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A REVIEW OF ASTRONOMY

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2007 October 12th at 16^{h} 00^{m}
in the Geological Society Lecture Theatre, Burlington House

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

The President. Welcome to today's meeting. First I have to announce the deaths of two distinguished members of the Society and dear friends to many of us; Professor Mike Seaton, FRS, died on 2007 May 29. He was President of the Society from 1979 to 1981, and was awarded the Society's Gold Medal in 1983. Professor Bernard Pagel, FRS, of the University of Sussex, died on 2007 July 14 and he was awarded the Society's Gold Medal in 1990. I'll ask you all to stand to remember those Fellows. Thank you.

Now, it's a great pleasure to congratulate the Society's various Fellows in the 2007 Birthday Honours list: Professor Lord Martin Rees of the IoA, Cambridge, who has been awarded the Order of Merit; Professor Jocelyn Bell-Burnell of the University of Oxford on being made a Dame of the British Empire; Heather Couper on the award of a CBE; Professor Mark Bailey of Armagh Observatory on the award of an MBE; and Professor Nigel Mason of the Open University on the award of an OBE. [Applause.]

It is also my pleasure to announce the winners of the 2006 RAS Blackwell Prize and the Michael Penston Astronomy Prize. First of all the RAS Blackwell Prize: first prize, of £1000, goes to Dr. Sophie Bassett of the University of Durham, currently based at JBA Consulting in a new coastal-risk-management team, for her thesis entitled 'Modelling sea-level observations to investigate the source and magnitude of major melt-water pulses during Termination 1'. The runner-up prize, a £50 book token, goes to Dr. James While of the University of Leeds, currently based at Environmental Systems Science Centre in Reading, for his thesis entitled 'Spectral methods in gravity radiometry'. The Michael Penston Astronomy Prize: first prize of £1000 goes to Dr. Kate Land of Oxford University for her thesis entitled 'Exploring anomalies in the cosmic microwave background', and she is currently nearing the end of the first year as a postdoc at the University of Oxford in the Astrophysics Department where she is a Glasstone Research Fellow. The runner-up prizes, of £50 book tokens, go to Dr. James Bolton of the University of Cambridge, currently based at the Max-Planck Institut für Astrophysik, Germany, for his thesis entitled 'Exploring the ionization state of the inter-galactic medium with Lyman-α absorption', and to Dr. Adam Moss of the Jodrell Bank Observatory, University of Manchester, currently based at the University of British Columbia in Canada, for his thesis entitled 'Solid dark energy and cosmic acceleration'. We very much hope the prize-winners will give talks on their theses at future monthly A&G meetings.

Fellows should be reminded that the deadline for the 2007 RAS Blackwell Prize and Michael Penston Astronomy Prize is 2008 January 31. Full details are available on our web page or can be obtained from the Executive Secretary in Burlington House. And talking of Burlington House, somewhat to our amazement, the refurbishment programme is nearing completion on schedule and on budget! The staff will resume working there from the beginning of November. The December Ordinary Meeting, which will feature a number of high-profile speakers including Martin Rees, will be the first to be held in our new lecture theatre, although the November meeting will be held here.

And before we move on with our programme, I am sure Fellows will want to join me in congratulating Al Gore and the Intergovernmental Panel on Climate Change on the announcement today that they have been awarded the Nobel Peace Prize. [Applause.]

So, now to our programme and our first speaker today, following very nicely after that announcement, is Professor Mike Lockwood of Southampton University and RAL; he is going to talk about 'Long-term variability of the Sun and recent climate change'.

Professor M. Lockwood. [The speaker explained that our climate system is driven by power received from our star, so changes in the output of the Sun will affect our climate. However, those outputs that affect our climate system, especially the optical and infrared electromagnetic radiations, are very hard to change on short timescales due to the large thermal time constant of the Sun. Those solar outputs that do vary rapidly, such as X-rays and extreme ultra-violet, do not have so much influence on our climate system, largely because the power involved is low. This results in poor understanding of the relation between solar and other climate forces, particularly volcanic aerosols and man-made emissions. One of the least-known feedback effects of the climate system is that of clouds — the models do not really agree — but they do have an effect on the Earth's albedo and also on the re-radiated out-going long-wavelength spectrum.

The solar wind also has a significant effect — the magnetic field causing geomagnetic activity — but it also punches a hole in the heliosphere and fills that hole with a weak magnetic field called the 'open solar flux'. It also deflects away from us Galactic cosmic rays. It has been postulated that the possible effect of Galactic cosmic rays on clouds has a direct input on our climate system, but this is a controversial subject. Cosmic rays do certainly produce cosmogenic isotopes which are stored in terrestrial reservoirs such as sediments, tree trunks, and ice sheets, and they are used frequently as indicators of solar variability in paleo-climate studies.

What do we know about long-term solar change? We have a handle on this through the interaction of the solar wind with the magnetosphere and hence geomagnetic activity. It is through missions like *Cluster* that we have learned to interpret geomagnetic activity. Using historic geomagnetic data from England and Australia we are able to quantify the open solar flux and a simple model based on sunspots has been derived; however, more complicated models have recently appeared, quantifying the production and loss of open solar flux. We now believe the open solar flux to be the primary modulator of the cosmic rays (and hence the cosmogenic isotopes), so that is very satisfying. Recent data from *Ulysses*, which maps the total open-solar-flux level, varies with sunspot number. Cosmic rays show an anti-correlation with the solar flux, which does not surprise us because the open solar flux is a primary shield against the Galactic cosmic rays. Using a composite of data from Davos Observatory in Switzerland, we find that low open solar flux and low sunpot number allows more Galactic cosmic rays in, so we have a minimum in the irradiance, whereas at sunspot maximum the converse is true. Where does the change in irradiance come from? Sunspots are dark and that lowers the irradiance but faculae stay hot and more than compensate for the sunspot effect, so the Sun is actually 0·1% brighter at sunspot maximum than minimum.

One postulate is that cosmic rays have a direct effect on climate, but this is controversial. The people who acquired those data do not believe they have been sufficiently well calibrated to use naïvely. The theory is that water vapour can condense around cosmic-ray nuclei, but it is difficult to see how this can be done rapidly enough since there is a shortage of condensation nuclei except in clean maritime air. However, some new data that do not have calibration problems were published by Giles Harrison at Reading last year. It involves measuring the solar intensity behind a Sun shield and then in direct sunlight; on a sunny day the ratio is close to 0 and on a cloudy day it is 1, so it is an excellent measure of cloudiness. It is well calibrated and the data go back about 50 years. The data were split according to whether the daily cosmic-ray flux was low or high and the fraction is slightly higher on high-cosmic-ray days; so there is an effect but it seems to be limited to clean maritime air and confirmed, in the opinion of the speaker, that there is an effect of cosmic rays on clouds. One problem in applying this, however, is that there is a very good anti-correlation between the irradiance and the clouds. An alternative hypothesis is that the relevant input into the cloud system is through the total solar radiance and that the cosmic rays are just a proxy indicator of what the irradiance is doing.

The speaker next looked at trends in recent data, by averaging out the solar-cycle variation with running means over timescales from 9 – 13 years in steps of 3 months. The results show that the sunspot numbers peaked in 1985, having risen through most of the last century. The open solar flux peaked about two years later, consistent with the models mentioned earlier, and since then it has been falling. The cosmic rays are almost a perfect mirror image of that, as expected, and the irradiance on average has also been falling since about 1985. The global-surface-temperature data has been rising despite the fact that the irradiance has been falling and the cosmic rays have been rising. Both should be global cooling effects if the Sun has been a major influence.

People have expressed reservations about the averaging procedures. When you look at the coupled ocean–atmosphere system, the time constant for change is much greater than 10 years so the solar cycle should be averaged out by the thermal capacity of the oceans. Certainly if you look at the temperatures at the surface of the troposphere, there is evidence that the time constant is much shorter than that, but then you have to allow for the fact that there is a heat input from the oceans.

Finally the speaker considered the effects of man-made activities. Fifty per cent of all the extra carbon dioxide that is in our atmosphere due to man's activities has come from deforestation, and still thirty per cent of our emissions today come from deforestation. Putting together all the effects from greenhouse gases the increase is linear in recent years. There have been two recent large volcanic events, in Mexico and Mount Pinatubo in the Philippines. The Pinatubo event led to large-scale cooling due to the amount of material thrown into the atmosphere. Volcanic eruptions also affect the optical depth of the atmosphere. If we look at the air surface temperature and tropospheric temperature then we have to allow for the oceans because the coupled ocean/atmosphere-system time-constants are very long, and there is a feedback effect which takes place in the South Pacific between South America and Australia in the form of the current known as El Niño. Heat comes up from below, as happened in 1998, followed rapidly by the reverse effect. The temperatures are still higher than we would expect even if they fell after the 1998 event.

So what is the total trend since 1987 — the point at which we think the Sun turned over and started going in the opposite direction? El Niños have actually brought less heat to the surface, which is a negative effect. We are recovering from Pinatubo, which is a positive effect, and the nett result is that we are left with a linear trend which is upwards while the solar effect is downwards.

The speaker concluded by offering the sobering statistics that during his talk six billion kilowatt hours of energy have been consumed, two million tons of carbon dioxide have been produced, more than 3500 cars and trucks have been made, and 6000 people have been born.]

Reverend G. Barber. Al Gore was criticised by the judge for suggesting that total meltdown of the ice caps would happen in years rather than millennia. Is that an exaggeration?

Professor Lockwood. I had a chat with John Mitchell, who is the chief scientist for the Met Office, and people who appeared at the trial, like Peter Stott. Their evidence was that at no point was *An Inconvenient Truth* actually wrong: there were no factual errors in it. They did say there were four or five points that were misleading, and that was one of them. What Gore said was that if the Greenland ice sheet melted then there'd be a certain rise in the sea-level. I've forgotten how many metres that was. What he failed to say is that in current scientific opinion that would take several thousand years to happen. The implication was that it's going to happen next week. They said this is misleading, but it isn't actually wrong — he never said it would happen next month, or next year, he just said the total amount, which was right and was scientifically correct.

There were other things, the most famous one being the graph of carbon dioxide and temperature going up and down throughout the Milanković cycles, which he showed, but failed to mention that in Milanković cycles it is Earth's orbital parameters that are affecting temperature, and that's making the oceans release carbon dioxide, so they're going together. Of course it's completely different now, at least that's what the climate scientists believe, and it's carbon dioxide driving the temperature and not the other way around. So that was considered misleading. At what point things get too difficult to explain in a film, I don't know. Personally I think the judgement is perverse; I think it's strange, because he's taken slightly misleading things and said the whole thing is misleading. John Mitchell, I know, said in evidence that he didn't think the individual facts that were misleading added up to a misleading case in total. They were just little things that cut corners, rather than anything more sinister than that, but the judge doesn't agree with me, or with John Mitchell I hasten to add.

The President. It's lucky that the Nobel Committee don't agree with him either.

Professor Lockwood. I know whose opinion I value more highly.

Professor N. O. Weiss. Apropos of that, isn't it the case that the Greenland ice is melting faster than predicted?

Professor Lockwood. It is, yes. People are worried about it. I think one of the reasons is that we didn't quite realize how ice sheets melt, and that they don't melt from the edges or the top, they melt from the bottom. So we're beginning to realize that it's melting faster than we feared. Frequent visitors to Svalbard will certainly find the place is unrecognizable twenty years on from when I first went there — it's just completely different. And that's completely consistent with the predictions, by the way, that the Arctic will melt much quicker because there's so much more landmass in the north; the high ocean content of the south means that the south will follow in twenty or thirty years time, and in fact the Antarctic ice sheet volume will stay just about constant; but that is what you predict, it's not an anomaly.

The President. Thank you very much for that wonderful talk. [Applause.] The next speaker is Giovanna Tinetti from University College London, and she's going to talk about 'Finding water vapour in the atmosphere of an extrasolar planet'.

Dr. Giovanna Tinetti. In the past decade, over 250 planets orbiting other stars (extrasolar planets) have been discovered. By inspection of the list of discovered objects and of their characteristics known so far, it is clear we need to be prepared for the unexpected. For a growing sample of giant extrasolar planets orbiting very close to their parent star, we can already probe their atmospheric constituents using transit techniques. A stellar occultation occurs when the light from a star is blocked by an intervening body, such as a planet, from reaching an observer. With this method, we can indirectly observe the thin atmospheric ring surrounding the optically thick disc of the planet — the limb — while the planet is transiting in front of its parent star. This method was traditionally used to probe the atmospheres of planets in our Solar System and most recently, thanks to the *Hubble Space Telescope* and *Spitzer*, has been successfully applied to a growing sample of giant exoplanets orbiting very close to their parent stars — the so-called 'hot Jupiters'. The idea was first proposed theoretically by Seager & Sasselov in 2000, and confirmed experimentally by Charbonneau *et al.* in 2002, when he and his team first detected the presence of sodium in the atmosphere of a hot Jupiter. This talk is focussed on the most recent discovery of water vapour in the atmosphere of an extrasolar planet using the *IRAC* camera on board *Spitzer*. Our team included twelve scientists coming from all over the world (Europe, US, Taiwan) with different expertise, from molecular spectroscopy to planetary/stellar modelling and infrared photometry. The selected target was a gas-giant planet orbiting very close to its parent star, located about 64 light years away from us. HD 189733b was chosen because of its very light, hot, and extended atmosphere but also because of its parent star's brightness and relative vicinity to our Solar System. All these characteristics made this hot Jupiter an ideal target to be observed with the primary-transit technique. The planet was observed while transiting in front of its parent star in three different infrared windows, at 3·6, 5·8, and 8 microns. According to our predictions, water was expected to be there in such a quantity as to be detected by this method. The observations confirmed that the star flux was dimming by a different amount in the three channels, meaning that the planetary radius was changing as a function of wavelength. More specifically, the planetary atmosphere appeared very opaque at 5·8 microns, most transparent at

3·6 microns, and something in between at 8 microns (the measurement at 8 microns was done by Knutson & Charbonneau's team at Harvard). Despite the fact that many molecules absorb in the infrared part of the spectrum, only water is able to produce such a strong signature in that wavelength range, so this was the first clear evidence that water vapour is present in the atmosphere of HD 189733b. Earlier attempts to search for water in a hot-Jupiter atmosphere were either unsuccessful or inconclusive because water was hunted in the visible part of the spectrum, where its signature is much weaker, or in the infrared with *Spitzer*, but using a complementary technique and a different instrument from the one we used. The presence of water vapour in the atmospheres of HD 189733b and HD 209458b (another hot Jupiter about ~ 2·5 times more distant) were most recently confirmed by different instruments, methods, and teams (Knutson *et al.*, Burrows *et al.*, Charbonneau *et al.*, Swain *et al.*). Some of these measurements show the presence of additional molecules apart from water. I cannot say more at present, other than "stay tuned!"

Thanks to *Spitzer* and *Hubble*, a new era for planetary science beyond the limits of our Solar System, has just begun. Unfortunately, neither of those two telescopes will last for long, but a new generation of space telescopes is expected to be launched in the next decade. The *James Webb Space Telescope*, even if not entirely dedicated to exoplanet observations, will be able to repeat the kind of observations we accomplished with *Spitzer* and *Hubble* with increased precision and accuracy. But what we are really longing for is the first generation of space- or ground-based telescopes for direct exoplanet detection, able to null out the contribution of the parent star and collect the photons reflected or emitted directly by the planet. The technology involved and costs required are far from negligible, but that will be the only way to make a leap forward in the understanding of the huge variety of extrasolar worlds, including the habitable ones.

The President. Obviously it's very exciting to detect water in hot Jupiters. What are the prospects of detecting water in terrestrial planets, and do these techniques help towards that sort of a goal? What are the prospects for detecting water in lower-mass extrasolar planets?

Dr. Tinetti. The improved sensitivity of the instruments on board *JWST* will allow us to probe the atmospheres of transiting Earth-sized planets, down to the habitable zone. So, possibly, water vapour in the atmosphere of terrestrial planets might be found with the same technique we used in the case of hot Jupiters or with the complementary one, the so-called secondary-transit method. I said "possibly", because according to preliminary calculations, these kinds of measurements will really challenge the limits of *JWST*'s capabilities. It will be easier, in any case, to discover water vapour on Earth-sized planets orbiting closer to the parent star because the higher the temperature, the more detectable are the atmospheric signatures, but also because in the habitable zone atmospheric water vapour partially condenses to form clouds and it is removed in this way from the upper part of the atmosphere. We really need direct detection to address properly the search for water — in liquid or gaseous form — on terrestrial planets, especially for those located in the habitable zone.

The President. Thank you very much. Monica?

Professor Monica Grady. How far away are we from being able to make any isotopic measurements of these atmospheres, so to look for deuterium, rather than just hydrogen?

Dr. Tinetti. I would bet that for an isotopic measurement you would really need a high spectral resolution.

Professor Grady. I'm not sure exactly; it depends on what particular feature you're going to be looking at.

Dr. Tinetti. I think that, in any case, the spectral resolution required would be much higher than present space-telescope capabilities. So, unless we are able to reproduce some of these observations from the ground, isotopic measurements are off limits for now. A lot of attempts have been made with ground-based telescopes to reproduce space observations of extrasolar-planet atmospheres, but for the moment nobody has been able to make a detection, only to set upper limits. In theory, it is not impossible, but it is certainly very hard, as you need to remove the contribution of the Earth's atmosphere, which is constantly interfering with your observations.

The President. Thank you very much. [Applause.] Our final talk this evening is by Professor John Womersley from STFC and he's going to talk about 'Developing STFC's science and technology strategy', and he's also going to say a bit about the outcome of the spending review this year.

Professor J. Womersley. The STFC is a new Research Council, founded on 2007 April 1. It is responsible for astronomy and space-science funding as well as particle physics, with an annual budget of 700 million pounds and addressing science from the fundamental, such as 'Is there life on Mars?', to much more applied and short-term-relevant science like cancer research. We hope to offer solutions to problems like climate and energy and this needs to be done in collaboration with industry and with other countries. The goals are not only to keep on with inherited research but also to try and do things better and in a more adventurous way. We want to do new things and influence those projects in which we are involved and we want to focus resources where there is excellence and not try to do a little bit of everything.

My job is to look about 15 years ahead because large telescopes, for instance, operate on that timescale, but there are short- and medium-term commitments for other projects which need upgrading and support; further, there is a portfolio of on-going programmes which need to be reviewed. Strategies need to be realized to allow one to commit resources to guide the STFC budget; the strategy must also be clearly communicated to the research community.

Many of the projects are international and we want to be able to influence these and take an active part. We need to be able to exploit people's skills, and the movement of people is one of the ways in which the discoveries of science have a large economic impact. Science and technology strategy cannot be separated, and we are developing the Daresbury and Harwell sites as innovation campuses. The structure is headed by the Science Board, which gives advice to Council and also to the Executive. It is supported by two science committees: PPAN is the Particle Physics, Astronomy, and Nuclear Physics Committee; and PALS is the Physical and Life Sciences Committee, concerned with light sources, lasers, and neutron sources. These in turn have grants panels reporting to them. Projects are considered by the Project Peer Review Panel and there is also an Accelerator Science and Technology Advisory Board. PPAN and PALS are also charged with organizing advisory panels, but these structures are not yet in place.

One of the main challenges is that we have to turn down support for perfectly good science. We believe that it is better to say "no" sooner and to whole projects, but this is difficult with the broad spectrum of STFC activity. It is necessary to balance research on topics like proteins with research on topics like protons, and this is very difficult. Government is not backing away from the idea that we ought to be more effective in getting the stuff out and into the market but they are claiming that basic research should not suffer. We need increasingly to consider the economic impact of any future research as well as the societal impact, the impact on education and training. Risk is not necessarily a bad thing provided it is fully considered and resources are put in place to deal with any consequences. We need to appreciate what the other Research Councils are doing and have a programme that is synergetic.

