, Clegg's volume has lots of ancillary materials — potted biographies of Einstein, Gödel, Tesla, and so on, the Roman calendar and calendar reform, a discussion of the Einstein/Bohr debate, descriptions of Bell's ideas and Aspect's defining measurements, *etc*. In this sense it is a *tour de force*, but whether one appreciates this additional material depends perhaps on how much leisure one has to read it. The resulting book is more than double the length of Paul Davies'. But it remains a thought-provoking and easy read, taking the reader a step or two beyond Doctor Who's *Tardis* and the DeLorean in *Back to the Future*. — BILL NAPIER.

Stellar Physics, Volume 2: Stellar Evolution and Stability, 2nd Edition, by Gennady S. Bisnovatyi-Kogan (Springer, Heidelberg), 2010. Pp. 491, 23·5 × 15·5 cm. Price £81/$124/€89·95 (hardbound; ISBN 978 3 642 14733 3).

Stellar structure and evolution have now definitely burst the bounds of a single volume, or perhaps even two. This is the second edition of the second volume of a monograph from 1989–2000. The author indicates that the first volume, dealing with thermodynamics and radiative transfer, convection, nuclear reactions, and the basic structure equations and methods for their solution, has not aged sufficiently to require revision, while volume two has. It includes star formation and pre-main-sequence evolution, stellar lives from the main sequence to final stages, and separate chapters on dynamical, thermal, and pulsational stability and instability.

Considerable material on accretion discs, jet formation, and gamma-ray bursts has been added, and the 1088 references now extend from Abdurashitov *et al.* (on solar neutrinos) to Zytkov (on mass outflow from stars), including 96 self-citations. Evolution of binary stars is specifically excluded, from which one must conclude that the total field now requires at least three volumes. The only index is a rather brief one of topics, so that Cauchy is a problem, Chandrasekhar a limit, Coulomb a correction, and Cowling an approximation. There is a very good (12 page!) list of abbreviations, acronyms, symbols, and so forth, though with 22 assorted flavours of lower-case epsilon, stellar physics appears in urgent need of more alphabets.

A spot check reveals some items I disagree with. Surely it is time to stop describing Cyg X-1 as a mere black-hole candidate, unless you actually agree with the statement on p. 377, "Because of the more rapid increase of the centrifugal force during contraction in comparison with the Newtonian gravitational force, the collapse of a rotating star will always be stopped at finite density by centrifugal forces." The only citation is to one of the author's own papers. My reaction is that the book should probably be used in conjunction with some other text on stellar physics. Neither my own nor several other recent favourites are cited, but Kippenhahn & Weigert from 1990 is, and it remains not at all a bad place to start.

The author mentions correction of a number of misprints from the first edition. My favourite of those remaining is a figure caption (11.14) "Pasta nuclei denotes the state of the matter, where drops of a lower density are surrounded by a higher density nuclear see (516)" [intended surely to be "... nuclear sea, (See (516)"]. If these words were meant for another section of the present journal, we would be asking "Do bishops like spaghetti?" In any case, Bisnovatyi-Kogan has provided a solid presentation of the Russian view of stellar structure and evolution, which is still not sufficiently appreciated in the west. He was a student of Zeldovich and spells the name that way in the dedication, though the references say Zel'dovich, so you can find them on-line. — VIRGINIA TRIMBLE.

Exoplanets: Finding, Exploring, and Understanding Alien Worlds, by Chris Kitchin (Springer, Heidelberg), 2012. Pp. 297, 23·5 × 15·5 cm. Price £26·99/$29·95/€29·95 (paperback; ISBN 978 1 4614 0643 3).

When I was an undergraduate, Chris Kitchin's *Astrophysical Techniques* was required reading as it was wide-ranging, sufficiently detailed, and with few obvious errors. Through several revisions *Astrophysical Techniques* has remained serious reading for undergraduates. It is with this background, I must admit, I was looking forward to reading Kitchin's latest work *Exoplanets: Finding, Exploring, and Understanding Alien Worlds*. With hindsight this was maybe a little unreasonable and impossible for it to live up to. *Exoplanets* is a strange book: it's not a text book, and not really a light read. Instead it ranges from an historical work (major discoveries in exoplanet science) to what seem flights of (informed) fantasy (travelling to and living on exoplanets). The historical part is absolutely stuffed full of facts claimed by the author to be major developments. Researchers in the field will know that some "facts" are better established than others and in *Exoplanets* one or more are wrong and others remain controversial. Kitchin is not a researcher in this field and so perhaps he can be forgiven this.