The 2005 RCUK Large Facilities Roadmap is now being updated; it is intended to give a broad view of research structures in the UK. It should go out for consultation in mid-October. STFC have put forward 21 projects for the road map, including upgrades for *Diamond*, the *ISIS* spallation neutron source, and for *Sapphire*. It is hoped to house a space centre on the Harwell Innovation Campus to deal with climate change, technology, and planetary exploration — perhaps dealing with future samples from a Mars-return mission. On the astronomical front, we have participation in the European *ELT* and the *SKA*, together with pathfinder projects, and a future gravitational-wave observatory. There will be a consultation period and comments will be solicited. I'm here to invite you to reply to the questionnaire on the web page.

There is a Large Facilities Capital Fund of £100 million per year over and above standard budgets, which the Research Councils can access with mutually agreed priorities, though financial flexibility is limited until about 2010. This fund is being used at moment to fund *ISIS* and *Diamond* construction amongst other things.

As far as the interface with technology is concerned, we need to develop new detectors, new materials, advanced computing facilities, and so on, which can also aid industrial research. A spend of £20 million per year in this research generates over £100 million per year in industrial activity.

In coming up with a science strategy for STFC, we must show that we can plan and prioritize our activities and that we can deliver on these. Unfortunately this means that we must stop things when they are good because other things may be better and we need to be as imaginative as we can and to do more with less resources; and increasingly we must show our relevance to society not only economically but also in terms of education and inspiration. In the end we do this because we want to solve problems and this requires people to be attracted to science, technology, and innovation and it requires the enthusiasm and dedication of young people. [Applause.]

Before I take questions, Michael did ask if could say something about next year's budget. The current spending round came out this week and there was a press release on the web, which is almost impenetrable. Some Research Councils did well and others did badly, but I'm told that they all came out roughly the same once you take out the different amounts of the full economic costs which had been given for university support and mandatory things which have been imposed on MRC for the agenda of working with the Health Service. It looks to me as though the medical people did quite well out of this. STFC received 8% more for next year, almost zero for the following year, and then a small increase in the third year of the spending period under review. So things will be tight as we are obliged to develop Harwell and Daresbury innovation campuses, and participate in Aurora, whilst at the same time providing support for our university colleagues.

The President. Thank you for that. We have time for one or two questions. If I might make a comment which may be in the minds of the Fellows here, I felt from your presentation that astronomy seems to be a rather small part of STFC, whilst within PPARC we felt we were a pretty large part, at least 50% of the action. Are we still a significant part of the thinking?

Professor Womersley. Well, I'm a particle physicist — there are words to frighten you — I'm a particle physicist but I'm here to help! I would answer that question by saying if you go into a fight you'd be more comfortable with a big guy going in in front, and to some extent our synchrotron and neutron facilities and their users represent a big bouncer going in front of you into that fight. It's interesting that many of our synchrotron or neutron users have the opposite view: that PPARC is running the show, that all the policies being applied are PPARC policies, all the committees are PPARC committees, and nobody understands the users of *their* kinds of facilities. So I do understand your sensitivity because from your point of view it looks as if there's a very big monolith of ex-CCLRC programmes that are going to eat up all of the budget. If you are a laboratory scientist you will have seen — and Mike can attest to this — exactly the same fears expressed in the other direction. I think it's a challenge for us to create something which is forward looking and not reflecting those concerns. The point that I was trying to make was that, within PPARC, this kind of discovery science of astronomy and particle physics was clearly *the mission*, but that meant that the decision of how much money went into that kind of science was hidden somewhere in OSI, as it then was. We have now got that decision into our own STFC processes with our Science Board and with people you know, and I think that's a step forward.

The President. So you don't receive the STFC budget with a directive about which section and which part of the mission it goes to?

Professor Womersley. No.

The President. That's very interesting. I'm looking through a forest of hands. Well I'm still going to ask another question! You'll need a bigger forest than that! A concern that I've had over the past 18 months talking to people from the Government, the Treasury, and so on, is that there is this strong agenda of knowledge transfer and the economic benefit of research. Of course the Treasury expects to see a very strong economic return on the money that it invests, which is entirely legitimate. But my concern has been to try and explain to people that a subject like astronomy clearly brings economic benefit not just in the long term but also sometimes in quite a short term. One can think of applications where the shift into industrial activity is really only a decade or something like that. So one is trying to persuade people that they mustn't set up a requirement that you have to demonstrate what the economic return is going to be for a new project when you apply for it. I don't know whether STFC sees this as an issue for blue-skies areas like astronomy and space science?

Professor Womersley. My answer is that we feel that pressure. We have to demonstrate that impact to some extent and it's certainly a lot easier having things like synchrotrons in the portfolio that can help answer that question. Space is also quite easy to explain, in contrast to what you might think of as more pure astronomy. These subjects certainly play a big rôle in attracting students, and one of the things we haven't seen, but I suspect will be coming now that we're in a government department with responsibility for universities and skills, is increased emphasis on physics education and attracting people into science. So it may be that while economic impact is the DTI agenda, we're not in DTI anymore, and I could imagine that education and skills are going to be stressed a little bit more, and I think that's something where astronomy, and for that matter particle physics, will have a good narrative to tell. The economic impact of any high-tech development in the sensors and focal-plane instruments and things like that are a good story that we can talk about. But we have to be aware that other areas have much bigger impact, such as our friends in the healthcare industry. It's not just what they're purchasing for their research, it's the consumer market. We'll never get into the consumer market with our things, except perhaps in terms of education.

Dr. Elizabeth Hoffman. Given that I'm more from the biological side, I wondered how much you're promoting the interaction between, for example, the biological and computing bio-informatics side. A lot of the logistical-support people I require actually have physics backgrounds. I've not noticed anyone here with an awareness of that. I wondered if you were trying to promote this perhaps to get some of the biological money into the physical sciences?

Professor Womersley. Well, strangely, John Zarnecki and I were musing on this on the way in after coffee, about ways to get biological money in — all good ideas gratefully received. Because I was on my way here, I was not at a cross-council meeting this afternoon to look at how to implement some of those programmes, bio and healthcare, and how to involve the strengths that other Research Councils can bring. It is typically better to have a bio-tech or life-science person coming and asking for help than for us to go out there saying we have the solution to your problem. So we need to create a dialogue rather than just march around saying we know. Particle physicists, especially, are stereotypically accused of going around saying we can solve everybody else's problems for them, and maybe they can, but only after working together to understand really what those problems are.

Professor I. Roxburgh. My question is, what is the decision process that leads to the choice of particular advisors and chairmen, and are you conscious that having done that you have already prejudiced the views that will emerge, and have you any internal mechanisms for controlling against that?

Professor Womersley. The mechanisms for choosing Council are statutory and they've been followed. The mechanisms for choosing the Science Board were interviews by the Chief Executive and Deputy Chief Executive after nominations were solicited publicly. We attempted to get a balance of different areas of expertise within the broad portfolio that STFC covers. We attempted geographical and gender balancing, and also age and experience balancing in so far as we could. You are free to be unhappy with our choice, but it was our choice; these people were appointed for one year with the intention that there should be a rollover with some of them being extended for more and other new people being brought on. Council provides one level of check, and the other level of check is provided by the fact that all of these committees are advisory committees. The executive greatly appreciates their advice; we try to give them as much ownership of the programme as we can so that they feel their advice is likely to be followed, but if they give us advice that is really not useful then we are not mandated to follow it.

Mr. H. Regnart. Thank you; what you said is very, very encouraging. Of course you don't need to be told, but considering how very little money has been put at your disposal maybe the grey politicians do need to be reminded of the famous encounter between Faraday and Queen Victoria, "What use is electricity?" — "What use is a baby Ma'am?" And whatever technologies — including most buildings — are made of, they're made with algebra, and if it could be taken away retrospectively the modern world would turn to dust. Fundamental research may be totally uneconomic in the short term, but in the long term the benefits are utterly inestimable.

The President. That's very true, but I think we should not carp at the overall allocation to science, which I think one has to say has been extremely good in the past decade and continues through the spending review to be more generous than other areas.

Professor Womersley. I had an interesting encounter with a couple of gentlemen from the Ukraine earlier this week, who have been trying to rebuild a viable science infrastructure in that country with a budget which is absolutely ridiculous. So it makes you realize that while we complain about not being able to do everything that we would like to do, other people are doing a great deal with far, far less than we do, and we do owe a responsibility to our taxpayers to give them good value for this relatively large amount of money that we've got.

Mr. R. Steppe. Does your remit allow you to pinpoint institutions which produce very good research and have that on the record, and allow them to have money given to them directly without having to have a piece of bread examined for how much peanut butter it has on it?

Professor Womersley. We have rolling-grants which support the group in a way that enables them to plan a strategic research programme with the expectation that that will continue, not just to do a single project; that's done through a peer-review panel. But the goal is to provide continued support for strong and excellent groups to allow that to happen.

Mr. M. Hepburn. Is there some possibility that your proposed unification of computer services will be able to exact a kind of audit rôle on the spectacularly incompetent computerization of things like the health service?

Professor Womersley. We would like to think that good project-management skills can be transferred from one level of public service to another. There was a National Audit Office study of large facilities which found that the Research Councils generally did pretty well, and it would be interesting to have other bits of government learn from that, but we have no particular money to do so.

Mr. M. Hepburn. The thing that worries me is what we now have is something like the National Audit Office, which doesn't really have the specialized skills needed to evaluate what's going on in these huge computer projects. Your council and the specialist institutions would have that expertise and it should be put to use. We've got into a frighteningly incompetent situation at central-government level.

Professor Womersley. It's interesting that one of the more attractive aspects of graduate students who trained in physics or astronomy going off to work, for example, in banks and financial sectors is their ability at programming, systems integration, and communicating and running projects, and I think that is an area in which we are already contributing. However, I suspect that some of those projects have problems to do with their specifications evolving or not being clearly defined, so that no matter how good the manager is you couldn't do better.

The President. Well I think we've exhausted the audience's desire for knowledge about STFC for the moment!

Professor Womersley. I feel extraordinarily guilty for standing up here and not showing anything that could be called science. I apologise for that and I do thank you for your time.

The President. I'm sure we want to hear from STFC again in the future, especially when the roadmap nears completion.

Following this meeting, there will be a drinks reception which is sponsored by Dr. Quentin Stanley to mark his engagement to Dr. Liz Hoffman, a former employee of the RAS. So today, for once only, drinks are free and on Quentin. I declare the meeting closed and the next monthly meeting will be on Friday November 9th.

THE VISUAL OBSERVABILITY OF THE CASSIOPEIA A SUPERNOVA

By John A. Morgan
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It is generally believed that the explosion which gave birth to the Cassiopeia A supernova remnant resulted from core collapse of a hydrogen-deficient star. A progenitor that has lost all of its hydrogen envelope and part of its helium envelope would lead to an explosion with the optical properties of a Type-Ic supernova. There is evidence, if not general agreement, that Flamsteed observed the Cas A supernova as a sixth-magnitude object in 1680 August. If an explosion with a typical SN-Ic light curve at the position and distance of Cas A attained maximum luminosity during the winter of 1679–1680, it would at that time have been poorly situated for visual observation, as its upper culmination would have taken place during daylight, while in August, between 170–200 days after peak luminosity, it would have been a sixth-magnitude star.

A persistent enigma regarding the origins of the Cassiopeia A supernova remnant is the dearth of contemporary accounts of a bright new star at the time of the outburst as estimated from the remnant expansion age. Kinematical studies[1] of the high-proper-motion clouds known as fast-moving knots point to an explosion in 1672 ± 18 years. Reed *et al.*[2] find a distance to the Cas A remnant of $3{\cdot}4^{+0{\cdot}3}_{-0{\cdot}1}$ kpc, obtained from radial velocities and proper motions of a large number of fast-moving knots. Their value agrees with that given earlier by Shklovski[3]. At this distance, assuming normal extinction of one magnitude per kpc, a Type-Ia supernova explosion should have had a peak visual magnitude of about -3, comparable to Venus. On the other hand, van den Bergh & Dodd[4] estimate that the Cas A supernova might not have exceeded $+2$ magnitudes at maximum light, and, noting the frequency with which both Oriental and European astronomers failed to record nova outbursts during this period, conclude that the absence of contemporary observations is unsurprising. The only 17th-Century record of a star near the location of Cas A is a single report by Flamsteed, who observed the sixth-magnitude star 3 Cassiopeiae on 1680 August 16 (Julian)[5]. This star has not been observed subsequently, and the validity of Flamsteed's observation has been contested[6]. Setting aside the question of whether or not Flamsteed did observe a star near the present-day location of the Cas A remnant, it seems worth trying to construct a plausible sequence of events that explains both the disputed observation of 3 Cassiopeiae by Flamsteed and the lack of other contemporary reports of a new star. This note examines the possibility that Cas A was a Type-Ic supernova (at least, as to light curve) that attained peak luminosity in the winter of 1679–1680, most probably in 1680 February or March. Flamsteed's observation of 3 Cassiopeiae, therefore, would have been made some six months after maximum light of the Cas A supernova. As will become apparent, the account to be presented does not tell a complete tale, and is at points necessarily conjectural.

It has long been thought that the Cas A supernova resulted from core collapse

*Institution shown for purposes of affiliation only.

of a hydrogen-deficient star. The discovery[7] of a compact X-ray source, interpreted as a neutron star[8], near the inferred expansion centre of the fast-moving knots and the observation of significant ^{44}Ti γ-emission[9] provide direct evidence for a core-collapse origin. Very little hydrogen emission has been observed in the fast-moving knots. If we posit a progenitor that has been stripped of its hydrogen envelope altogether, the resulting explosion would be an SN-Ib or SN-Ic supernova[10].

There are difficulties with assuming that the Cas A supernova was of Type Ic. To produce an SN-Ic light curve requires a progenitor that is (*a*) compact and (*b*) of low mass. No direct evidence bearing on the radius of the Cas A progenitor at the time of the explosion is available to us, but from the remnant one might hope to learn something concerning its mass. Estimates of the Cas A progenitor mass at the time of the explosion vary widely. One-dimensional hydrodynamic calculations[11,12] of the explosion of helium stars that have undergone significant mass loss reproduce the light curve of an SN-Ic outburst with a progenitor mass between $2 \cdot 3\ M_\odot$ and $3 \cdot 6\ M_\odot$. This range sits awkwardly with observational estimates[13,14] of the Cas A supernova ejecta mass of order $4\ M_\odot$. Measured abundance ratios of nucleosynthetic products[15] appear consistent with a mass of $\sim 12\ M_\odot$, while a recent three-dimensional numerical study of the evolution and explosion of Cas A finds that a WN Wolf-Rayet star of mass between $4\ M_\odot$ and $6\ M_\odot$ best fits the combined constraints posed by nucleosynthesis, ejecta mass, and compact remnant[16]. Explosion of a Wolf-Rayet star of appreciable mass would presumably result in an SN-Ib outburst. There is also evidence that the Cas A progenitor may have had a very thin layer of hydrogen[17]. However, the light curve of the peculiar Type-II SN1993J also fits the requirements of the scenario presented here.

Cas A lies near the Galactic plane at the low latitude of $-2 \cdot 1°$, and extinction appears to be non-uniform across the remnant. Hurford & Fesen[18] have determined from [S II] line ratios that the extinction to five fast-moving knots lies in the range $A_V = 4 \cdot 6 - 5 \cdot 4$ within errors estimated as $\pm(0 \cdot 25 - 0 \cdot 45)$. Using (less certain) Balmer Hα/Hβ ratios, they also find $A_V \leqslant 5 \cdot 3 \pm 0 \cdot 9$ and $\leqslant 6 \cdot 2 \pm 0 \cdot 9$ to two of the low-proper-motion quasi-stationary flocculi. These values are about one magnitude greater than the value $A_V = 4 \cdot 3$ previously found by Searle[19], and suggest that the extinction varies across the remnant by as much as a magnitude. The distance modulus for Cas A is $12 \cdot 7^{+0 \cdot 2}_{-0 \cdot 1}$. If we provisionally accept Flamsteed's observation of a sixth-magnitude star near the location of the present-day Cas A supernova remnant, and estimate the visual extinction to that remnant to be $A_V = 5 \pm 0 \cdot 45$, then the absolute magnitude of the supernova was approximately $M_V = -11 \cdot 7^{+0 \cdot 5}_{-0 \cdot 4}$ in 1680 August. This value is characteristic of late ($>$ 200-day) behaviour of a supernova light curve, rather than an early peak value of approximately $M_V = -16$ to -18, suggesting that the initial outburst could have taken place as much as 200 days earlier than 1680 August 16.

At any time between, roughly, New Year and early June of 1680, the (circumpolar) Cas A supernova would have had its upper culmination in the daytime sky in the northern hemisphere. First observation of the supernova would almost certainly have been with the naked eye, despite the general use of the telescope[20] by late 17th-Century astronomers. A Type-Ic supernova at the distance of Cas A would have been unobservable by naked eye in daytime even at peak brightness. The threshold visual magnitude for observation of a star by a human observer in the presence of Rayleigh-scattered sunlight may be estimated as described by Hughes[21]. The minimum detectable brightness contrast in lux detectable by human observers[22,23] is converted to stellar magnitude with Russell's[24,25] value for the stellar equivalent 1 lux $= -14 \cdot 18$ visual magnitudes. The atmospheric radiative-transfer code MODTRAN4[26,27] was used to calculate the brightness of the sunlit sky in clear-air conditions at the 1680 February 14 position of the expansion centre of Cas A as viewed from Greenwich for Julian dates 1680 February 2, February 12, February 22/23, and March 3, using solar positions from Gingerich & Welther[28]. Threshold m_V magnitudes corresponding to a just-detectable star at those dates for Cas A altitudes near upper culmination appear in Table I. The m_V values in the table are probably in error by no more than ±0·1 magnitudes. Threshold m_V values are higher for very low solar altitudes, samples of which also appear in the table, but the plane-parallel MODTRAN model is probably not to be relied upon for solar altitudes as low as 5°.

TABLE I

Threshold V magnitudes at location of Cas A

Date	*Cas A altitude*	*Solar altitude*	*Threshold m_V*
1680 Feb 2	83°·66	22°·62	−2·4
"	65°·22	5°·75	−1·0
1680 Feb 12	83°·40	28°·64	−3·1
"	56°·65	5°·04	−0·8
1680 Feb 22	51°·78	8°·07	−1·3
1680 Feb 23	83°·76	33°·14	−2·8
1680 Mar 3	82°·57	36°·66	−3·0
"	42°·71	5°·51	−1·2

For purposes of discussion, consider SN1994I as a template for the Cas A outburst. Estimated apparent V magnitudes for SN1994I at the distance of Cas A for maximum light and various times thereafter appear in Table II[29]. If the values shown for A_V are assumed uncertain by ±0·45 magnitudes, the apparent V magnitudes of Cas A in the table should have a nett uncertainty (−0·4, +0·5) from combined errors in extinction and distance modulus. The uncertainty arising solely from error in the distance modulus is (−0·1, +0·2) magnitudes. Table II also contains apparent V magnitudes for SN1993J at the distance of Cas A[30]. The SN1993J V light curve is quite similar to SN1994I. While the peak M_V of SN1993J is dimmer by about half a magnitude, its exponential tail is somewhat brighter at the same time after maximum light, and at the distance of Cas A it attains the same visual magnitude as SN1994I as much as 60 days later. Without further adjustment of the overall brightness of the light curve, it is just possible to fit it into the scenario for Cas A.