The bulk of this book is given over to a review of the detection techniques employed by astronomers, with an estimate of their effectiveness. While this information is available in many other volumes, Kitchin writes clearly and makes the information accessible to non-experts.

So, at whom is this book aimed? Kitchin's style naturally lends itself more to a textbook than a coffee-table tome. But in *Exoplanets*, Kitchin banishes the mathematics and technical details to a series of appendices, enabling him to discuss his material rapidly. Nonetheless, the factual nature of the material makes this a read for the determined. Consequently, I think this book is ideal background for low-level undergraduate courses or motivated lay persons. — DON POLLACCO.

Chemical Evolution of Galaxies, by Francesca Matteucci (Springer, Heidelberg), 2012. Pp. 225, 23·5 × 15·5 cm. Price £81/$124/€89·95 (hardbound; ISBN 978 3 642 22490 4).

In order to evolve a galaxy, you must choose a mass of gas; decide on a prescription for turning it into stars (an initial mass function, perhaps some binaries, and some number of solar masses per year); let those stars die and put out (after a suitable delay time) some mix of elements (borrowed from your colleagues in stellar evolution); decide what fraction of the expelled gas and expelled nuclear products are to be blown out of the galaxy and how much gas (and its composition) might be allowed to flow in; and make a note of where your 'leaky box' has got to at various future times. Chances are, you will also want to let many of your choices vary with time and with position in the galaxy and to tie all these items to the mass of the dark-matter halo confining the gas and to the total mass present in stars (and their luminosities) at each time marker. You will then, with luck, be ready to interpret integrated spectra of stellar populations and, if you can get them, spectra of individual stars or sums of multiple stars of presumed similar types.

Matteucci's book explains, at varying levels, how to do all these things, ending with a discussion of cosmic chemical evolution in which she derives the current cosmic mean metallicity. It is 0·127 of the solar metallicity, including all the baryons, both in stars and in the intergalactic medium (whose mean metallicity should be 0·027 of solar). Because very little of the IGM has been observed, this definitely has the status of an honest, scientific, falsifiable prediction.

A very large fraction of the references are to papers wholly or partly by the author's Italian colleagues and to fairly recent work. The preface gets off to a slightly bad start by describing the work of Beatrice Tinsley (no early papers cited) as simultaneous with that of "R. J. Jr. Talbot and D. W. Arnett" (the 'Jr.' belongs after the surname, and he is W. D. Arnett). Tinsley's first paper was in 1968; Talbot & Arnett's in 1971, though Arnett had been evolving stars since 1967. Nor do I entirely agree with all the choices made on how, for instance, to treat populations of binary stars, but anyone wanting to get up to speed quickly on the topic should find the book very useful. It is mercifully short and has lots of nice figures comparing models and data for abundance gradients, correlations of various elements, and so forth. The table of solar and meteoritic abundances is from a 2009 review article, and if one wants to be surprised by something, it might be how little these numbers have changed over the past 50 years.

A special thank you goes to Evan Kirby of Caltech, who gave a fine colloquium talk on the fruits of medium-resolution spectroscopy at UC Irvine this February, thereby reminding me of the essential minimum content of galactic-chemical-evolution models. — VIRGINIA TRIMBLE.

Observational Astrophysics, 3rd Edition, by P. Lena, D. Rouan, F. Lebrun, F. Mignard, & D. Pelat (Springer Heidelberg), 2012 (French original 2008). Pp. 719, 23·5 × 15·5 cm. Price £117/$179/€129·95 (hardbound; ISBN 978 3 642 21814 9).