TABLE II

Apparent V magnitudes of SN1994I and SN1993J at distance of Cas A

Template	A_V	*Peak m_V*	*m_V(170d)*	*m_V(185d)*	*m_V(200d)*	*m_V(220d)*	*m_V(235d)*	*m_V(250d)*
SN1994I	5·0	−0·43	5·0	5·3	6·0			
"	5·3	−0·13	5·3	5·6	6·3			
"	6·0	0·57	5·5	5·8	6·5			
SN1993J	5·0	0·04			4·6	5·0	5·2	5·5
"	5·3	0·34			4·9	5·3	5·5	5·8
"	6·0	1·04			5·5	6·0	6·2	6·5

Richmond *et al.*[29] find a peak visual magnitude $M_V = -18{\cdot}09 \pm 0{\cdot}58$ for SN1994I. Taking $M_V = -18{\cdot}09$ for the peak magnitude and $A_V = 5$, the peak apparent visual magnitude of Cas A would have been $m_V(\text{peak}) = -0{\cdot}43^{+0{\cdot}2}_{-0{\cdot}1}$.

In early 1680 February, the threshold visual magnitude which would have been observable by naked eye against the background of Rayleigh-scattered sunlight would have been brighter[31] than m_V(threshold) $= -1$ for solar altitudes above 5°. If the Cas A explosion occurred between the New Year and June, Cassiopeia would have been unobservable by naked eye anytime near upper culmination.

The foregoing estimates show that, on this account, the position of the Cas A supernova in winter 1680 would have been such that sunlight would have precluded naked-eye discovery anywhere near the meridian. This may go some way toward accounting for the failure of contemporary astronomers to note any such phenomenon, but leaves unexplained why the circumpolar supernova was not observed at night. Cas A would have been visible at peak brightness (whenever that occurred) during some portion of night-time at an appreciable altitude above the horizon. On February 2, it should have exceeded the visible threshold shortly after sunset at an altitude of approximately 57°. At this altitude, atmospheric extinction would have been about 0·1 V magnitudes, so that Cas A could have appeared as bright as $m_V = -0{\cdot}3^{+0\cdot2}_{-0\cdot1}$ ($A_V = 5$).

It is necessary, then, to stipulate some reason for the absence of reported observations of the supernova during the winter months, apart from its unobservability during daylight. Here the story, unavoidably, becomes speculative. Apart from known factors such as the small number of astronomers active in the 17th Century and the lack of any network for collection, dissemination, or archival of reports by non-astronomers, one might adduce the discouraging effect on observation of winter conditions in the northern hemisphere, including the possibility of extended periods of cloudy weather. The supernova could have been dimmer at its peak, or its extinction greater, and it could have faded somewhat more slowly, than assumed to this point: on February 2, an outburst with peak $M_V \geqslant -17$ or extinction $A_V \geqslant 7$ would have appeared to an observer with m_V(peak) $\geqslant 1{\cdot}7^{+0\cdot2}_{-0\cdot1}$, in the range which van den Bergh & Dodd suggest might have escaped notice by contemporary observers[4].

Return to the observation of 3 Cassiopeiae on the night of 1680 August 16[5]. Flamsteed reported stellar magnitudes as integer rank values, so that one may suppose that 3 Cassiopeiae could have been as bright as $m_V = 5{\cdot}5$. The SN1994I light curve could have faded to $m_V = 5{\cdot}5^{+0\cdot2}_{-0\cdot1}$ by day 170 after maximum ($A_V = 5$), while SN1993J could have done so by day 200 ($A_V = 6$). Note that the rapid decay of SNe-I is necessary in order to reach sixth magnitude within the requisite period of 170–200 days after peak brightness; no normal SN-II light curve is known to decay that rapidly.

Although the particular scenario presented in this note requires the Cas A supernova to have been observable at the time and location of the disputed report by Flamsteed, the explosion could just as well have occurred mid to late winter of some other year in the late 17th Century, resulting in an unprepossessing object of sixth magnitude at the upper culmination of Cassiopeia in August of that year.

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CCD PHOTOMETRY OF TWO NEGLECTED CEPHEIDS IN CARINA

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A sequence of CCD frames in the BVI_c system was obtained for two Cepheids lacking published finding charts, ET Car and EW Car, using the 40-cm telescope of the Observatorio Cerro Armazones (Universidad Católica del Norte, Chile). Identification charts and tables of observations are presented along with new light curves. Both stars appear to be Type II variables, class CWB, rather than classical Cepheids as suggested previously.

Introduction

The variable stars ET Car and EW Car were discovered on photographic plates by Hertzsprung[1], who classified them as Cepheids and presented coordinates and mean light curves for them, along with corresponding light elements. No further observations or finding charts for the Cepheids have been published, although van Houten[2] later improved upon their light elements with the following ephemerides:

$$\text{ET Car: JD}_{\max} = 2426883{\cdot}217 + 2{\cdot}910837\,E,$$
$$\text{EW Car: JD}_{\max} = 2424560{\cdot}830 + 4{\cdot}238730\,E.$$

Both stars were included in our programme of photoelectric observation of southern-hemisphere Cepheids, but the available coordinates lacked sufficient precision to identify them unambiguously. Presented here are the results of a programme of observation to obtain a series of CCD images in different filters around ET Car and EW Car for the purpose of re-identifying the two Cepheids and constructing more precise light curves for them.

Observational data

All observations for this study were made at the Observatorio Cerro Armazones (OCA) of the Universidad Católica del Norte (UCN), Chile, in 2005 April and May. The data were obtained using the Observatory's 16-inch Schmidt-Cassegrain telescope in combination with an ST-9 CCD camera that was used with Cape-system BVI_c filters[3]. Differential photometry was performed on the programme objects relative to six comparison stars for the field of ET Car, and three for the field of EW Car. Magnitudes and colours for the comparison stars were determined photoelectrically by LNB from observations obtained with the 30-inch telescope of the South African Astronomical Observatory (SAAO).

Our observations for the comparison stars were also used to determine the coefficients ξ_V, φ_V, μ_V, ξ_B, φ_B, μ_B, ξ_I, φ_I, and μ_I, for transformation of the instrumental quantities v, b, and i to the standard Kron-Cousins system from the standard formulae:

$$\begin{aligned} V &= v + \xi_V\,(B-V) + \varphi_V\,(B-V)^2 + \mu_V \\ B &= b + \xi_B\,(B-V) + \varphi_B\,(B-V)^2 + \mu_B \\ I_c &= i + \xi_I\,(V-I_c) + \varphi_I\,(V-I_c)^2 + \mu_I \end{aligned} \qquad (1)$$

Mean transformation coefficients of $\xi_V = -0{\cdot}054 \pm 0{\cdot}019$, $\varphi_V = -0{\cdot}003 \pm 0{\cdot}012$, $\xi_B = 0{\cdot}081 \pm 0{\cdot}008$, $\varphi_B = -0{\cdot}023 \pm 0{\cdot}005$, $\xi_I = 0{\cdot}031 \pm 0{\cdot}009$, and $\varphi_I = 0{\cdot}003 \pm 0{\cdot}004$ were established from observations made on the best nights. The relations defined by equations (1) were also used to define zero-point offsets μ_V, μ_B, and μ_I in V, B, and I_c, respectively, from measurements of the standards made on each frame.

Results

When all CCD frames were reduced, the observations for all stars around the published positions for the Cepheids were convolved with the elements from van Houten[2], and the Cepheids themselves were found quite easily. They are marked on the finding charts presented in Fig. 1, and their coordinates are given in Table I.

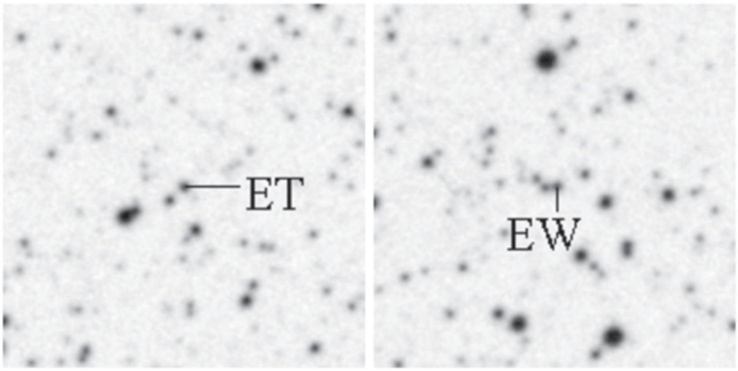

FIG. 1

Finder charts for ET Car (left) and EW Car (right). Each field measures 3′ × 3′ and north is up.

TABLE I

Coordinates for two Cepheids in Carina

Cepheid	*RA(2000)*	*DEC(2000)*
ET Car	$10^h\ 11^m\ 24^s{\cdot}3$	−61° 29′ 10″
EW Car	$10^h\ 20^m\ 49^s{\cdot}7$	−61° 15′ 07″

The BVI_c measurements obtained for the two Cepheids are summarized in Table II as a function of Heliocentric Julian Date, and are plotted in Fig. 2 with phasing from the following updated ephemerides:

$$\text{ET Car: JD}_{\max} = 2453502{\cdot}610 + 2{\cdot}910837\,E,$$
$$\text{EW Car: JD}_{\max} = 2453496{\cdot}590 + 4{\cdot}238730\,E,$$

where the original periods[2] have been adopted. The telescope tracking was not particularly reliable, so it was not possible to obtain CCD exposures of the fields for more than 30 seconds at a time. The signal-to-noise ratios for such faint stars were therefore insufficient to achieve a precision in the photometry of better than ± 0·02 magnitude in all bands.

The reason for adopting new light elements for the variables is because the data for EW Car with the original ephemeris are displaced in phase by roughly half a cycle relative to light maximum occurring at zero phase, while the data for ET Car display a smaller offset of roughly 0·1 cycle with the original elements. The original ephemerides for both stars were based on the moments of brightening between JD 2421035 and JD 2431907, and, strictly speaking, they were valid for that time interval only. Our experience indicates that period changes in Cepheids may be very large on a scale of even a dozen years[4], so it is necessary to be very careful when using light elements significantly removed in time from the epoch when they were established. In order to study the period changes in our two Cepheids, it would be necessary to fill in the large time gap between JD 2432000 and the present. The only way to do that is to estimate magnitudes of the stars on

TABLE II

CCD observations of ET Car and EW Car

HJD	*V*	*B–V*	*V–I_c*	*B*	*I_c*
		ET Car			
2453487·5321	14·906	...	1·122	...	13·784
2453488·4223	14·039	0·890	0·909	14·930	13·130
2453489·5075	14·666	1·054	1·152	15·729	13·514
2453490·4223	14·868	1·118	1·266	15·986	13·603
2453491·4212	14·196	...	0·963	...	13·233
2453492·4274	14·655	...	1·226	...	13·429
2453493·4627	14·815	...	1·143	...	13·672
2453495·4223	14·759	0·858	1·274	15·617	13·485
2453496·4523	14·672	0·755	1·169	15·428	13·503
2453497·4229	14·349	0·909	1·057	15·258	13·292
2453498·4199	14·641	...	1·112	...	13·499
2453499·4140	14·311	0·942	0·972	15·254	13·334
2453500·4235	14·336	0·925	1·069	15·261	13·267
2453501·4286	14·690	...	1·197	...	13·493
2453502·4296	13·892	0·712	0·771	14·604	13·122
2453503·4331	14·402	1·001	1·072	15·402	13·328
2453504·4245	14·726	1·004	1·188	15·730	13·538
2453506·5247	14·509	0·934	1·147	15·444	13·362
		EW Car			
2453487·5344	14·367	...	1·077	...	13·290
2453488·4258	14·014	0·949	1·047	14·955	12·979
2453489·5036	14·434	1·171	1·306	15·613	13·128
2453490·4177	14·563	1·383	...	15·915	...
2453491·4230	14·745	...	1·315	...	13·430
2453492·4288	13·995	...	1·066	...	12·929
2453493·4661	14·379	...	1·271	...	13·147
2453495·4259	14·945	0·889	1·454	15·865	13·474
2453496·4544	13·971	0·765	1·011	14·736	12·960
2453497·4260	14·299	0·850	1·201	15·149	13·099
2453498·4233	14·580	1·210	1·300	15·789	13·279
2453499·4176	14·774	1·025	1·388	15·814	13·385
2453500·4276	14·174	0·877	1·128	15·061	13·046
2453501·4329	14·091	...	1·078	...	13·014
2453502·4334	14·512	1·164	1·260	15·611	13·253
2453503·4374	14·711	1·144	1·445	15·854	13·266
2453504·4282	14·532	0·863	1·238	15·395	13·294
2453506·5276	14·476	1·038	1·287	15·514	13·189

old photographic plates. Neither is it possible to update the current light elements for both Cepheids using ASAS-3 data[5], because the large pixel size of the CCDs used in the ASAS project restricts the potential detection of all faint individual stars in such crowded fields.

In the *General Catalogue of Variable Stars*[6] ET Car and EW Car are listed as classical Cepheids of type DCEP. Petit[7], in analyzing all available data, denoted them as "Population I:". That conclusion can now be tested with the new observations. The blue amplitude for ET Car is $\sim 1^m{\cdot}4 - 1^m{\cdot}5$, whereas the maximum value observed for classical Cepheids of identical period is $\sim 1^m{\cdot}0$. Only Type II Cepheids have amplitudes that large at a period of 2·9 days[8]. The case for EW Car is a bit more complicated. Its blue amplitude is $\sim 1^m{\cdot}2$, comparable to the maximum value observed for classical Cepheids of its period. Yet it also has a curious light curve: skewed like that of a fundamental-mode classical Cepheid in

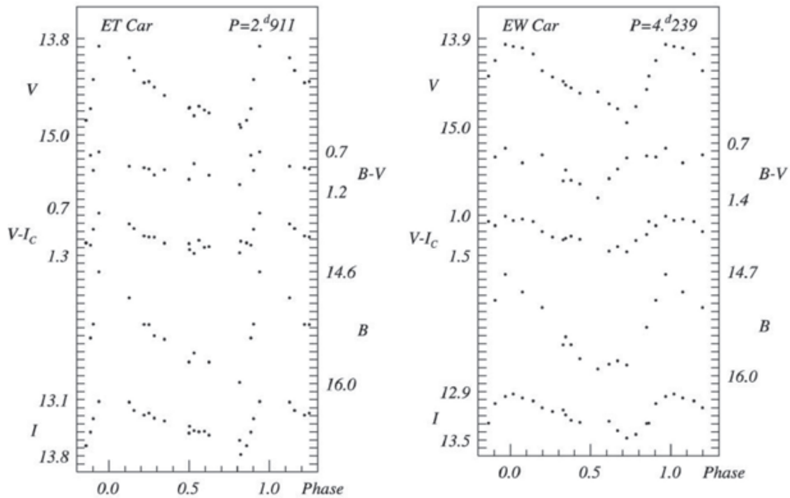

FIG. 2

Light and colour curves for ET Car (left) and EW Car (right) with the new ephemerides presented here.

yellow light, but almost sinusoidal in the blue. Such behaviour is atypical of a classical Cepheid. Given the short pulsation periods of the two Cepheids, the shapes of their light curves shown, and their large pulsation amplitudes, it seems likely that both variables are Type II Cepheids, most likely CWB type variables.

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THE SPECTROSCOPIC ORBIT AND TIDAL CIRCULARIZATION OF HD 8634

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Two formally incompatible spectroscopic orbits with eccentricities 0·38 and 0·28 have been published for the 5·4-day, single-lined binary HD 8634. We re-observed this system and derived a new orbit from our velocities. Moreover, all available data fit a common orbital solution with $e = 0{\cdot}27$. Although most binaries of spectral type F5 have circular orbits at such short periods, the non-zero eccentricity is expected for HD 8634 because the primary is not yet convective. If this system were in the process of rapid circularization (as one could naïvely infer from the decreasing eccentricity), its period would be decreasing, and such small period changes would actually be detectable with the long time span of the data available for HD 8634. We argue that if a suitable candidate binary in the stage of rapid circularization is found, the rate of tidal dissipation can be measured by accurately monitoring its period over several decades.

Introduction

Radial velocities of stars of spectral types later than F5 can be measured with a high precision, hence their spectroscopic orbits are usually well determined. Essentially all such binaries with periods shorter than 10 days are circularized by tides (*e.g.*, Fig. 5 of Duquennoy & Mayor[1]). The 5·4-day spectroscopic binary HD 8634 with eccentric orbit and F5 III primary contradicts this trend, hinting that circularization may still go on in this system. Two high-quality orbits of HD 8634 with very different eccentricities, $e = 0{\cdot}378 \pm 0{\cdot}02$ and $e = 0{\cdot}28 \pm 0{\cdot}03$, have been published. We re-observed this binary in an attempt to find the reason for this contradiction.

Data overview

HD 8634 = HR 407 = HIP 6669 (2000: $\alpha = 1^h 25^m 35{\cdot}7^s$, $\delta = +23° 30' 42''$) is a 6th-magnitude star of spectral type F5 III. The *Hipparcos* parallax is $13{\cdot}0 \pm 0{\cdot}8$ mas (distance modulus $4^m{\cdot}43 \pm 0^m{\cdot}14$) and the proper motion is (+34, −22) mas per yr. The *WBVR* photometry from Kornilov *et al.*[2] is (V, $W-B$, $B-V$, $V-R$) = (6·182, −0·086, 0·444, 0·385), while the photometry from 2MASS[3] is (J,H,K) = (5·32, 5·13, 5·03). The star is located on the main sequence (MS) band in the $B-V$, $W-B$ two-colour diagram and has $B-V$ and $V-K$ colours appropriate for the F5 spectral type. The primary component is a sub-giant (Fig. 1); it was a late-A type star on the MS. Böhm-Vitense[5] gives

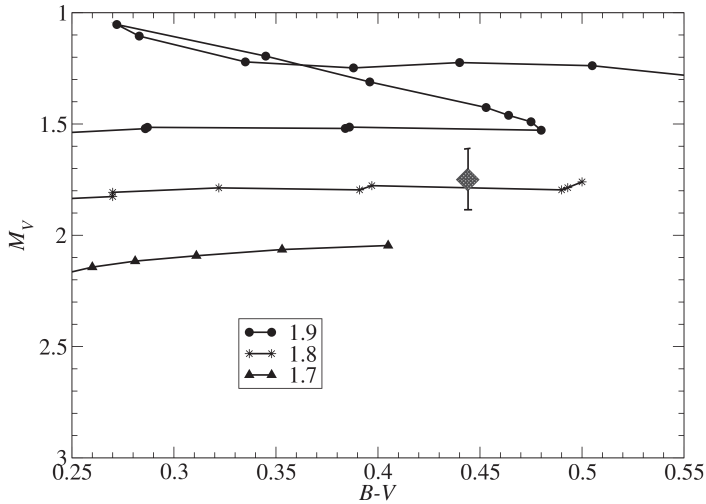

FIG. 1

Evolutionary tracks from Girardi *et al.*[4] for stars of 1·7, 1·8, and 1·9 $M_\odot$ up to the age of 1·6 Gy. The primary component of HD 8634 is marked by a large diamond at ($B-V$ = 0·444, M_V = 1·75). Its estimated age is 1·3 Gyr, mass 1·82 $M_\odot$.

basic parameters of this star: $\log T_e$ = 3·813, $v \sin i$ = 34 km s^{-1}, [Li/Fe] = 2·65. The presence of lithium confirms the sub-giant status of the primary, which has not yet developed an outer convective envelope and is still burning hydrogen.

The variability of the radial velocity was established by Plaskett[6] with four observations in 1918. Wright & Pugh[7] (WP54) observed this object in 1938–1952 and determined the first spectroscopic orbit with a period of 5·42906 days and eccentricity e = 0·378 ± 0·023, unusually high for such a short period. Assuming the primary mass to be 1·82 $M_\odot$ (Fig. 1), the minimum mass of the secondary is 0·19 $M_\odot$.

The spectroscopic orbit was re-determined by Mayor & Mazeh[8] (MM87) in search of precession caused by a tertiary companion, with a negative result. Curiously, a significantly lower eccentricity e = 0·28 ± 0·03 was found by MM87 than by WP54. They claim that their data are "inconsistent" with the period determined by WP54, although in fact they are not (see below).

Melo & de Medeiros[9] measured the rotation velocity of the primary to be $v \sin i$ = 31·5 km s^{-1} and noted that the axial and orbital rotations are not synchronized. They studied the ratio of X-ray to visible fluxes as an indicator of coronal activity and noted that HD 8634 is less active compared to other giants in close binary systems. De Medeiros *et al.*[10] gave $v \sin i$ = 30·8 km s^{-1} and drew attention to the non-circularity of the orbit.

The radius of the primary is 3·0 $R_\odot$ from isochrone fitting, hence the average equatorial velocity corresponding to synchronization is 28·0 km s^{-1}. The synchronous velocity at periastron is $(1 - e^2)^{1/2}(1 - e)^{-2}$ = 1·8 times larger or 51 km s^{-1} (e = 0·27). The inclination is not known, so the measured $v \sin i$ = 31 km s^{-1} can correspond to pseudo-synchronous rotation.

The system HD 8634 is in fact triple. A faint physical tertiary component with a separation $1''\cdot47$ and position angle 70° was discovered in 2004 with adaptive optics by Tokovinin *et al.*[11]. The magnitude difference with the spectroscopic binary is $\Delta(J,H,K) = (6\cdot3, 5\cdot8, 5\cdot5)$, hence the tertiary component has $(J,H,K) = (11\cdot6, 11\cdot0, 10\cdot5)$. These magnitudes correspond to a MS dwarf of $\sim 0\cdot3\ M_\odot$ at the distance of HD 8634. The orbital period is of the order of 900 yr.

In order to check whether the change of the spectroscopic orbit is real, a set of 19 new observations was secured in 2006 September by NAG using the correlation radial-velocity-meter (RVM) on the 1-m telescope in Crimea. The radial velocities are measured with the RVM to a precision[12] of $0\cdot3$ km s^{-1}. However, in the case of HD 8634 rapid axial rotation and shallow contrast of the correlation profile lead to reduced precision. Moreover, a systematic offset of instrumental nature may appear, despite reference to radial-velocity standards. The velocities and residuals of the new orbit (E3) are listed in Table I. The internal measurement errors are determined in the process of fitting a Gaussian curve to the correlation profiles. The new data set is designated as TG06 and is analyzed below jointly with the two other data sets, WP54 and MM87.

Orbital solutions

Table II lists the elements of the spectroscopic orbits published by WP54 and MM87, in common notation. The 7th column contains the number of velocities and the weighted r.m.s. residuals. For consistency, we re-computed these orbits from the same data by using two different codes and obtained concordant results, also quoted in Table II as E1 and E2. We excluded the first four points by Plaskett from the WP54 data set (thus designated WP54*) and imposed a fixed period $P = 5\cdot42923$ days for all re-computed orbits. The period is determined by fitting all data simultaneously.

TABLE I

New observations of HD 8634 and residuals

JD – 2 400 000	*RV* *km s^{-1}*	*Err.* *km s^{-1}*	*O–C* *km s^{-1}*
53969·562	−18·10	1·46	1·44
53970·588	−20·38	2·56	3·06
53971·592	−18·48	1·68	−0·40
53972·553	1·76	0·83	0·94
53973·572	−1·10	0·92	0·18
53974·573	−16·33	1·05	−0·55
53975·578	−21·62	1·08	1·07
53977·560	−8·83	0·86	−0·12
53981·545	−22·38	1·33	1·02
53982·520	−17·77	0·83	−0·60
53985·519	−15·76	1·04	0·92
53986·510	−24·72	1·84	−1·80
53987·516	−21·52	1·03	−0·03
53989·502	5·02	0·95	1·08
53990·571	−14·08	1·11	−1·72
53991·533	−21·48	0·82	−0·28
53992·544	−26·02	1·80	−2·82
53993·589	−12·86	1·26	1·03
53994·557	3·29	0·89	−1·59

TABLE II

Spectroscopic orbits of HD 8634

P days	*T* *JD*	*e*	*w* °	K_1 km s^{-1}	γ km s^{-1}	*N* σ	*Author, orbit*
5·42908	2433243·762	0·378	322·5	14·50	−15·86	53	WP54 original
	±0·043	±0·023	±3·8	±0·46	±0·24	—	
5·42923	2433243·72	0·352	319·6	14·28	−15·72	49	WP54* (E1)
	±0·09	±0·043	±7·6	±0·88	±0·44	2·44	
5·4264	2449998·46	0·28	351	15·2	−14·8	18	MM87 original
±0·0009	±0·12	±0·03	±9	±0·4	±0·4	1·3	
5·42923	2444998·25	0·290	335·2	15·52	−14·32	18	MM87 (E2)
	±0·10	±0·033	±7·9	±0·50	±0·39	1·25	
5·42923	2453972·77	0·246	337·5	14·39	−12·32	19	TG06 (E3)
	±0·08	±0·026	±5·9	±0·40	±0·25	1·04	
5·42923	2433243·93	0·274	334·3	14·78	−14·26	86	Combined (E4)
±0·00001	±0·05	±0·017	±3·6	±0·27	±0·17	1·62	

We also combined all three data sets in a common solution (Fig. 2). In doing so, we added offsets of $+1\cdot2$ and $-2\cdot0$ km s^{-1} to the WP54* and TG06 data, to bring into agreement the centre-of-mass velocities (Table III). The relative weights of the data sets were adjusted in order to reach $\chi^2/(N-M) \sim 1$ for each set separately. The WP54* data are assigned errors $\sigma_V = 4\cdot0/\sqrt{W}$, where W are the published weights (typically $W = 3$, $\sigma_V = 2\cdot3$ km s^{-1}). The true errors are not known; this estimate follows from the residuals to the E1 orbit. By similar argument, the errors given by MM87 are multiplied by 2·0 and the TG06 errors are left unmodified. The weights in the combined solution are inversely proportional to the square of the errors. The weighted r.m.s. residuals of three data sets to the individual orbital fits, σ, and the residuals to the orbits E1 and E4 are compared in Table III. In calculating the residuals to different orbits, the elements T_0 and γ are fitted, thus allowing for the shifts in time and velocity zero point for each set.

The data sets MM87 and TG06 are formally incompatible with the orbit E1: the probabilities of getting χ^2 larger than the observed ones, $P(\chi^2)$, are estimated as 10^{-4} and 2×10^{-6}, respectively. All three sets are compatible with the orbit E4, $P(\chi^2) = (0\cdot17, 0\cdot27, 0\cdot22)$ (this conclusion depends, of course, on the adopted errors). The point here is that no strong evidence of any significant orbit change is furnished by the data.

In search of possible period changes, the fitting program was modified to include the linear period drift A as an additional parameter. The formal fit gives $A = (dP/dt)/P = -1/\tau_P = (-2\cdot0 \pm 3\cdot8) \times 10^{-10}$ d^{-1}. The period decreases, with $\tau_P = 13$ Myr, but this number is not statistically significant. The 1σ lower limit is $\tau_P > 4\cdot7$ Myr.

TABLE III

Analysis of the data sets

Data set	*N*	*Error*	*Offset* km s^{-1}	σ km s^{-1}	σ(E1) km s^{-1}	σ(E4) km s^{-1}
WP54*	49	$4\cdot0/W$	+1·2	2·44	2·44	2·65
MM87	18	×2·0	0	1·25	2·12	1·37
TG06	19	×1·0	−2·0	1·04	1·87	1·13

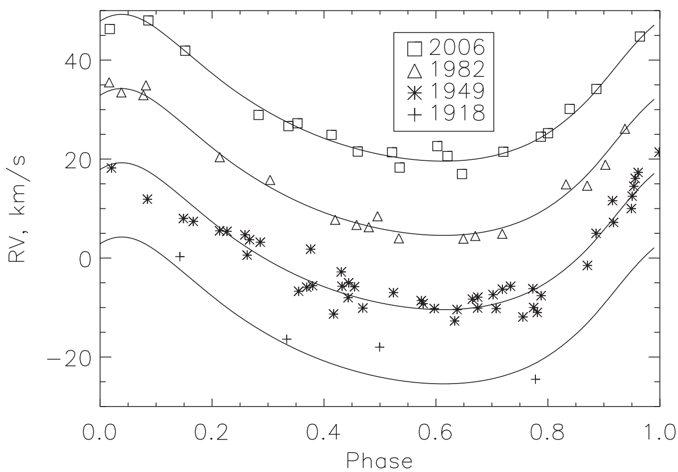

FIG. 2

New combined spectroscopic orbit of HD 8634 is compared to the individual data sets, labelled by their average epochs and displaced vertically by 15 km s^{-1} for clarity.

Discussion

Our study shows that great care is needed in the interpretation of old data. Even a formally significant change of some orbital element may be spurious. However, apparent changes of e can be also caused by a *real distortion of the radial-velocity curve* due to such effects as spots, blending with lines of a tertiary companion, or gas streams. We could demonstrate that all these effects are insignificant in the case of HD 8634. On the other hand, a real eccentricity modulation in the close triple system HD 109648 was detected by Jha *et al.*[13].

One could naïvely interpret the eccentricity changes in HD 8634 as being caused by tidal orbit circularization, but this process is too slow to be observable. The time scale of orbit circularization $\tau_e = -1/(\mathrm{d}\ln e/\mathrm{d}t)$ is a strong function of the ratio of semi-major axis a to component radius R, $\tau_e \propto (a/R)^8$ (see Eggleton[14] for the theory). Even in the most favourable cases (binaries with short periods and/or large radii), τ_e is longer than 1 Myr, leaving no hope of measuring this parameter directly.

However, if a circularizing binary evolves with *constant orbital angular momentum* proportional to $P^{1/3}(1 - e^2)^{1/2}$, its period P and eccentricity e are related by the condition

$$P\,(1 - e^2)^{3/2} = \mathrm{const.} \tag{1}$$

As e becomes smaller due to tides, P decreases as well. The period can be measured with a very high accuracy, so its small change caused by tidal evolution may actually be detectable. It can be inferred from Eqn. 1 that $\tau_P = -P/(\mathrm{d}P/\mathrm{d}t) = 2(1 - e^2)/(3e^2)\,\tau_e$. In eccentric binaries with $e > \sqrt{2/5}$, the period evolves even faster than the eccentricity, $\tau_P < \tau_e$. Tidal evolution of (P,e) also implies an exchange between orbital and rotational angular momenta, therefore Eqn. 1 is approximately valid only for binaries where the rotational angular momentum is much smaller than the orbital one. Careful modelling will be needed to relate period changes of tidally interacting binaries with the tidal dissipation rate, while Eqn. 1 is useful for preliminary estimates.

HD 8634 appeared to be an interesting case to test this idea. By adding a new data set to the two existing orbits, we could hope to detect a period change, or at least to place a limit. Indeed, the data presented above indicate that $\tau_P > 4 \cdot 7$ Myr. It turns out that HD 8634 is not expected yet to have circularized its eccentric orbit. Nevertheless, our project shows a feasibility of measuring on-going circularization in some other, more suitable binaries with time scales below a few Myr, given data of similar or better quality. This work is a 'training exercise' that might stimulate further attempts to measure the tidal-dissipation rate directly.

Binaries with rapid (*i.e.*, detectable) tidal circularization must be rare — either very young or with components expanding into giants ($\tau_e \propto R^{-8}$). If such a binary is found, its observation over a sufficiently long time will lead to the measurement of τ_P and hence to the measurement of the tidal dissipation.

Acknowledgements

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SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 199: HD 105443, HD 108576, HD 112276, AND HD 112641 WITH A PRELIMINARY DISCUSSION OF HR 4964

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Cambridge Observatories

This paper presents results on five more spectroscopic binaries in the vicinity of the North Galactic Pole. All of them are double-lined. The first three HD objects are all pairs of main-sequence stars having approximately solar-type primaries and orbits with periods of about five years and with eccentricities of 0·29, 0·66, and 0·64, respectively. The primary of HD 112641, a very unequal double system with a period of 87 days and an eccentricity of 0·25, appears to be an early-K subgiant — a type that (if it exists at all) ought to belong only in a very old population, membership in which is not confirmed by other properties of the system.

HR 4964 is very reminiscent of HD 113995, which was treated in Paper 194. Radial-velocity measurements demonstrate that in the late 1990s HR 4964 underwent nodal and periastron passages in an orbit whose period appears to be of the order of a century or more. Near the node it was seen as double-lined, possessing a weak secondary whose signature was always partly blended with that of the K-giant primary. It is surmised that the secondary is a main-sequence F star. The semi-axis of the proposed relative orbit is nearly 40 AU, which at the distance of HR 4964 should subtend an angle of about 0″·4.

Introduction

As part of the continuing fallout from the photometric and radial-velocity survey, published by Yoss & Griffin[1] some ten years ago, of all the late-type *Henry Draper Catalogue* stars within 15° of the North Galactic Pole (NGP), orbits are given here for five of the 125 spectroscopic binaries recognized in that work. The first four named in the title are about the ninth apparent magnitude (HD 105443 is a little fainter); only two of them, HD 108576 and HD 112641, feature in the *Hipparcos* survey. HD 105443 and HD 108576 are in the vicinity (but are not kinematic members) of the Coma Cluster, the former being about 4° preceding γ Com and the latter, which carries the Trumpler[2] number 143 in the cluster, about 3° south of it. HD 112276 lies about a degree and a half south of the fifth-magnitude star 35 Com (itself a binary, for which an orbit was published[3] about 20 years ago), while HD 112641 is at higher declination, in Canes Venatici, a degree and a half south of Cor Caroli (α CVn). The *HD* spectral types are K0, G5, K0, and K0, respectively.

Much of what is known about the four stars is to be found in the survey paper[1], which does not show up in the *Simbad* bibliographies. The survey data on them are listed here in Table I, which also includes the absolute magnitudes that are implied by the *Hipparcos*[4] data for the two stars for which they are available. It should be pointed out that the spectral types and luminosities were derived in the survey from *DDO*-style[5] photometry and are therefore some sort of brightness-weighted means of the values pertaining to the two components of each system; the luminosities are accordingly under-estimates of the totals for the systems, and the z-distances that follow from them and are tabulated in the survey paper[1] are correspondingly too small. Some of the other parameters listed for the double-lined stars in the survey[1] are equally suspect.

TABLE I

NGP Survey[1] results for the four stars

Star	*V*	*(B−V)*	*Type*	M_V	*Hipparcos* M_V
	m	*m*		*m*	*m*
HD 105443	9·62	0·57	G1 V	+4·6	
HD 108576	8·99	0·56	G0 V	+4·4	3·5 ± 0·35
HD 112276	8·96	0·63	G4 V	+4·9	
HD 112641	8·93	0·97	K2 IV−V	+5·8	3·3 ± 0·4

There is in the literature a small amount of additional material that is relevant to the stars treated here. In the case of HD 105443 it amounts only to three presumably independent spectral classifications. By virtue of the fact that it happens to be within Selected Area[6] 56, the star appears in the *Bergedorfer Spektral-Durchmusterung*[7], where it has the designation 56–271, is cross-referenced as BD +28° 2082, and is classified G0. Upgren[8], in a survey in which he designated it 28° 5, also gave it as G0. Hill[9] included it in a catalogue of the designations and positions of A and F stars in an NGP area*, and gave a type of F8, from one of the 11 sources from which his catalogue was drawn. It has been traced to the *AGK3*[10], which in turn says that it is "from the HD or Vyssotsky"; since the *HD* type is K0, by a process of elimination we deduce that it must be from Vyssotsky, and indeed a comb through his publications locates it[11]. We have to recall again that all classifications of double-lined objects whose components are not exactly similar to one another must represent some sort of mean type. All the same, the more recent classifications are certainly more plausible than the *HD* type for HD 105443; it has been noticed that, at any rate in the NGP field, many of the dwarfs have *HD* types that are too late. In fact, of the 327 dwarfs included in the NGP survey[1], which was restricted to stars classified G5 and later in the *Henry Draper Catalogue*, no fewer than 152 — almost half — were indicated by *DDO* photometry to be earlier than the ostensible cut-off.

HD 108576 has naturally featured in a number of listings of stars in the NGP field, but the only times that it has been singled out for attention are when it has been used (as it has in three different cases[12–14]) as the comparison object for a variable star. Knude[15], and independently Nordström *et al.*[16], interpreted $ubvy\beta$ photometry in terms of absolute magnitude, metallicity, *etc.*, but since they did

*Opening his *Introduction* to the paper, Hill laments that observations of the same star often get published under different names, associated with catalogues having different epochs, and the situation is only resolved by creating a cross-indexing catalogue, which is what his paper sets out to provide. There is an entry for BD +28° 2082, but the *HD* column that might be expected to carry the number 105443 is blank.

not take account of the fact that the object is double-lined their results may not be very meaningful. Eggen[17] included the star in a large photometric investigation in which it is listed with $V = 8^m \cdot 99$, $(B-V) = 0^m \cdot 56$, $(U-B) = 0^m \cdot 00$. Duflot & Fehrenbach[18] classified the star as G0 V and published a mean radial velocity of -16 km s^{-1} with a 'probable error' of $4 \cdot 6$ km s^{-1} from four measurements made by their objective-prism method. Sandage & Fouts[19] gave a value of $-8 \cdot 0$ km s^{-1}, in a project that yielded velocities with r.m.s. errors of $4 \cdot 7$ km s^{-1} notwithstanding that they were obtained with the coudé spectrograph of the 100-inch reflector. Nordström *et al.*[16] listed a mean of $-6 \cdot 5 \pm 0 \cdot 4$ km s^{-1} from 12 *Coravel* velocities spanning a total time interval of 3951 days. In none of those papers are any dates given; even if the dates were available, refs. 18 and 19 could not be expected to contribute usefully to an orbit for HD 108576, while in the case of ref. 16 most (if not all) of the observations must be the present writer's own, which he permitted to be utilized by the authors concerned; the only question is why the number of available velocities is listed as only 12 when there are in fact 24 of the writer's measurements of HD 108576 on the *Coravel* data base from which Nordström *et al.*'s listing was derived.

The only paper retrieved by *Simbad* for HD 112276 is one by Eggen[20] on 'old-disk-population red giants'; it does not in fact refer to our star at all but to HD *112278*.

HD 112641 fares perhaps slightly better in the literature. There is *BV* photometry of it by Häggkvist & Oja[21], $V = 8^m \cdot 94$, $(B-V) = 0^m \cdot 95$, and under the alias 37° 187 there is a spectral classification[22], that the parallax (if nothing else) shows to be certainly erroneous, of G9 III. The object features in the NGP investigation of Soubiran *et al.*[23], who listed *V* and $(B-V)$ transformed from *Tycho 2* magnitudes (not likely to be as accurate as the ones deliberately measured in the *UBV* system), and made one spectroscopic measurement of each star on their programme. They noticed that HD 112641 was double-lined, but there is only one set of atmospheric parameters. The radial velocity (of the primary star?) is given as $-0 \cdot 80$, but there is no date, so it cannot be compared with the orbital solution derived below.