The greatest science-book reviewer of the 20th Century, Philip Morrison, once called a book "ample" because it contained everything he thought one needed to know about a subject. In this sense, the third edition of *Observational Astrophysics* is probably not ample, but it is impressively massive and contains a very large number of things that an astronomer might want to know, from millimetres of precipitable water above some of our favourite sites (less than 1 at the summer south pole to a shameful 7·1 at Kitt Peak) to the meaning of the Rao-Cramer inequality (which applies to the variance of an estimator). It would be interesting to have an estimator for the following sort of quantity: if I look deliberately at X pages out of a total of Y and find the first mistake on number N of the X (not necessarily examined in order), what does this imply for the total number of 'oopses' in a volume? Should we worry that p. 422 says that there will be primordial neutrinos, not emitted by the Sun, at 233 keV? What the authors meant is that there are neutrinos from $p + p \rightarrow d + e^{+} + \nu_{e}$ (in the Sun) at that energy and so detectable by a gallium experiment. The primordial neutrinos live down at temperatures a bit less than 2 K. It is extremely unlikely that anyone will try to build a primordial-neutrino detector on the basis of this glitch, but obviously one worries about less-easily-detected errors that might lead someone astray in the lab or at the telescope.

The authors explain that they have aimed to produce a reference book, not a text (though there are problems, some with answers, at the ends of the chapters). With this in mind, they have removed most bibliographic references from the text, but provided a detailed 'webography'. An interesting project for, say, 2022 would be to attempt to access the 222 sites and see how many still exist and provide the sort of information advertised, from NASA missions to text editors. The table of future missions mentioned in the text is, perhaps inevitably, a bit optimistic, from *JWST* in 2013 to *Maxim* and *TPF* in 2020. The major additions since the second edition include an expanded discussion of data analysis and a new chapter on sky surveys and virtual observatories.

And an item that left me saying, "how nice": the discussion of high-energy neutrino observatories puts the southern-hemisphere ones first! — VIRGINIA TRIMBLE.

A Field Guide to Deep Sky Objects, **2nd Edition**, By M. D. Inglis (Springer, Heidelberg), 2012. Pp. 278, 23·5 × 15·5 cm. Price £35·99/$39·95/€39·95 (paperback; ISBN 978 1 4614 1265 6).

The book starts off with a couple of chapters on the generic factors covering basic observing and how to use a telescope, but also covers some of the more important aspects of deep-sky observing, like the use of averted vision. The majority of the book is then broken up into chapters covering stars, nebulae of all forms (planetary, dark, diffuse), open and globular clusters, as well as galaxies. Each chapter is then subdivided by month to show the best objects to look for in that time period. Each object has a very brief description, apparently culled from other sources, or physical information. Some of the objects chosen and described also suggest that the author is not an observer himself, which perhaps makes it a bit more challenging with some of his suggestions. Although the book is aimed apparently at observers with small telescopes in the 20-cm class, many of the objects will be a challenge for much larger apertures. It is in this section that some of the oddities lie. I do have issues with the primary identification for many objects. If an object has an NGC number then that should be used as the primary identification. To use Caldwell, PK, or Herschel numbers as primary identifications I think really only confuses the issue. If these numbers must be listed they should come under the secondary identification. I did like the sections on dark nebulae as these are often overlooked in many other observing guides, and the brief guides to the object types included in each chapter are also good, particularly the one on galaxies. The book ends with chapters on obscure and naked-eye challenges. I must admit that in the star section I am not sure why there are sections on bright stars and the nearest stars. It looks like page-filling, but the section on red stars is good.

Overall this is a very mixed book and I think there are better books on the market, so I am not sure I could recommend it at the price it is. — OWEN BRAZELL.

Treasures of the Southern Sky, by R. Gendler, L. Lindberg Christensen & D. Malin (Springer, Heidelberg), 2012. Pp. 220, 30·5 × 25 cm. Price £40·99/$44·95/€44·95 (hardbound; ISBN 978 1 4614 0627 3).

This well-presented book is intended, in the words of its preface, "to celebrate the southern sky in words and with world-class astronomical images". It contains over a hundred large and scintillatingly spectacular colour photographs, with relevant descriptive texts, and opens with a succinct account of the historical development of our astronomical knowledge of southern-sky objects. This includes reference to David Gill's famous photograph of the 1882 comet, and the award of the Gold Medal of the Royal Astronomical Society in 1884 to Ainslie Common for his photograph of the Orion Nebula, pioneer events in demonstrating the value of the camera in astronomy and its ability to reveal objects beyond the direct perception of the human eye, and therefore of particular relevance to this book.

This is because all the photographs in its four main sections are of NGC or IC objects — nebulae, clusters, galaxies, *etc.* — which are only discernible *via* the camera, and stars are shown only to the extent that they are in front of the main subject of the picture, which is itself portrayed in a rich variety of colours. The four sections cover in sequence the items visible in the summer, autumn, winter, and spring skies, and between them disclose a vast range of fascinating phenomena; but the fact, as indicated above, that none of the objects portrayed is directly visible to the human eye (except in a few cases as a blur) prompts a couple of comments.

The first comment concerns the 'Southern stars and constellations'. As the authors point out in their introduction, the air in the southern hemisphere is clearer and light pollution is less pervasive than in the north. Add to that the fact that the Milky Way is better seen from the south, and in consequence anyone who has viewed the night sky from remote areas of the southern continents or from darkened ships in the southern oceans will recall a dazzling display of stars and constellations, so breathtakingly bright and sharp as to be almost audible. It is therefore something of a disappointment that a book which sets out to depict the treasures of the southern sky does not include a few illustrations of the stars and constellations as seen by the naked eye; and personally I should have found it helpful if some of the main pictures had been accompanied by small diagrams showing their location in the relevant constellation, though I realize that this would probably be an unnecessary adjunct for the professional or practising and dedicated amateur astronomer.

The second comment concerns colour. The book makes some mention of colour enhancement, digital remastering, and the like, but colour is such a dominant feature of the photographs that a description of current techniques for translating information picked up from the infrared or ultraviolet into appropriate parts of the visible spectrum, and making due allowance for dust, distance, and degradation would have been welcome. The colours give a visual impact to the illustrations which greatly enhances their interest and gives many of them a real aesthetic beauty, but this does give rise to the question whether reality in deep space is truly as kaleidoscopic as it is here portrayed.

These musings aside, the book is a pleasure to handle and to look at, and has obviously been produced by enthusiasts to share their own expert awareness, admiration, and appreciation of the treasures to be found in southern skies. Being essentially a picture book, 'look at' is a more appropriate term than 'read', but the historical preface and the texts accompanying the illustrations are clear and helpful, and anyone interested in a well-chosen and spectacular array of *New General Catalogue* and *Index Catalogue* objects will not only enjoy it but also come away with a greater awareness of the amazing events taking place beyond the Solar System. — COLIN COOKE.

3,000 Deep-Sky Objects, by Ted Aranda (Springer, Heidelberg), 2012. Pp. 575, 23·5 × 15·5 cm. Price £40·99/$44·95/€44·95 (paperback; ISBN 978 1 4419 9418 9).

Has there been a more comprehensive visual survey of deep-sky objects since the original Webb Society *Deep-Sky Observer's Handbooks*? Ted Aranda has compiled his own selection of over 3000 objects and then observed them all with his homemade binocular telescope. A tremendous personal achievement but has it produced a useful reference book? To cut to the chase — probably yes, but read on to see if the particular circumstances of its compilation will make it relevant to you.

To select his 3000 objects the author used the software MEGASTAR and, at least for the non-stellar objects, a magnitude limit of 13·2. Perhaps not the most scientific way of selecting visual objects but in view of the sheer number involved perhaps an understandable short-cut. All his observations were carried out over a 5-year period with the same telescope — actually 10-inch (250-mm) Cassegrain binoculars. Four sites were used in south-western USA with a practical southern declination limit of around 35° — the limit for the objects selected. These observation sites were 6000 feet or higher and free of light pollution. A consistent magnification of 170× was used for virtually all the observations and no filters were employed.

That brings me to the meat of the book — descriptions of the visual appearance of those 3000 objects. As might be expected these are necessarily brief and succinct. What is described is fairly conservative: no exaggerated claims as to what might be discerned after hours of study. However, given the sites used perhaps lengthy study was not needed! Objects are ordered simply by RA and not separated according to types. If you were expecting or hoping for sketches then you will be disappointed — it's text all the way though. A shame, as those sketches included in the Webb Society *Handbooks* were, for me, an essential component. However, producing 3000 sketches would have meant the book would probably never have been completed.