Radial velocities and orbits

After two more or less unsuccessful attempts had been made at Cambridge, the radial velocity of HD 105443 was first measured with the spectrometer[24] at the 200-inch Palomar coudé in 1973. The next observation was not until 1984, and systematic measurements did not begin until 1988. Even then the double-lined nature of the system was not really recognized until the nodal passage of 1993. That is not surprising, since the secondary dip is a weak feature that, even at the node of the ~5-year orbit, remains so badly blended with the primary that the asymmetry of the dip is easily overlooked in traces where the integration has not been prolonged in a deliberate effort to obtain a better *S/N* ratio than would normally be regarded as satisfactory for a single-lined object. In further extenuation, it could be remarked that the object is faint for an *HD* star, and its early type means that even the primary dip is quite shallow. Fig. 1 shows a trace obtained right at the node and so demonstrates the most favourable situation that is ever presented for seeing the object as double-lined.

The radial-velocity observations made of HD 105443 include 28 obtained with the OHP *Coravel*, 31 with the Cambridge one, seven with the original spectrometer at Cambridge, two at ESO, and one each at the DAO and Palomar —

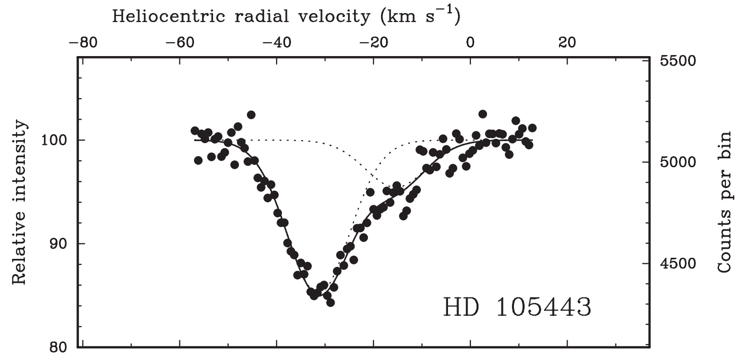

FIG. 1

Radial-velocity trace of HD 105443, obtained with the Cambridge *Coravel* on 2002 April 23 and showing the two dips at practically their maximum separation.

70 in all. Not all of them, however, have proved to contribute usefully to the solution of the orbit. All but three of the Cambridge *Coravel* traces have been reduced as double-lined and are useable, the odd three having been made at phases when the two dips were almost exactly superposed on one another. It is a pleasure to be able to report that it has now proved possible once again to obtain fresh reductions of traces that were made with the OHP and ESO *Coravel*s and are stored in the data base in Geneva, and I am much indebted to Dr. S. Udry and Mr. P. Figueira for furnishing new output for all the stars discussed in this paper. In the case of HD 105443, however, fewer than half the OHP traces can usefully be treated as double-lined, owing to inadequate integration times. Many of the early observations were terminated after only a minute or so at levels of about 500 counts per bin, which though sufficient for single-lined objects is simply not enough to enable meaningful pairs of velocities to be disentangled from traces that always consist of very unequal blends. Having at length been convinced of the duplicity of HD 105443 at its nodal passage in early 1993 (it was suspected previously, in the observation of 1991 January 31) and obtained traces with count levels* of about 10 000 instead of 500, the writer reprehensibly omitted to flag it on his observing programme and did not remember the duplicity until it became apparent again at the ensuing nodal passage in 1997. From that time onwards, appropriate integration levels were always the aim.

A review of the results of reducing all the OHP traces as double-lined convinced the author that they were altogether unreliable at count levels less than about 1000 per bin. It seemed unscientific to pick and choose which to believe on the basis of whether one liked the individual results: better to apply dispassionately an objective cut-off level, which was set by inspection at 1200/bin. It followed from that decision that only ten of the total of 30 OHP and ESO observations passed muster and are shown in Table II as double-lined. For the rest, we have to be content with single-lined reductions, which are of course not useable in the solution of the orbit and are plotted with different, less conspicuous, symbols in

*Count levels of OHP traces are not directly comparable with those of Cambridge ones, because the 'bins' of the OHP system are about twice as wide as the Cambridge ones (about 1·2 km s^{-1} against 0·6 km s^{-1} at Cambridge). Thus equivalent results require twice as many counts per bin at OHP as at Cambridge, because there are only half as many bins delineating the 'dip'.

Table II

Radial-velocity observations of HD 105443

Except as noted, the sources of the observations are as follows:
1988–1998 — Haute-Provence Coravel; 2000–2007 — Cambridge Coravel

Date (UT)	*MJD*	*Velocity* Prim. km s−1	*Velocity* Sec. km s−1	*Phase*	*(O−C)* Prim. km s−1	*(O−C)* Sec. km s−1
1973 June 13·21★	41846·21	−23·5		0·913	—	—
1984 Apr. 25·87†	45815·87	−27·4		3·251	—	—
1988 Mar. 13·03	47233·03	−31·2	−14·6	4·086	0·0	+0·8
Nov. 7·20	472·20	−27·2		·227	—	—
1989 Feb. 23·25‡	47580·25	−26·5		4·291	—	—
Mar. 25·05	610·05	−26·0		·308	—	—
Apr. 28·01	644·01	−24·9		·328	—	—
June 4·92†	681·92	−23·4		·351	—	—
1990 Jan. 31·06	47922·06	−21·3	−28·2	4·492	+0·8	−2·1
Feb. 12·28‡	934·28	−23·1		·499	—	—
Mar. 26·86†	976·86	−20·7		·524	—	—
Apr. 29·94†	48010·94	−23·4		·544	—	—
Dec. 27·16†	252·16	−21·1		·687	—	—
1991 Jan. 28·11	48284·11	−19·0	−32·0	4·705	−0·5	−1·7
Feb. 6·10	293·10	−20·3		·711	—	—
May 2·91†	378·91	−20·0		·761	—	—
June 12·92†	419·92	−19·5		·785	—	—
1992 Jan. 15·06	48636·06	−22·7		4·913	—	—
Feb. 27·40§	679·40	−25·4		·938	—	—
Apr. 22·93	734·93	−25·6		·971	—	—
June 20·90	793·90	−26·6		5·006	—	—
Aug. 15·83	849·83	−27·1		·039	—	—
1993 Feb. 13·05	49031·05	−30·9	−16·9	5·145	+0·1	−1·2
Mar. 19·02	065·02	−31·0	−16·8	·165	−0·4	−0·7
July 7·90	175·90	−27·2		·231	—	—
Dec. 26·08	347·08	−25·8		·332	—	—
1994 Jan. 8·13	49360·13	−23·9		5·339	—	—
Feb. 19·15	402·15	−24·8		·364	—	—
Apr. 30·93	472·93	−24·5		·406	—	—
Aug. 3·84	567·84	−22·9		·462	—	—
Dec. 29·19	715·19	−21·5		·549	—	—
1995 Jan. 3·12	49720·12	−21·2		5·551	—	—
June 2·93	870·93	−19·8	−29·7	·640	−0·5	−0·4
Dec. 27·15	50078·15	−21·2		·762	—	—
1996 Mar. 30·03	50172·03	−21·6		5·818	—	—
1997 Feb. 8·11¶	50487·11	−28·2	−18·9	6·003	−0·4	+0·5
Mar. 28·98¶	535·98	−28·3	−12·3‖	·032	+1·2	+5·1
Apr. 7·99¶	545·99	−30·2	−15·6	·038	−0·4	+1·4
26·00	564·00	−30·7	−16·6	·049	−0·4	−0·1
May 10·93¶	578·93	−30·7	−14·8	·057	−0·1	+1·3
July 21·86	650·86	−31·3	−15·6	·100	0·0	−0·3
Dec. 25·16	807·16	−30·3	−16·8	·192	−0·3	+0·1
1998 May 1·91	50934·91	−28·5	−18·7	6·267	−0·6	+0·6

TABLE II (*concluded*)

Date (UT)	*MJD*	*Velocity* Prim. km s^{-1}	*Velocity* Sec. km s^{-1}	*Phase*	*(O – C)* Prim. km s^{-1}	*(O – C)* Sec. km s^{-1}
2000 Jan. 10·18	51553·18	−18·9	−32·5	6·631	+0·6	−3·3
Feb. 14·18	588·18	−18·9	−29·6	·652	+0·3	−0·1
Apr. 6·95	640·95	−18·6	−27·9	·683	+0·1	+2·1
June 6·93	701·93	−19·0	−31·1	·719	−0·6	−0·7
2001 Jan. 6·25	51915·25	−18·2	−29·9	6·845	+0·7	0·0
Dec. 15·26	52258·26	−29·3	−13·7	7·047	+0·9	+2·9
2002 Jan. 18·19	52292·19	−30·8	−16·0	7·067	+0·1	−0·2
Feb. 27·07	332·07	−31·0	−15·6	·090	+0·3	−0·3
Mar. 27·06	360·06	−31·8	−15·7	·107	−0·5	−0·4
Apr. 24·01	388·01	−30·9	−14·9	·123	+0·3	+0·5
2003 Jan. 16·21	52655·21	−27·4	−19·3	7·281	+0·1	+0·4
Feb. 21·03	691·03	−27·2	−20·0	·302	−0·3	+0·4
Mar. 17·00	715·00	−26·6	−20·6	·316	−0·1	+0·3
May 7·94	766·94	−25·0		·346	—	—
2004 Feb. 26·11	53061·11	−21·5	−26·9	7·520	0·0	−0·1
Apr. 15·01	110·01	−21·1	−25·7	·549	−0·2	+1·7
May 16·93	141·93	−20·6	−26·4	·567	0·0	+1·5
2005 Jan. 22·16	53392·16	−18·4	−29·2	7·715	0·0	+1·2
Apr. 3·97	463·97	−18·8	−30·6	·757	−0·6	+0·1
May 14·96	504·96	−17·9	−29·1	·781	+0·3	+1·6
2006 Mar. 3·11	53797·11	−24·6		7·953	—	—
4·05	798·05	−25·0		·954	—	—
May 31·93	886·93	−27·7	−18·3	8·006	+0·3	+0·9
June 24·94	910·94	−29·5	−18·2	·020	−0·6	−0·1
2007 Feb. 3·15	54134·15	−31·1	−19·7	8·152	−0·3	−3·9
Apr. 10·95	200·95	−29·6	−18·3	·191	+0·4	−1·5
May 25·00	245·00	−29·0	−16·6	·217	+0·3	+1·1

*Observed with Palomar 200-inch telescope.
† Observed with original spectrometer.
‡ Observed with ESO *Coravel*.
§ Observed with DAO 48-inch telescope.
¶ Observed with Cambridge *Coravel*.
‖ Rejected measurement.

the orbit diagram (Fig. 2). Similarly, all the measurements made with the original spectrometer at Cambridge were reduced as single-lined, and so were the isolated measures made at Palomar and the DAO, both of which happened to be made at phases when there was no possibility of recognizing the duplicity. Thus the orbital elements depend upon a total of 38 double-lined measurements, of which 28 are from Cambridge and ten from OHP. Owing to the great inequality between the two dips, velocities derived from single-lined reductions naturally tend to follow the variation of the primary's velocity but with a muted amplitude.

The observed velocities are set out in Table II, in which the OHP and ESO ones have been accorded the usual zero-point adjustment of +0·8 km s^{-1}, which has also been applied to all the other velocities listed in this paper from the same sources. The Cambridge *Coravel* data have been adjusted by −0·5 km s^{-1}, a value suggested by previous experience to be appropriate to an object of the relevant colour. The single-lined reductions are listed mid-way between the columns for

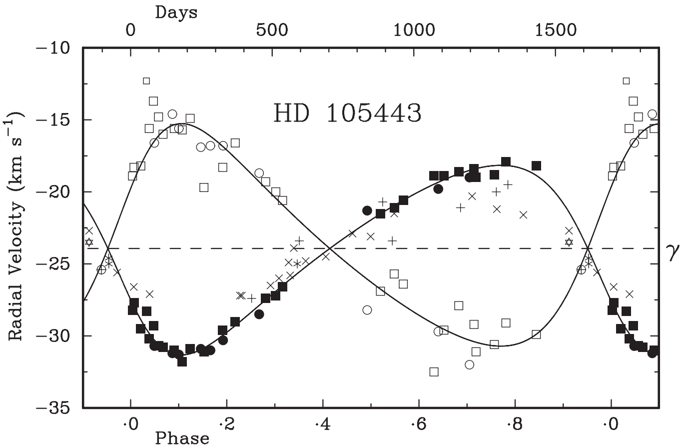

FIG. 2

The observed radial velocities of HD 105443 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. Filled squares and circles represent Cambridge and OHP measurements, respectively, of the primary star; the corresponding open symbols plot measurements of the secondary. The one rejected observation is plotted with a smaller symbol. Radial velocities that were measured as if the system were single-lined naturally tend to follow the phasing of the primary but with somewhat reduced amplitude. The sources of such velocities are as follows: cross — OHP or ESO; plus — the original photoelectric radial-velocity spectrometer at Cambridge; open star — Palomar; circle with a plus in it — DAO; asterisk — Cambridge *Coravel*.

the primary and secondary. The Cambridge and OHP velocities have merited equal weights in the orbital solution; it has been necessary to weight the measurements of the secondary star $^{1}/_{10}$ in comparison with the primary to equalize the variances for the two components. One Cambridge observation of the secondary gives an excessive residual and has been rejected. The solution is plotted in Fig. 2, and its elements are listed, with those of the other three *HD* stars, in Table VI later in the paper.

The first observation of HD 108576 was made with the original Cambridge spectrometer in 1980; the next was not made until 1987, but it showed a large

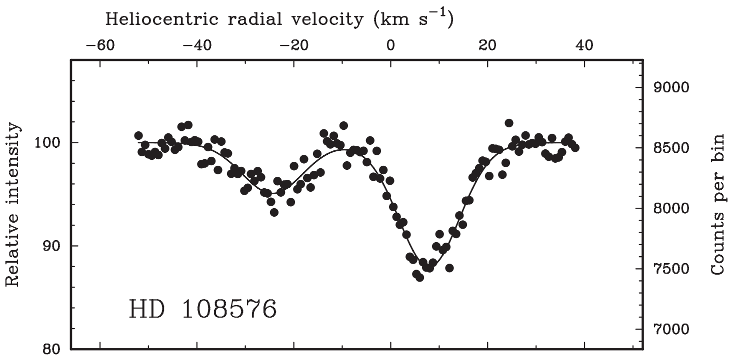

FIG. 3

Radial-velocity trace of HD 108576, obtained with the Cambridge *Coravel* on 2007 March 2 during the short interval of phase when the two dips could be seen entirely separated.

Table III

Radial-velocity observations of HD 108576

Except as noted, the sources of the observations are as follows:
1988–1998 — Haute-Provence Coravel, weighted 1/4 in orbital solution;
1999–2007 — Cambridge Coravel

Date (UT)	*MJD*	*Velocity* Prim. km s⁻¹	*Velocity* Sec. km s⁻¹	*Phase*	*(O − C)* Prim. km s⁻¹	*(O − C)* Sec. km s⁻¹
1980 Jan. 13·18*	44251·18	+8·0	—	0·034	−3·2	—
1987 Mar. 1·01	46855·01	−5·8	−9·1	1·357	−0·3	−1·3
21·98*	875·98	−7·2		·367	—	—
Dec. 8·25*	47137·25	−7·3		·500	—	—
1988 Jan. 26·54†	47186·54	−7·0		1·525	—	—
Mar. 15·15	235·15	−10·4	−1·9	·550	−0·6	+1·1
1989 Mar. 26·03	47611·03	−9·9		1·741	—	—
1990 Feb. 15·36‡	47937·36	−16·1	+4·4	1·907	−0·2	+0·5
Mar. 13·42†	963·42	−16·5	+3·0	·920	−0·8	−0·7
1991 Feb. 4·21	48291·21	+7·4	−21·2	2·086	+0·7	+0·4
1992 Jan. 15·07	48636·07	−5·1		2·262	—	—
Feb. 27·47†	679·47	−6·6		·284	—	—
Apr. 23·02	735·02	−5·7		·312	—	—
Dec. 20·19	976·19	−6·9		·434	—	—
1993 Feb. 12·14	49030·14	−7·3		2·462	—	—
Mar. 23·06	069·06	−7·2		·482	—	—
Dec. 28·19	349·19	−8·0		·624	—	—
1994 Feb. 21·07	49404·07	−8·6		2·652	—	—
May 1·95	473·95	−8·9		·687	—	—
Dec. 12·20	698·20	−14·0	+2·9	·801	+0·6	+0·4
1995 Jan. 5·15	49722·15	−14·6	+3·8	2·813	+0·3	+1·1
May 30·99	867·99	−16·9	+2·3	·887	−1·0	−1·6
June 6·89	874·89	−16·2	+3·1	·891	−0·2	−0·9
1996 Jan. 1·20	50083·20	+4·3	−12·0	2·997	+2·0	+4·6
Mar. 30·08	172·08	+10·5	−25·7	3·042	−0·3	+0·5
1997 Feb. 8·14§	50487·14	−0·7	−15·2	3·202	−0·2	−1·7
Mar. 29·03§	536·03	−1·4	−12·5	·227	+0·1	−0·2
Apr. 16·00§	554·00	−2·5	−12·5	·236	−0·7	−0·6
May 10·97§	578·97	−2·5	−11·6	·249	−0·2	−0·2
July 26·85	655·85	−5·9		·288	—	—
Dec. 24·23	806·23	−6·7		·364	—	—
1998 May 1·96	50934·96	−6·7		3·429	—	—
1999 Dec. 20·23	51532·23	−13·7	−1·0	3·733	−0·4	−2·0
2000 Jan. 10·22	51553·22	−12·8	+3·1	3·743	+0·7	+1·9
Feb. 11·16	585·16	−13·6	+2·1	·760	+0·2	+0·5
Mar. 4·07	607·07	−13·9	+2·6	·771	+0·2	+0·8
Apr. 5·95	639·95	−14·3	+2·0	·787	+0·1	−0·2
June 10·92	705·92	−15·5	+3·9	·821	−0·5	+1·0
Nov. 20·25	868·25	−16·3	+3·2	·903	−0·3	−0·7

TABLE III (*concluded*)

Date (UT)	*MJD*	*Velocity* Prim. km s⁻¹	*Velocity* Sec. km s⁻¹	*Phase*	*(O – C)* Prim. km s⁻¹	*(O – C)* Sec. km s⁻¹
2001 Jan. 11·21	51920·21	−15·5	+3·0	3·930	−0·1	−0·3
Mar. 5·13	973·13	−12·6	+0·9	·957	+0·1	+0·6
May 4·99	52033·99	−2·2	−12·8	·988	+0·2	−1·5
11·97	040·97	−0·4	−13·9	·991	+0·2	−0·6
28·98	057·98	+3·6	−17·7	4·000	−0·3	+0·7
June 7·93	067·93	+5·3	−20·4	4·005	−1·0	+0·6
27·94	087·94	+9·9	−24·8	·015	+0·3	0·0
2002 Jan. 2·19	52276·19	+4·2	−18·9	4·111	−0·5	+0·4
Feb. 21·17	326·17	+3·1	−17·0	·136	+0·1	+0·3
Mar. 28·03	361·03	+2·0	−15·4	·154	+0·1	+0·7
Apr. 19·97	383·97	+1·6	−15·1	·165	+0·3	+0·3
2003 Jan. 28·15	52667·15	−4·0	−10·8	4·309	+0·2	−1·6
2004 May 22·94	53147·94	−9·4	−2·5	4·553	+0·4	+0·4
July 2·92	188·92	−10·8	−3·6	·574	−0·6	−1·1
2005 Jan. 11·23	53381·23	−11·9	0·0	4·672	+0·2	+0·4
Mar. 25·07	454·07	−12·0	+0·9	·709	+0·8	+0·5
Dec. 18·26	722·26	−15·4	+1·7	·845	0·0	−1·7
2006 Feb. 16·12	53782·12	−15·7	+4·2	4·876	+0·2	+0·4
Nov. 19·25	54058·25	+9·9	−26·0	5·016	+0·1	−1·0
Dec. 17·27	086·27	+12·0	−27·8	·030	+0·8	−1·2
2007 Jan. 14·25	54114·25	+9·7	−25·0	5·044	−1·0	+1·0
Feb. 4·15	135·15	+10·1	−25·5	·055	+0·3	−0·5
Mar. 2·11	161·11	+9·1	−23·4	·068	+0·6	+0·1
22·10	181·10	+7·3	−22·3	·078	−0·2	+0·1
Apr. 29·95	219·95	+5·8	−19·9	·098	+0·1	+0·5

*Observed with original spectrometer; wt. 0.
†Observed with DAO 48-inch telescope; wt. 1/4.
‡Observed with ESO *Coravel*; wt 1/4.
§Observed with Cambridge *Coravel*.

change in velocity, and the star was immediately taken onto the spectroscopic-binary programme and has been observed tolerably regularly ever since. The double lines (illustrated in Fig. 3) were not discovered until the star was observed at ESO early in 1990 as it was approaching the favourable nodal passage in its high-eccentricity five-year orbit. Altogether there are 64 radial-velocity observations, set out in Table III; 36 of them have been made with the Cambridge *Coravel*, 21 at OHP, three with the old Cambridge spectrometer, three at the DAO, and one at ESO. All of the new Cambridge traces have been reduced as double-lined, as have one from the DAO and nine of the OHP ones, in addition to the ESO observation. The discrepancy between the 21 OHP measures referred to here and the 24 mentioned above as being available in principle to Nördstrom *et al.* arises because (*a*) the one ESO measure is also in the Geneva data base, (*b*) three observations were made consecutively on one evening at OHP and have been averaged in this paper.