There is a comprehensive index and background appendices. One features a description of the author's binocular telescope and has the only (colour) photographs in the book. These show his binoculars in some detail and give some idea of what would be involved in making a similar instrument. Overall, the book could well be a useful addition to the dedicated visual observer's library, particularly given the huge number of objects described. Despite the rather atypical telescope and sites used, its descriptions are relevant and believable. — DAVID RATLEDGE.

Sketching the Moon, by R. Handy, D. Kelleghan, T. McCague, E. Rix & S. Russell (Springer, Heidelberg), 2012. Pp. 264, 25·5 × 17·5 cm. Price £35·99/$39·95/€39·95 (paperback; ISBN 978 1 4614 0940 3).

This book is a companion to an earlier volume in the same series, *Astronomical Sketching* (2007), and it arose in part from the on-line astronomy forum *Cloudy Nights*, where many astronomical artists (including several of the present authors) have long shared advice on the techniques of sketching celestial objects.

It is important to understand from the outset what the present volume sets out to do: as it says on the cover, it is about *sketching* the Moon, not *observing* it. The techniques discussed within its pages are primarily those that relate to draughtsmanship, rather than to the act of visual observation. It is essentially about how to depict, rather than how to see or interpret; and, apart from the briefest of introductions to each of the various types of feature to be drawn, the reader will find little that will help towards an understanding of the Moon and its surface. The emphasis is thus primarily aesthetic, rather than scientific, and those expecting a more thorough grounding in lunar observation might well be disappointed.

However, it is fair to say that the authors make their approach clear from the start, and they do an effective job in introducing the observer to the practicalities of rendering the Moon's complex and challenging surface features, not only on paper but also through the use of digital means. Following a brief introductory tutorial on pencil sketching, the authors offer a step-by-step guide to the techniques required to depict effectively the various categories of lunar surface feature: craters, maria, mountains, rilles, wrinkle ridges, faults, domes, rays, *etc*. Each section offers a choice of techniques and materials, from relatively simple graphite on white paper to the more demanding procedures of pen and ink and stippling. In this respect the book will be of interest to both the complete novice and the more advanced artist.

Despite the onset of high-resolution CCD imaging, visual observation and sketching of the Moon still has a place, perhaps most importantly in continuing our link to the great tradition of visual observation. This book does its best to keep alive, and add to, the techniques practised by the great lunar observers of the past. It is thus a welcome addition to the literature. — BILL LEATHERBARROW.

OBITUARY

David John Axon (1951–2012)

With the premature death of David Axon on 2012 April 5, the School of Mathematical and Physical Sciences at the University of Sussex has lost someone who in a mere two and a half years had made his mark as a strong administrator who also always made time to talk and listen to both students and staff, and the astronomical community has lost a distinguished and innovative astrophysicist who had many friends and colleagues worldwide. He will be much missed.

Born on 1951 June 18, David was both an undergraduate (BSc in Theoretical Physics 1972) and a postgraduate student at the University of Durham, obtaining his PhD in Astronomy in 1977. Following his PhD, David moved to the University of Sussex as a postdoctoral researcher (1976–1979), when the Royal Greenwich Observatory (RGO) was still based at Herstmonceux Castle and the University had obtained a grant for observational PDRAs. He was one of a group of distinguished astrophysicists who held research positions at Sussex in the late 1970s, including Martin Ward and the late Andrew Wilson, and collaborated with colleagues at the RGO, such as Keith Taylor. David and Keith developed David's PhD work on optical polarization in M 82, and were able to show conclusively that the galaxy showed outflow rather than the inflow that had been the conventional view for the previous 20 years. As part of this work, they were amongst the few astronomers to use the new technique of electronography before it was overtaken by the development of CCDs. They also used this technique on other galaxies and on various gas clouds in our own Galaxy, including a curious object in Orion: a ring of emission within which radial filaments emanate from the centre like the spokes of a wheel. I became involved in trying to explain this 'Cartwheel nebula' (see K. Taylor & D. J. Axon, *MNRAS*, **188**, 687, 1979; sadly, the original electronograph did not reproduce well in the published photographic rendering). However, we didn't come up with any very satisfactory explanation for the radial filaments — and when I asked him last year David said that he had never fully solved that particular mystery.