For HD 108576, as for HD 105443, the Cambridge *Coravel* observations have been adjusted by $-0 \cdot 5$ km s^{-1}; it has been found appropriate to weight the OHP measures 1/4, and to give all velocities of the secondary 1/4 of the weight to which

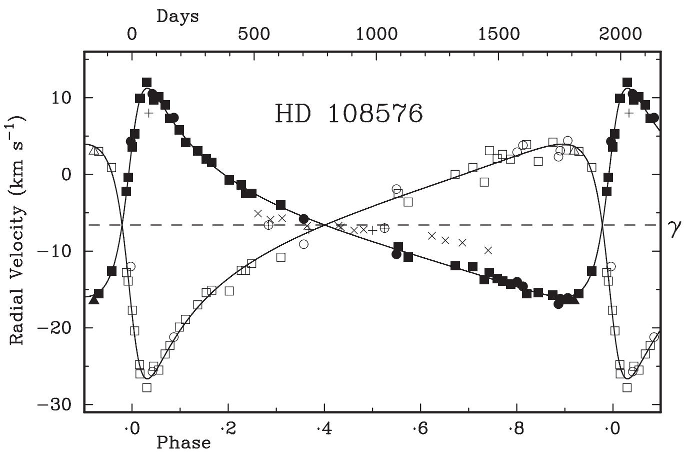

FIG. 4

As Fig. 2, but for HD 108576. The coding of sources is the same as in Fig. 2, but here there is in addition a resolved DAO observation, plotted as filled and open triangles.

the corresponding primary would be entitled. The ESO and DAO SB2 velocities have been weighted like the OHP ones. The very first (1980) measurement made at Cambridge fortuitously falls right at the node, where it must represent a measure of the primary alone; it is not obvious how it should be weighted except that its weight must be very small, so it has been given zero weight, like the other 'old Cambridge' measures which are both blends. The orbit is shown in Fig. 4, and the elements appear in Table VI below.

The radial velocity of HD 112276 was measured first at Cambridge as long ago as 1971; a second observation, made at Palomar in 1973, was discordant with it, but not by quite enough, in view of the very 'feeble' dips in the traces, to be certainly significant or to arouse any enthusiasm for spending much time on such a difficult object. Four further observations in the ensuing fifteen years all gave velocities intermediate between the first two. The breakthrough came in 1989, when the OHP *Coravel* revealed the object as double-lined, with almost equal components; since then it has been observed fairly systematically, and the 60 measurements set out in Table IV have been accumulated. A Cambridge trace obtained during the brief phase in the five-year orbit when the system is truly double-lined appears as Fig. 5. The observations comprise 33 made with the Cambridge *Coravel*, 21 with the OHP one, four with the original spectrometer at Cambridge, and one each from Palomar and ESO. Fifty-four of the 60 observations have yielded double-lined reductions; only the 'original spectrometer' and Palomar ones, and just one Cambridge *Coravel* measure that was made at a time when the velocities of the components were practically identical, have been treated as single-lined. The Cambridge *Coravel* observations of HD 112276 have been adjusted by $-0 \cdot 5$ km s^{-1}, like those of the two stars already described; all 54 pairs of velocities have been given the same weight in the solution of the orbit, from both sources and both components. The resulting solution is plotted in Fig. 6 and its elements are given in Table VI below.

The first two velocities of HD 112641 were obtained by G. A. Radford (at that time a research student) with the original Cambridge spectrometer in 1973 and

TABLE IV

Radial-velocity observations of HD 112276

Except as noted, the sources of the observations are as follows:
1988–1998 — Haute-Provence Coravel; 1999–2007 — Cambridge Coravel

Date (UT)	MJD	Velocity Prim. km s⁻¹	Velocity Sec. km s⁻¹	Phase	(O − C) Prim. km s⁻¹	(O − C) Sec. km s⁻¹
1971 Jan. 4·18*	40955·18	+8·1		0·435	—	—
1973 June 12·15†	41845·15	+3·6		0·898	—	—
1975 Apr. 20·02*	42522·02	+4·0		1·251	—	—
1977 Feb. 4·19*	43178·19	+5·6		1·593	—	—
1986 May 18·98	46568·98	+7·4		3·360	—	—
1988 Mar. 13·06	47233·06	+4·0	+8·5	3·706	−0·1	−0·2
1989 Mar. 26·08	47611·08	−3·9	+16·3	3·903	−0·1	−0·5
Apr. 29·99	645·99	−5·1	+18·2	·921	−0·1	+0·1
May 2·93	648·93	−4·8	+18·1	·923	+0·4	−0·1
1990 Jan. 27·14	47918·14	+12·6	+0·5	4·063	+0·1	+0·3
Feb. 12·34‡	934·34	+12·1	−0·1	·071	−0·9	+0·2
1991 Feb. 6·17	48293·17	+11·6	+1·0	4·258	−0·5	+0·5
1992 Jan. 18·16	48639·16	+8·8	+3·7	4·438	−0·5	+0·3
Apr. 27·03	739·03	+7·4	+6·0	·491	−1·0	+1·7
Dec. 21·23	977·23	+4·7	+7·9	·615	−1·5	+1·3
1993 Feb. 14·14	49032·14	+5·5	+7·6	4·643	−0·1	+0·4
Dec. 30·22	351·22	+1·6	+12·6	·810	+0·6	+0·7
1994 Feb. 19·18	49402·18	+0·2	+13·1	4·836	+0·3	0·0
Apr. 30·02	472·02	−2·2	+14·6	·872	−0·3	−0·3
Aug. 7·83	571·83	−5·4	+17·6	·924	−0·1	−0·7
Dec. 14·21	700·21	−5·0	+17·0	·991	−0·3	−0·8
1995 Jan. 7·16	49724·16	−0·8	+14·5	5·004	0·0	+0·8
June 4·98	872·98	+13·4	−0·8	·081	0·0	−0·1
1996 Mar. 31·02	50173·02	+12·3	−0·5	5·238	−0·1	−0·7
1997 Mar. 31·04§	50538·04	+9·6	+3·6	5·428	+0·1	+0·4
Apr. 30·94§	568·94	+9·8	+3·5	·444	+0·6	0·0
July 26·87	655·87	+9·0	+3·2	·489	+0·5	−1·1
Dec. 25·19	807·19	+7·3	+4·5	·568	+0·2	−1·2
1998 July 9·88	51003·88	+4·0	+9·2	5·671	−1·0	+1·4
1999 Dec. 20·27	51532·27	−6·7	+19·8	5·946	+0·1	−0·1
2000 Jan. 9·20	51552·20	−8·1	+19·7	5·956	−0·7	−0·8
Feb. 11·13	585·13	−7·9	+20·9	·973	−0·5	+0·4
20·17	594·17	−6·4	+20·3	·978	+0·6	+0·2
Mar. 4·12	607·12	−5·7	+19·9	·985	+0·4	+0·7
25·99	628·99	−2·9	+16·3	·996	+0·4	0·0
Apr. 5·97	639·97	−1·0	+14·6	6·002	+0·4	+0·3
22·01	656·01	+1·4	+11·1	·010	−0·3	−0·1

TABLE IV (*concluded*)

Date (UT)	*MJD*	*Velocity* Prim. km s^{-1}	*Velocity* Sec. km s^{-1}	*Phase*	*(O − C)* Prim. km s^{-1}	*(O − C)* Sec. km s^{-1}
2000 Apr. 30·02	51664·02	+4·1	+9·3	6·015	+0·9	−0·4
May 30·95	694·95	+5·9		·031	—	—
2001 Feb. 14·14	51954·14	+14·2	−0·2	6·166	+0·8	+0·6
2002 Jan. 2·22	52276·22	+11·3	+1·0	6·334	+0·3	−0·7
Feb. 24·06	329·06	+10·6	+2·2	·361	+0·1	0·0
Mar. 28·07	361·07	+10·3	+1·5	·378	0·0	−0·9
Apr. 24·03	388·03	+9·0	+2·7	·392	−1·0	0·0
2003 Mar. 3·12	52701·12	+7·7	+5·1	6·555	+0·4	−0·3
2004 Apr. 5·05	53100·05	+1·8	+9·9	6·763	−0·8	−0·4
June 14·92	170·92	+1·3	+11·9	·800	0·0	+0·3
July 6·92	192·92	+0·8	+12·5	·811	−0·1	+0·5
2005 Jan. 13·22	53383·22	−4·7	+17·9	6·910	−0·4	+0·6
Apr. 21·99	481·99	−7·6	+21·0	·962	0·0	+0·4
May 28·97	518·97	−6·3	+19·7	·981	+0·4	−0·1
June 6·95	527·95	−6·2	+19·3	·986	−0·2	+0·3
22·94	543·94	−5·1	+15·9	·994	−1·1	−1·1
July 9·91	560·91	−0·7	+15·3	7·003	+0·4	+1·3
2006 Jan. 29·22	53764·22	+13·0	−0·9	7·109	−0·8	+0·2
Mar. 2·15	796·15	+13·8	−1·0	·126	0·0	+0·1
Apr. 4·08	829·08	+13·9	−0·5	·143	+0·2	+0·5
May 11·97	866·97	+13·1	−1·2	·162	−0·4	−0·4
June 24·96	910·96	+13·2	+0·1	·185	0·0	+0·6
2007 Feb. 3·21	54134·21	+11·8	+1·0	7·302	+0·4	−0·2

*Observed with original spectrometer.
†Observed with Palomar 200-inch telescope.
‡Observed with ESO *Coravel*.
§Observed with Cambridge *Coravel*.

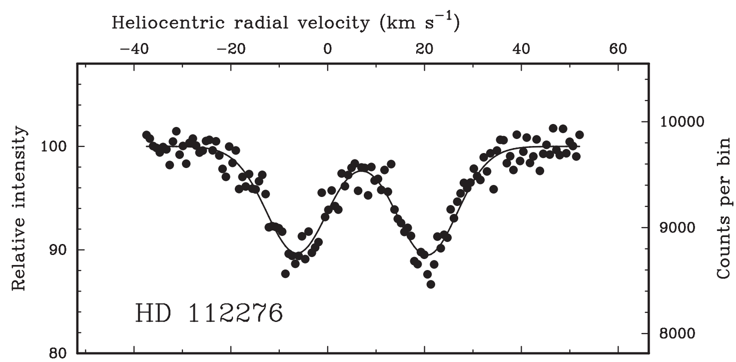

FIG. 5

Radial-velocity trace of HD 112276, obtained with the Cambridge *Coravel* on 2000 February 20 and showing the two dips at practically their maximum separation.

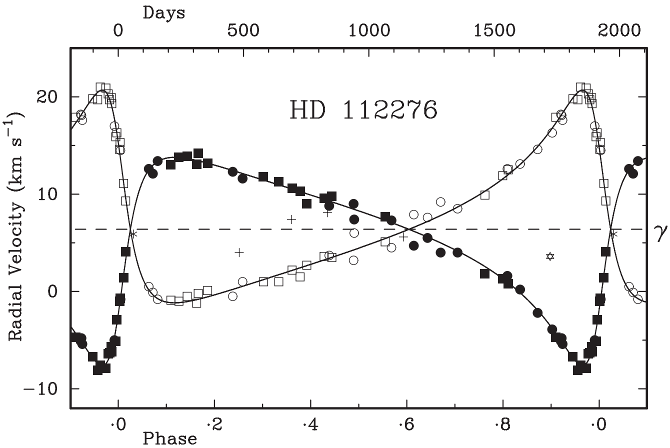

FIG. 6

As Figs. 2 and 4, but for HD 112276. The coding of sources is the same.

1974, and although several orbital cycles intervened between them they were fortuitously made at similar phases and agreed exactly with one another. It was only because the time interval between them, of little more than one year, was shorter than was usual on the NGP programme that the object was re-observed much later, in 1989, with the OHP *Coravel*, and proved then to give an entirely different velocity. Not until the twelfth observation, in 1992, was the secondary star discovered. Up till that time the weak secondary dip had either been so far from the primary as to be outside the scan range or else so close to it as to be masked. From that time onwards most traces have not only been double-lined but have duly been reduced as such (see Fig. 7). Altogether, 63 observations have

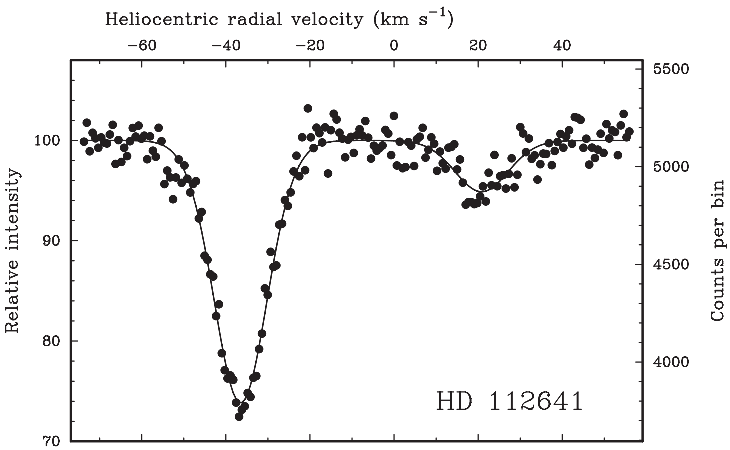

FIG. 7

Radial-velocity trace of HD 112641, obtained with the Cambridge *Coravel* on 2006 April 4 and showing the two dips very well separated.

TABLE V

Radial-velocity observations of HD 112641

Except as noted, the sources of the observations are as follows:
1989–1998 — Haute-Provence Coravel; 1999–2007 — Cambridge Coravel

Date (UT)	MJD	Velocity Prim. km s⁻¹	Velocity Sec. km s⁻¹	Phase	(O − C) Prim. km s⁻¹	(O − C) Sec. km s⁻¹
1973 Mar. 11·12**	41752·12	−23·6	—	0·721	−1·1	—
1974 May 21·99**	42188·99	−23·6	—	5·721	−1·1	—
1989 May 2·09	47648·09	+6·9	—	68·192	+0·1	—
3·01	649·01	+7·8	—	·203	+0·3	—
1990 Jan. 27·13	47918·13	+9·1	—	71·282	−0·1	—
Apr. 4·98*	985·98	−14·4		72·059	—	—
1991 Jan. 30·07	48286·07	−0·7	—	75·493	−0·1	—
Apr. 4·03*	350·03	+7·6	—	76·225	−0·8	—
May 9·97*	385·97	−13·1		·636	—	—
24·99*	400·99	−31·8	—	·808	+0·2	—
June 13·96*	420·96	−17·8		77·037	—	—
1992 Jan. 14·16	48635·16	−0·1	−21·6	79·488	+0·1	+2·8
18·18	639·18	−3·9	−20·8	·534	−0·1	−0·8
20·18	641·18	−5·7	−19·1	·557	+0·1	−1·5
Feb. 26·54†	678·54	−31·0	+12·7	·984	−0·4	−0·1
28·48†	680·48	−26·1	—	80·006	+0·1	—
Apr. 22·06	734·06	−11·6		·619	—	—
25·07	737·07	−15·1	−5·1	·654	+0·1	+1·0
29·07	741·07	−19·6	+0·1	·700	+0·5	+0·2
June 20·95	793·95	+8·8	−34·4	81·305	−0·2	+1·2
24·97	797·97	+7·5	−34·0	·351	−0·2	0·0
Aug. 12·87	846·87	−38·1	+20·7	·910	−0·2	−0·9
13·84	847·84	−37·8	—	·922	−0·2	—
15·83	849·83	−36·0	+17·7	·945	+0·1	−1·7
16·86	850·86	−34·9	+18·5	·956	0·0	+0·6
Dec. 18·23	974·23	+7·2	−33·2	83·368	+0·2	0·0
1993 Feb. 12·19	49030·19	−25·8	+6·4	84·008	0·0	−0·4
13·11	031·11	−23·4	+2·8	·019	0·0	−1·1
Mar. 18·13	064·13	+5·2	−31·0	·397	−0·4	+0·5
20·15	066·15	+4·1	−29·4	·420	−0·2	+0·5
Dec. 28·21	349·21	−15·6	−3·8	87·659	+0·1	+1·7
1994 Feb. 18·09	49401·09	+9·3	−34·9	88·253	+0·2	+0·9
1995 Jan. 4·22	49721·22	−37·7	+22·1	91·916	+0·1	+0·7
1996 Mar. 28·90	50170·90	−13·5	−7·9	97·062	−0·4	+0·7
Apr. 3·95	176·95	+0·5	−23·7	·131	+0·2	+1·3
1997 Mar. 31·07‡	50538·07	+8·9	−36·1	101·264	−0·3	−0·2
Apr. 30·99‡	568·99	−11·5		·618	—	—
1998 Apr. 28·99	50931·99	−28·1	+9·0	105·772	0·0	−0·7
1999 Apr. 14·41†	51282·41	−29·1	+10·9	109·782	+0·1	−0·1
July 13·23†	372·23	−32·1	+16·5	110·809	+0·1	+1·9
Dec. 29·25	541·25	−25·0	+5·9	112·744	0·0	0·0

TABLE V (*concluded*)

Date (UT)		*MJD*	*Velocity*		*Phase*	*(O − C)*	
			Prim.	*Sec.*		*Prim.*	*Sec.*
			km s⁻¹	*km s⁻¹*		*km s⁻¹*	*km s⁻¹*
2000 Jan.	9·21	51552·21	−36·8	+22·2	112·869	+0·2	+1·7
	10·27	553·27	−37·2	+20·9	·881	+0·3	−0·3
Apr.	10·03	644·03	−37·6	+21·8	113·920	0·0	+0·5
2001 Mar.	8·07	51976·07	−21·9	+4·9	117·719	+0·4	+2·3
	9·03	977·03	−23·6	+4·9	·730	−0·1	+0·8
	11·11	979·11	−26·7	+8·2	·754	−0·5	+0·8
	12·14	980·14	−28·1	+7·9	·766	−0·6	−1·1
	14·07	982·07	−30·1	+11·5	·788	−0·2	−0·4
2002 Mar.	1·12	52334·12	−33·0	+16·9	121·817	−0·1	+1·4
	2·14	335·14	−34·1	+19·4	·829	−0·1	+2·5
Apr.	7·05	371·05	+9·3	−34·6	122·239	+0·4	+0·9
	27·02	391·02	+1·2	−24·5	·468	0·0	+1·6
2003 Mar.	15·12	52713·12	+3·2	−28·4	126·154	−0·1	+0·3
June	12·99	802·99	+5·7	−30·9	127·182	−0·3	+1·1
2004 Feb.	9·21	53044·21	−36·2	+21·3	129·943	+0·1	+1·7
May	22·96	147·96	+0·3	−24·0	131·130	+0·2	+0·8
2005 May	11·00	53501·00	+4·9	−29·5	135·170	−0·1	+1·2
2006 Apr.	4·99	53829·99	−36·7	+20·3	138·935	+0·2	−0·1
	11·04	836·04	−26·6	+7·4	139·004	+0·1	−0·5
	12·07	837·07	−24·2	+4·9	·016	−0·2	+0·2
2007 Feb.	6·19	54137·19	+2·4	−26·8	142·450	0·0	+0·8
Apr.	5·06	195·06	−2·7	−21·5	143·113	0·0	−0·1

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

TABLE VI

Orbital elements for the four HD stars

Element	*HD 105443*	*HD 108576*	*HD 112276*	*HD 112641*
P (days)	1697 ± 4	1968·6 ± 1·9	1919·2 ± 1·1	87·3857 ± 0·0011
T (MJD)	52179 ± 14	52058·4 ± 1·4	51636·0 ± 1·7	50602·41 ± 0·11
γ (km s^{-1})	−23·93 ± 0·09	−6·59 ± 0·07	+6·39 ± 0·06	−11·10 ± 0·03
K_1 (km s^{-1})	6·58 ± 0·11	13·61 ± 0·11	10·69 ± 0·12	23·59 ± 0·05
K_2 (km s^{-1})	7·72 ± 0·29	15·30 ± 0·21	10·93 ± 0·12	28·79 ± 0·21
q	1·17 ± 0·05	1·125 ± 0·018	1·022 ± 0·016	1·220 ± 0·009
e	0·289 ± 0·018	0·661 ± 0·005	0·636 ± 0·006	0·2455 ± 0·0018
ω (degrees)	115 ± 4	298·0 ± 0·8	240·9 ± 1·0	236·0 ± 0·5
$a_1 \sin i$ (Gm)	147·1 ± 2·7	276·3 ± 2·8	217·8 ± 2·8	27·48 ± 0·06
$a_2 \sin i$ (Gm)	173 ± 7	311 ± 5	222·6 ± 2·9	33·54 ± 0·25
$f(m_1)$ ($M_\odot$)	0·0441 ± 0·0024	0·217 ± 0·007	0·112 ± 0·004	0·1086 ± 0·0007
$f(m_2)$ ($M_\odot$)	0·071 ± 0·008	0·309 ± 0·014	0·120 ± 0·005	0·197 ± 0·004
$m_1 \sin^3 i$ ($M_\odot$)	0·244 ± 0·022	1·10 ± 0·04	0·468 ± 0·016	0·653 ± 0·011
$m_2 \sin^3 i$ ($M_\odot$)	0·208 ± 0·012	0·981 ± 0·028	0·458 ± 0·015	0·535 ± 0·005
R.m.s. residual (wt. 1) (km s^{-1})	0·44	0·43	0·58	0·24

been made — 27 from OHP, 25 with the Cambridge *Coravel*, seven with the original spectrometer, and four from the DAO. One of the OHP traces is a blend, reduced as single and not contributing to the orbit, as are three of the 'old' Cambridge observations and one of the new ones, so there remain 58 valid measurements of the primary. Ten observations (including the eight un-blended ones made before the discovery of the secondary) do not allow the secondary to be measured, so the number of measures of the weak component is 48. The HD 112641 velocities are all listed in Table V, in which those from the Cambridge *Coravel* have been adjusted by $-0 \cdot 3$ km s^{-1}.

All measures of the primary have been given the same weight in the solution of the orbit, except the 'original Cambridge' ones, which merit a weight of only $^{1}/_{10}$. The secondary velocities have been weighted $^{1}/_{20}$. The Cambridge observations of the secondary exhibit a statistically significant positive offset of as much as $+0 \cdot 67 \pm 0 \cdot 19$ km s^{-1}, which is as worrying as it is inexplicable and is noticeable in the diagram (Fig. 8) of the orbit. The orbital elements of HD 112641, as well as of the three other stars whose measurements have been described above, are set out in Table VI.

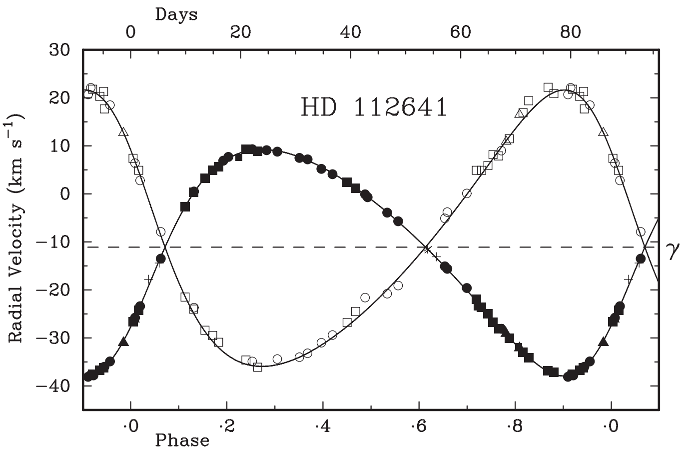

FIG. 8

Orbit diagram analogous to Figs. 2, 4, and 6, but this time of HD 112641. Small squares plot measurements of the primary with the original Cambridge spectrometer. The text comments on the tendency for the Cambridge observations of the secondary (the open squares) to show positive residuals. The effect is visible (though not conspicuous) near both nodes, and so cannot be resolved by the adoption of a different amplitude but only of a different γ-velocity. The γ-velocity is, however, for practical purposes fixed by the observations of the primary, which have much greater weight in the solution.

Discussion

The principal results of this paper have already appeared above, in the shape of the demonstration that the four systems are all double-lined binaries and the provision of tolerably accurate orbital elements for them. Such discussion as is possible is largely limited to attempts to divine the natures of the component stars, using as input data (*a*) the relative areas of the dips seen in radial-velocity traces, (*b*) the mass ratio, and (*c*) the colour indices and (the mainly photometrically derived) estimates of the spectral types.

It seems clear from the averaged type and absolute magnitude suggested by narrow-band photometry[1] that HD 105443 is a pair of main-sequence stars. Radial-velocity traces, *e.g.*, Fig. 1, show a mean ratio of dip areas (or depths, since the dips share the same, practically unbroadened, profile) of 1 to 0·3, corresponding to about $1^{m}\cdot 3$ in stellar-magnitude terms. To allow for the better match of the fainter, presumably later-type, component with the mask with which stellar spectra are cross-correlated in the spectrometers, the 1·3 may be multiplied by an empirical factor[25] of 1·15 to yield the best estimate of the V-magnitude difference, of approximately $1^{m}\cdot 5$. That would indicate a difference in type of a whole spectral class: types of F8 and G8 would match the integrated $(B-V)$ colour index of $0^{m}\cdot 57$. The mass ratio, however, is not in very good agreement. Andersen's graph[26] of $\log M$ against $(B-V)$ appears to show a rather constant gradient of about $-0\cdot 013$ per sub-type when the colour index is interpreted in terms of spectral type. On that basis, the mass ratio of HD 105443 corresponds to a difference of only $5\cdot 2 \pm 1\cdot 6$ sub-types between the components.

The weakest point in the observational basis for the discrepancy is no doubt the value of K_2, which would need to be raised from 7·7 to almost 9 km s^{-1} in order to bring the mass ratio into accord with the dip ratio. Such an error on K_2 does not look from Fig. 2 to be at all likely. The apparent mass ratio would be increased if the radial-velocity traces were reduced with a smaller depth imposed for the secondary dip, but then the concomitant increase in Δm implied by the revised dip ratio would exacerbate the discrepancy that it was intended to reduce. A tentative suggestion — but one which has no merit or support apart from its specific objective — is to suppose that the luminosity difference between the components is partly to be ascribed to the primary star's beginning to evolve. Types of about F9 IV–V and G4 V, with the primary star about three-quarters of a magnitude above the main sequence, would satisfy the mass and dip ratios simultaneously, and would match the observed colour index.

Whatever model of the system is adopted for HD 105443, the relationship between the minimum masses demanded by the orbit and the probable real masses of the stars shows that $\sin^3 i$ is a little above 0·2, so $\sin i$ is close to 0·6. The semi-axis of the relative orbit accordingly measures about 3·5 AU. The distance implied by the main-sequence model (interstellar absorption being assumed negligible in the NGP field) would be about 150 pc; the evolved-primary model would put it near 190 pc. In either case the separation could be expected to represent something like $0''\cdot 02$ in angular terms.

We can make use of the same relationships[25,26] as for HD 105443 to discuss the probable compositions of the other three *HD* systems. For HD 108576 the dip ratio is close to 1 to 0·45 and corresponds to a Δm of just $1^{m}\cdot 0$; types of F8 V and G4 V would match the colour index. The mass ratio expressed logarithmically is $0\cdot 055 \pm 0\cdot 007$ and should correspond to a difference of four or five sub-types. There is a slight discrepancy of the same sign as for HD 105443, but in this case it is so small that it does not seem worthy of further comment. The combined absolute magnitude of the (F8 + G4) model is $3^{m}\cdot 63$, agreeing well with the *Hipparcos*-derived value of $3^{m}\cdot 35 \pm 0^{m}\cdot 35$. The implied distance modulus is $5^{m}\cdot 4$, giving a distance of 120 pc. The minimum masses are practically the same as the masses to be expected for stars of the relevant types, so $\sin i \sim 1$. The semi-axis of the relative orbit is therefore scarcely more than the sum of $a_1 \sin i$ and $a_2 \sin i$, or about 4 AU; at 120 pc it would subtend $0''\cdot 033$, but in view of the high orbital eccentricity it is more useful to consider that near apastron (where the system in fact spends most of its time) the separation would reach $a(1 + e)$ or $0''\cdot 055$. The quoted angular distances that represent the linear separation of the components as viewed at the distance from here to the relevant system do not necessarily represent the actual angular separation that would be seen in a sufficiently powerful telescope. The mentioned separations would only be achieved if the orbit were seen in plan view on the sky ($i \sim 0$) or else the major axis were aligned in the 'plane of the sky' ($\omega \sim 0°$ or $180°$). In the case of HD 112276 we see the orbit close to edge-on, and its ω of 298° shows that its major axis is inclined only 28° to the line of sight, reducing the maximum angular separation that can be expected to little more than $0''\cdot04$.

HD 112276 consists of an almost equal pair of stars. Although it does not appear to be true in the case of the trace shown as Fig. 5, there is on average a slight difference in the depths of the two dips, such that one component is found to be stronger than the other in the ratio of approximately 1 to 0·9, indicating a ΔV of about $0^{m}\cdot1$ — less than one sub-type. The radial-velocity amplitudes tend to show a tiny difference in favour of the same component, a difference in $\log M$ of $0\cdot009 \pm 0\cdot007$, equivalent again to less than one sub-type. The colour index suggests a pair of stars very close to solar type, although the 'mk' type derived through *DDO* photometry indicates[1] G4 V. The absolute magnitude of the system must in any case be close to 4^{m}, yielding a distance modulus of 5^{m} and a distance of just 100 pc. The minimum masses required by the orbit are slightly less than half the true masses; taking the cube root of that ratio shows that $\sin i \sim 0\cdot83$, $i \sim 58°$. Then the semi-axis of the relative orbit comes out at about 3·5 AU, which would correspond to an angular separation of about $0''\cdot035$. As in the case of HD 108576, for much of the time the separation is larger than equates to the semi-major axis, by a factor up to $(1 + e)$, equivalent to a maximum of $0''\cdot057$. But also as in the case of HD 108576, projection effects foreshorten the separation appreciably from our standpoint, reducing the maximum value to about $0''\cdot045$: the values of i and ω show that both the plane of the orbit and the major axis within it are tipped only about 30° with respect to our line of sight.

In the case of HD 112641 we are faced straight away with a serious discrepancy between the Upgren[22] spectral type of G9 III and the *DDO*-photometric type[1] of K2 IV–V, with which is allied an absolute-magnitude estimate of $5^{m}\cdot8$. A further problem is provided by the *Hipparcos*-derived M_V of $3^{m}\cdot3 \pm 0^{m}\cdot4$, which represents about a tenth of the brightness of the Upgren estimate and ten times that of the *DDO* one. The $(B-V)$ colour index of $0^{m}\cdot97$ would be consonant with either classification, but it is quite easy to make a choice between them, and not only on the grounds that classification is subjective and photometry objective (although it is to be recalled that photometry does not classify spectra in the direct manner that the classical method does). It is unusual, although not unheard-of, to find a giant star in an orbit with a period as short as the 87 days of HD 112641, and especially in such an orbit having substantial eccentricity, although again it is not unexampled — the NGP star HD 118234 is actually a giant (or nearly so) with a shorter period and a higher eccentricity[27]. HD 118234, however, exhibits major photometric variations of the RS CVn type brought about by rotation and starspots; *Hipparcos* saw no such changes in HD 112641. Moreover, the principal dip in radial-velocity traces of HD 112641 shows no rotational broadening at all. Thus we are inclined to discount the classification of HD 112641 as a giant.

It is not so easy to adjudicate on the discrepancy between the *DDO* and *Hipparcos* luminosity estimates, since it is altogether outside the range of reasonably admissible uncertainty of either of them. We notice, however, that Soubiran *et al.*[23], using an (automated) assessment of an actual spectrum as opposed to an interpretation of basically photometric data, found for HD 112641 an absolute magnitude that they gave (to an ambitious precision) as $5^{m}\cdot398$; their text indicates an accuracy of the order of $0^{m}\cdot 4$. It could perhaps be argued that the existence of the secondary star, not accounted for in the derivation of the absolute magnitude, entirely falsifies the conclusions from both photometry and spectroscopy, but that seems likely to be over-stating an otherwise good case.

The mean ratio of dip areas in radial-velocity traces is 1 to 0·24, equivalent to $1^{m}\cdot 55$ expressed in terms of stellar magnitudes. Since both components are probably quite good matches for the *Coravel* mask, we could take that same number as representing the magnitude difference between the components in the wavelength region utilized by the *Coravel*, approximately the photometric *B* band. Without fixing on a particular model we cannot say what the magnitude difference should be in *V*. If we disregarded the parallax listed by *Hipparcos*, we could very well suggest that we were dealing with a main-sequence pair with types of around K0 and K5, which would satisfy both the magnitude difference and the mass ratio of 1·22 to 1, and would yield for the combination exactly the observed colour index and an absolute magnitude ($5^{m}\cdot 64$) in full agreement both with the Yoss–Griffin survey[1] and with the Soubiran *et al.*[23] spectroscopy. The implied distance modulus, however, would be only $3^{m}\cdot 3$, at which the parallax ought to be $0''\cdot 022$ — almost three times the *Hipparcos* value. The photocentric motion cannot be enough greatly to compromise the *Hipparcos* measurement. The only way of satisfying the listed parallax appears to be to assume that the primary is a star of the requisite colour and not fainter than about $M_V = 4^{m}$, and adopt for the secondary a main-sequence type about a magnitude and a half fainter, say about G8. But then we are faced with the questions as to how the primary came to be so over-luminous for its colour, and how the absolute magnitudes inferred both from narrow-band photometry[1] and from actual spectroscopy[23] could have been so misled by that combination. Moreover there has scarcely been time since the beginning thereof for a star to evolve into a subgiant of the type and magnitude that we need to postulate in such a model for the primary of HD 112641, and anyway a star *that* old would have no business to be a member of a low-velocity population and still less to mimic the positive [Fe/H] values found in both the references[1,23]. Thus the writer cannot offer any satisfactory adjudication on the issue: all that he can say is that there are stark discrepancies in the information about HD 112641 and that, whatever may ultimately prove to be truth about it, *some*thing(s) currently in the literature will have to give!

HR 4964 (HD 114357)

HR 4964 is reserved to this tailpiece because it can be accorded only the most preliminary of orbits, not only for the present but also for a long time to come (by the standards of terrestrial observing programmes, obviously, not astronomical time-scales!). In the course of Cambridge observations spanning 34 years, supplemented by published ones extending the time base to 73 years, a nodal/periastron passage in a very eccentric orbit has been witnessed, and it is very clear that another will not occur at all soon. All the same, the form (though not the period) of the orbit is clear, and since little further progress can be expected from the writer on the matter, the opportunity is taken to present the current situation.

The star is in the northern part of the NGP field, about 3° following and 1° south of Cor Caroli (α CVn); its brightness has been listed by Häggkvist & Oja[28] as $V = 6^{m}\cdot 02$, $(B-V) = 1^{m}\cdot 146$, and by Yoss & Griffin[1] as $V = 5^{m}\cdot 99$, $(B-V) = 1^{m}\cdot 17$ (thus the *B* magnitudes agree, but there is a small discrepancy in *V*). The *HD* type is K2; there are other pre-MK types of K2 from the DDO[29] and gK4 from Mount Wilson[30]. Zaitseva[31] gave an MK classification from an Abastumani objective-prism plate, K2 III: (the colon probably indicates that the spectrum of such a bright star was difficult to classify through over-exposure), and Appenzeller[32] found a type of K3 III — the type adopted in the *Bright Star Catalogue* — from a slit spectrogram obtained (very quickly, one would imagine!) at the McDonald 82-inch Cassegrain at 120 Å mm^{-1} at Hγ. The type and absolute magnitude implied by the *DDO*-style photometry in the NGP survey[1] are K2 III and $+1^{m}\cdot3$. The *Hipparcos* parallax of $0''\cdot01089 \pm 0''\cdot00076$ implies a distance modulus of $4^{m}\cdot82 \pm 0^{m}\cdot15$ and thereby an M_V of $+1^{m}\cdot20$, with the same uncertainty; the star has recently been considered a 'clump giant' by Mishenina *et al.*[33], who found its elemental abundances to be slightly enhanced with respect to those in the Sun. There is a paper[34] that refers to the existence at a web site of moderate-resolution spectra of a number of stars, but does not even say for what stars it might be worth looking there; it is understood that HR 4964 is one of them.

The radial velocity of HR 4964 was first measured at Mount Wilson in 1934; a mean value of $-17\cdot3$ km s^{-1} with a 'probable error' of $0\cdot7$ km s^{-1}, from four observations, was published by Wilson & Joy[30]. Abt[35] subsequently gave the individual dates and velocities, which have been copied to the head of Table VII here; the dispersions that he noted (36 and 38 Å mm^{-1}) identify the telescope used as being the 60-inch. Meanwhile, the star had been observed four times at the DDO 74-inch Cassegrain with a spectrograph giving 33 Å mm^{-1} at Hγ, and a mean velocity of $-18\cdot7$ km s^{-1} with a 'probable error' of only $0\cdot4$ km s^{-1} was published by Young[29] in 1945. Nothing is said about the times of the observations, but it is implicit that they were made after the publication of the previous programme[36] in 1942, and since there is every reason to believe that the star's velocity was practically constant at that epoch, the DDO mean velocity has been entered at a representative middle date in Table VII. Two OHP *Coravel* measurements have been given by de Medeiros & Mayor[37]. Famaey *et al.*[38] noted that the object is a binary and listed a mean of $-18\cdot0 \pm 0\cdot3$ km s^{-1}; they took their velocities from the *Coravel* data base in Geneva and were permitted to incorporate the present writer's measurements in their material, so it seems likely that the mean value that they gave is their interpretation of those measurements.