From Sussex, David moved to Cambridge with an SERC personal fellowship (1979–1981), and then to a research fellowship at UCL (1981–3). His first lectureship (1983) was at the University of Manchester, based at Jodrell Bank, where he maintained a position until 1999. During this period he spent five years (1993–1998) on leave as an ESA scientist at the Space Telescope Science Institute in Baltimore, where he had management as well as scientific responsibilities and introduced a streamlined process for the Phase II reviews of *HST* observing programmes which considerably improved the efficiency of *HST* scheduling. He subsequently became chair of the Department of Physical Sciences at the University of Hertfordshire (1999–2002), where he helped to supervise a dramatic expansion of astronomy. He followed this by moving to the USA as head of the Physics Department at Rochester Institute of Technology (RIT, 2002–2009) in New York State, where he also oversaw a considerable expansion, both in undergraduate physics numbers and in research activity. He maintained a research professorship there, and was on a visit to RIT, with his wife Lynne, when he unexpectedly collapsed and died, as a result of a heart attack.

He had returned to Sussex in 2009 September to head the new School of Mathematical and Physical Sciences, where he used his administrative experience to excellent effect and served on many university committees. He was not afraid of taking difficult and sometimes unpopular decisions that he saw to be in the best interests of the School, and by the time of his death most staff were persuaded that he had done (and was doing) an excellent job. He was a man with presence, who drove himself hard but always found time to stop and talk to people. He developed an excellent relationship with the students in the School, and attended both the Physics and the Mathematics Balls that were held at the end of the spring term, a couple of weeks before his death. Students spontaneously created a tribute page on Facebook for him and for another colleague, Wolfgang Lange, who had died two days earlier, and the tributes were very moving.

His main research was in the field of active galaxies and the evolution and structure of galactic nuclei, with polarization and multi-wavelength studies as recurring themes in his observations; he was involved in the development of the unification scheme for AGN, working extensively on both the narrow-line and broad-line regions. In addition to his early discovery, mentioned above, of the first galactic superwind, in the Cigar Galaxy (M 82), David's many scientific achievements include the discovery of the first X-ray-selected BL Lac object, the discovery of strong magnetic fields in the jets of young stellar objects, pioneering work on the magnetic-field structure of magnetic accreting white dwarfs, and conclusively demonstrating the presence of a supermassive black hole in the galaxy M 87. He had well over 200 refereed publications (30 in the last three years alone), the great majority in high-impact journals (*MNRAS*, *ApJ*, *A&A*, and *Nature*), and more than 100 conference papers and other publications, was co-designer (with Richard Bingham and Mike Scarrott, who both pre-deceased him) of the Durham Imaging Polarimeter, gained many nights of telescope time on world-class facilities, both ground-based and space-based, held numerous research grants, and served on many PPARC and other committees.

David's enthusiasm for astrophysics was boundless. Seb Oliver remembers as a young PhD student meeting him at the observatory in La Palma and being enthused by his expositions as one of the group of 'Lovers of Active Galaxies (LAGs)', which started with an International Time Programme in 1989–91. His enthusiasm had not dampened and recently he inspired a whole new research project following a local presentation by one of the Sussex research students. His passing has left a hole in the Department and the School that will be hard to fill. — ROBERT CONNON SMITH.

Here and There

THANKS FOR NOTHING

We also thank — for evaporating the mirror. — *PASP*, **121**, 383, 2009.

IT CERTAINLY IS

We report the discovery … of the peculiar Type IIn supernova ... in NGC 1260. With a peak visual magnitude of about –22, it is the most luminous supernova ever recorded. — *ApJ*, **666**, 1116, 2007.

AN INFLAMMATORY IDEA

Displays of the Northern Lights occur when solar particles enter the Earth's atmosphere and on impact emit burning gases that produce different coloured lights (oxygen produces green and yellow; nitrogen blue). — *Daily Telegraph*, *Travel Supplement*, 2012 February 11, p. 2.

ALLOWS THE HOT AIR TO ESCAPE

… our current telescopes lack the resolving power to see the accretion disc down to the vent horizon … — *Astronomy Now*, 2012 February, p. 9.

GET SET TO DUCK!

The [James Webb Space Telescope], which will orbit 1·5 kilometres above Earth, is set to launch in 2018. — *Victoria Times-Colonist*, 2012 March 7, p. B7.