HR 4964 was observed at Cambridge three times in 1973/4 by G. A. Radford in the course of the NGP survey programme[1]; since the velocities that he obtained were in good agreement with one another and tolerable agreement with the then-published velocities[29,30], re-observation was by no means a high priority and the star was not measured again until 1988, when the writer observed it with the OHP *Coravel*. The result was still in good enough agreement with 1973/4, but the difference, not significant in itself, had moved it slightly further from the published values, just far enough to begin to arouse misgivings, so further observations were made in most seasons after that. By 1994 a change, though small, was definite, and in 1995 a suspicion arose that the traces exhibited an asymmetry suggestive of a weak secondary dip, which became quite clear in the following year; the node was reached in 1997/8 and the system thereafter closed together more quickly than it had opened. The amplitudes were not nearly enough to separate the two dips, whose strength differed by a factor of about 10. Some uncertainty remains as to the rotational broadening of the secondary dip, and that uncertainty feeds through into its area and its velocity too. Accordingly, although the velocity curve of the primary (which is so much stronger that it is little affected by any problems over the secondary) is quite reliable, the secondary velocities are badly scattered and the amplitude is subject to considerable doubt. The Cambridge *Coravel* was

TABLE VII

Radial-velocity observations of HR 4964

Except as noted, the sources of the observations are as follows:
1988–1998 — Haute-Provence Coravel; 1999–2007 — Cambridge Coravel

Date (UT)	*MJD*	*Velocity* Prim. km s⁻¹	*Velocity* Sec. km s⁻¹	*Phase*	*(O − C)* Prim. km s⁻¹	*(O − C)* Sec. km s⁻¹
1934 June 21·21*	27609·21	−16·0		0·525	—	—
1939 Apr. 10·28*	29363·28	−16·6		0·560	—	—
1942 Jan. 28·52*	30387·52	−20·5		0·580	—	—
1943 June 25†	30900	−18·7		0·591	—	—
1945 Apr. 21·32*	31566·32	−16·1		0·604	—	—
1973 Feb. 14·13‡	41727·13	−20·6		0·807	—	—
May 23·04‡	825·04	−21·3		·809	—	—
1974 May 9·07‡	42176·07	−21·4		0·816	—	—
1988 Feb. 9·19§	47200·19	−22·3		0·917	—	—
Mar. 15·16	235·16	−23·3	−12·3	·917	−0·1	+2·0
1989 Mar. 29·06	47614·06	−23·5	−12·5	0·925	+0·1	+1·4
Apr. 23·10§	639·10	−23·0		·925	—	—
1991 Feb. 5·16	48292·16	−24·3	−11·5	0·938	−0·1	+1·4
1992 Apr. 30·05	48742·05	−24·1	−9·4	0·947	+0·7	+2·7
1994 May 4·08	49476·08	−26·3	−11·0	0·962	−0·3	−0·5
1995 Jan. 3·21	49720·21	−26·7	−7·8	0·967	−0·3	+2·0
7·17	724·17	−26·5	−7·6	·967	−0·1	+2·2
June 1·97	869·97	−26·8	−11·3	·970	0·0	−1·9
Dec. 27·20	50078·20	−26·7	−5·7	·974	+0·5	+3·0
1996 Mar. 31·05	50173·05	−27·6	−8·2	0·976	−0·2	+0·2
Apr. 1·06	174·06	−27·7	−9·2	·976	−0·3	−0·8
25·04	198·04	−27·6	−9·5	·977	−0·1	−1·2
Dec. 15·26	432·26	−28·3	−8·5	·981	−0·3	−0·9
1997 Jan. 23·14	50471·14	−28·1	−8·4	0·982	0·0	−0·9
Mar. 6·17¶	513·17	−27·8	−5·9	·983	+0·4	+1·5
Apr. 10·08¶	548·08	−27·9	−6·1	·984	+0·4	+1·2
July 19·93	648·93	−28·3	—	·986	+0·1	—
Sept. 9·79	700·79	−28·7	−7·8	·987	−0·2	−0·9
Dec. 22·19	804·19	−28·7	−9·0	·989	−0·1	−2·2
1998 Apr. 29·01	50932·01	−28·7	−8·5	0·991	−0·1	−1·7
July 9·92	51003·92	−28·4	−9·3	·993	+0·1	−2·4
1999 Dec. 27·28	51539·28	−26·2	−11·8	1·003	−0·4	−1·1
2000 Feb. 11·18	51585·18	−25·5	−13·3	1·004	0·0	−2·1
Apr. 6·05	640·05	−25·4	−15·1	·005	−0·4	−3·3
June 11·92	706·92	−24·2	−8·9	·007	+0·3	+3·6
July 19·93	744·93	−24·0	—	·008	+0·2	—

TABLE VII (*concluded*)

Date (UT)	*MJD*	*Velocity* Prim. km s^{-1}	*Velocity* Sec. km s^{-1}	*Phase*	*(O − C)* Prim. km s^{-1}	*(O − C)* Sec. km s^{-1}
2001 Jan. 16·25	51925·25	−23·2	—	1·011	−0·3	—
Mar. 5·19	973·19	−22·5	—	·012	0·0	—
May 31·95	52060·95	−21·6	—	·014	+0·4	—
July 15·91	105·91	−21·4	—	·015	+0·3	—
2002 Jan. 1·27	52275·27	−21·5	—	1·018	−0·7	—
Feb. 24·16	329·16	−20·6	—	·019	0·0	—
Apr. 4·10	368·10	−20·3	—	·020	+0·1	—
May 7·99	401·99	−20·0	—	·021	+0·3	—
July 10·93	465·93	−19·8	—	·022	+0·2	—
Sept. 12·81	529·81	−19·7	—	·023	+0·1	—
2003 Feb. 18·15	52688·15	−19·6	—	1·026	−0·3	—
May 20·01	779·01	−18·9	—	·028	+0·2	—
Aug. 30·83	881·83	−19·0	—	·030	−0·1	—
2004 Apr. 23·06	53118·06	−18·4	—	1·035	0·0	—
June 21·91	177·91	−18·4	—	·036	−0·1	—
2005 Jan. 9·19	53379·19	−18·3	—	1·040	−0·2	—
June 21·92	542·92	−17·7	—	·044	+0·2	—
2006 Jan. 29·24	53764·24	−17·8	—	1·048	−0·1	—
June 30·91	916·91	−17·5	—	·051	+0·1	—
2007 Feb. 15·19	54146·19	−17·6	—	1·056	−0·1	—
June 26·92	277·92	−17·4	—	·058	0·0	—
July 26·90	307·90	−17·5	—	·059	−0·1	—

*Photographic observation from Mt. Wilson[30,35].
†Mean of photographic observations from the DDO[29].
‡Observed by G. A. Radford with the original spectrometer.
§Published OHP measure[37], reduced as single-lined.
¶Observed with Cambridge *Coravel*.

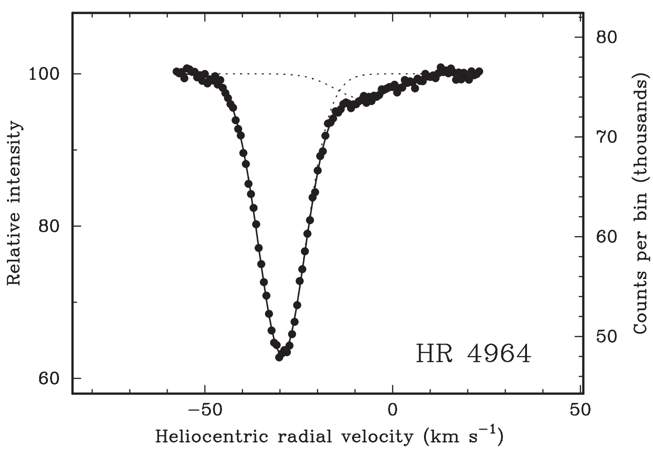

FIG. 9

Radial-velocity trace of HR 4964, obtained with the Cambridge *Coravel* on 1997 April 10, near the nodal passage during which the weak secondary dip was seen, albeit always heavily blended with the primary.

operational in a preliminary form for a time in early 1997, and two traces of HR 4964 were obtained with it then at a time when it was not far from the node. One of them is shown here as Fig. 9. They suggest a rotational velocity of about 10 km s^{-1} for the secondary dip. When the instrument came into routine operation at the end of 1999, HR 4964 was rapidly closing, and although the traces made during the first season showed evidence of the secondary and could just about be reduced with the profile for that component fixed in accordance with its appearance in the 1997 traces, the results were disappointingly ragged, and since then the secondary has been entirely masked by the primary. Efforts to reduce the closely blended post-2000 traces as double-lined have not given satisfactory results: when one is trying to disentangle a very weak feature from a close blend the result is too much at the mercy of almost infinitesimal differences in the observed profile. All the available velocities are set out in Table VII.

It is clear that the periastron passage that was witnessed in the 1990s was a rare event. The radial velocity of the primary star was evidently already declining towards it when it was observed in 1973/4, and probably also even in the 1930s and '40s although there must be hesitation over the possible errors of the early measurements. An orbit can be computed just from the Cambridge and OHP velocities of the primary alone (accepting measures of the unresolved blend as being for practical purposes measures of the primary star); the period is more than a century but is so uncertain as to be effectively indeterminate. It seems best to *impose* a period upon the solution. As far as the fit to the data is concerned, *any* period above about 25 000 days achieves a sum of squares of the residuals that is not unacceptably far above the minimum value. The values of T, γ, K_1, and ω remain almost constant against changes in the imposed period; the only substantial variation is in e, which naturally increases with increasing period. Periods at the 'short' end of the range of acceptable fits imply that the components' velocities ought to differ enough in recent years to imply considerable asymmetry, much more than is observed, in the radial-velocity traces. For that

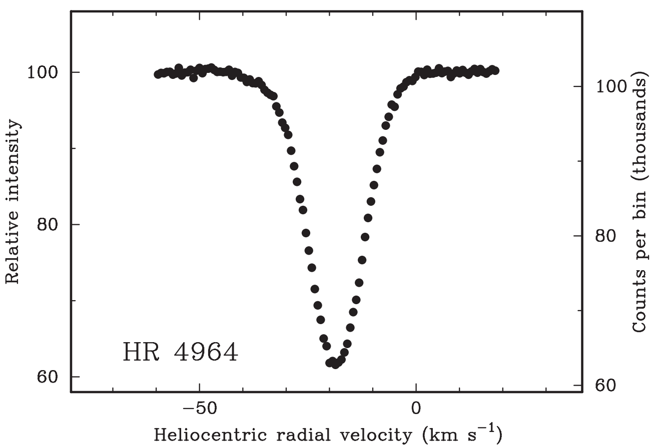

FIG. 10

Cambridge radial-velocity trace obtained on 2007 July 26, shortly before this paper went to press. The secondary dip creates only a minimal blunting of the negative-velocity shoulder of the principal dip; its velocity cannot be reliably determined, but clearly it cannot be far from the velocity of the primary.

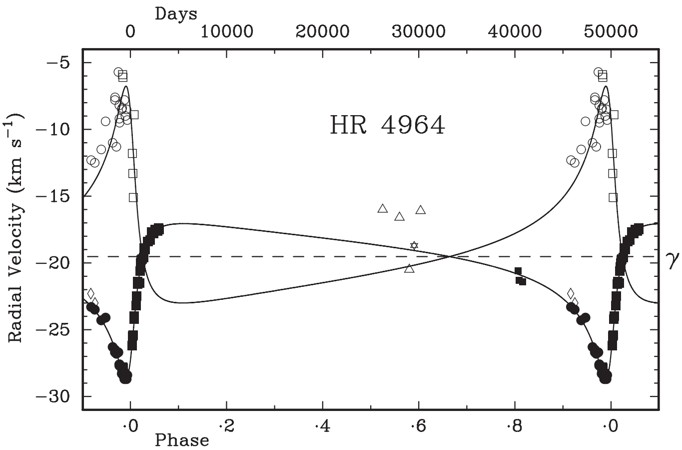

FIG. 11

Illustrative orbit diagram for HR 4964, based upon an imposed period of 50 000 days (about 137 years). Shorter periods would require the velocities of the components at the time of writing (when we are approaching the apastron-side node that occurs near phase ·1) to be more widely separated than traces such as that in Fig. 10 allow. Symbol coding remains as before, except that the open triangles represent the Mount Wilson observations[30,35] dating from 1934–45, the open star is a mean from the DDO[29], and the open diamonds are OHP velocities referred to by de Medeiros & Mayor[37]. None of those was reduced as double-lined, or could be used in the derivation of the orbital elements.

reason, a period of 50 000 days (about 137 years) has been selected for purposes of illustration. Anything shorter seems likely not to be consonant with the recent dip profiles, of which Fig. 10 is an example. Much longer periods would suit the data just as well, but their *a priori* likelihood decreases approximately as $1/P$, simply reflecting the (im)probability that a periastron passage would have been witnessed during the time that the system has been under observation.

The orbit that is computed with the period constrained at 50 000 days is illustrated in Fig. 11. The 29 Cambridge *Coravel* and 19 OHP measures of the primary have been given unit weight and the three early Cambridge measures $^1/_4$; the earlier published velocities have been zero-weighted. The velocity curve of the secondary is necessarily a mirror image of that of the primary, the sole extra degree of freedom being the secondary amplitude, K_2. The weight appropriate to the velocities of the secondary is $^1/_{50}$ of that of the primary, but for the two Cambridge observations taken near the node in 1997 that weight has arbitrarily been doubled. It is in any case so small that the only quantity that the secondary velocities affect to any appreciable degree is K_2 itself, for which of course they are wholly responsible. The elements of the adopted orbit are:

P	= 50000 days (fixed)	T	= MJD 51367 ± 28
γ	= −19·53 ± 0·16 km s^{-1}	$a_1 \sin i$	= 2486 ± 44 Gm
K_1	= 5·78 ± 0·08 km s^{-1}	$a_2 \sin i$	= 3500 ± 157 Gm
K_2	= 8·1 ± 0·4 km s^{-1}	$f(m_1)$	= 0·245 ± 0·013 $M_\odot$
q	= 1·41 ± 0·06 (= m_1/m_2)	$f(m_2)$	= 0·68 ± 0·09 $M_\odot$
e	= 0·780 ± 0·005	$m_1 \sin^3 i$	= 2·00 ± 0·22 $M_\odot$
ω	= 222·9 ± 1·5 degrees	$m_2 \sin^3 i$	= 1·42 ± 0·10 $M_\odot$

R.m.s. residual (unit weight) = 0·26 km s^{-1}

The reader is cautioned that any validity that the standard errors of the elements may possess is limited to the context of the fixed period; as indicated above, e in particular will change systematically with period.

Before discussing the HR 4964 system in the light of those elements, we ought really to recall that many of the radial-velocity measurements upon which they depend represent the unresolved blend of the two components, although for the purposes of the calculation they have been treated as velocities of the primary star. It is as well to consider what errors may be incurred through that approximation. At first sight they might appear negligible, since the secondary dip is so weak; it is about 1/10 of the strength of the primary. The effect of blending of a principal dip by a weak secondary has been discussed previously by the writer, both in this *Magazine*[39,40] and elsewhere[41,42], and its effects quantified and allowed for under the style and title 'dragging-function approach'; although the writer was not aware of it when he was first obliged to take an interest in the issue, exactly the same problem had been discussed earlier and at a higher level of sophistication (though with similar results) by Young[43] under the more prosaic appellation 'blending shifts'.

It is intuitively obvious (and was enunciated by Hartmann[44] a long time ago), that when the velocity difference between the components is small enough, the velocity given by the blend is simply the mean of its components weighted by their relative strengths. As the difference increases, the 'dragging' of the measured velocity away from the true value for the principal component falls below the linear relationship, reaches a maximum, and declines again to zero when the pair is practically resolved. The form of the 'dragging function' has been explored not only numerically[43] but also by reducing artificially constructed blends of known velocity separations in the same manner as real traces[39] and furthermore by plotting the residuals of many measurements that had unwittingly been made on spectra that were subsequently recognized to be blended[42]. In the case of interest the relative strengths of the constituents of the blend are approximately 10 to 1, so when the blend is a very close one its measured velocity is expected to be dragged 1/11 of the way towards the secondary, which in the light of the value of q found above is a little more than 1/5 of the way to the γ-velocity. Looked at in that light — that it is trying to reduce K_1 by some 20% — the effect seems much more significant than might have been expected. But we have to bear in mind that the 20% reduction in the departure of the primary's velocity from the γ-velocity occurs only when that departure is very small anyway, and the proportion decreases at larger separations. The maximum numerical value of the dragging appears to be only about 0·4 km s^{-1}. When an attempt was made to apply dragging corrections to all the velocities measured from blends, the new solution that was computed for the orbit was so similar in every respect to the one computed directly from the blends that the decision was easily made to stick to the latter, publishing and utilizing the velocities of the blends 'as observed' and thereby obviating any possible appearance of artifice.

The most likely source of the secondary dip in the radial-velocity traces is a main-sequence companion somewhat earlier than solar type. Judged from the strength of the dip, it would need to be nearly 2 magnitudes fainter than the giant in the blue where *Coravel* operates (a good 2 magnitudes in V), so it could be expected to have a type around mid-F. That would be more or less within expectation of the mass found above from the orbital elements, 1·42 ± 0·10 $M_\odot$, provided that the factor $\sin^3 i$ does not differ much from 1, so we may conclude from that that the orbital inclination is high. The computed mass and its standard error vary scarcely at all with the period that is imposed on the solution. The semi-major axis of the relative orbit, of course, does vary with the assigned period: at the adopted one of 50 000 days it is about 40 AU, and by way of examples it is about 30 AU at 30 000 days and 60 at 100 000. At the known distance of a little under 100 pc those separations correspond to $0''\cdot 4$, $0''\cdot 3$, and $0''\cdot 6$, respectively. Projected on the sky the separations would be somewhat less, because at the high eccentricity of the HR 4964 orbit the minor axis is about three-fifths of the major one, and we see from the longitude of periastron that the long axis of the orbit is inclined only about 40° to the line of sight. Although at the time of writing we are in one sense still near to periastron (phase ·06 if P = 50 000 days), the high eccentricity means that we already see the system in the apastron part of its orbit — the true anomaly (angular motion round the orbit since periastron passage) is about 120° on the basis of the 50 000-day period. Thus the angular separation on the sky can be expected to be of the order of tenths of a second of arc, and with a ΔV of something like 2^{m} or so the object ought not to be difficult to resolve by speckle or other types of optical interferometry, or directly by adaptive optics. It is perhaps a bit surprising that the duplicity was not detected by *Hipparcos*. In an orbit of the character discussed, there is, however, a conjunction that can take place far from periastron if the inclination is close to 90°, and it could have occurred at the *Hipparcos* epoch. If so, it is to current advantage, as in that case it could not also happen about now.

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CORRESPONDENCE

To the Editors of 'The Observatory'

Erwin Finlay-Freundlich

In your issue of 2007 October, there appears a review (**127**, 342) of the book *Parting the Cosmic Veil* by K. R. Lang. Your reviewer, anxious to correct a spelling mistake in the name "Freundlich" inadvertently commits two more-substantial errors. Freundlich actually arrived in St. Andrews before the outbreak of World War II in September 1939, not in the 1940s. He obtained British citizenship and did indeed legally adopt the surname "Finlay-Freundlich", thereafter publishing under that name, although in St. Andrews he was usually addressed and referred to simply as "Professor Freundlich". The "Finlay", however, was derived not from the maiden name of his wife, but from that of his mother, who was born Ellen Finlayson. It is an interesting coincidence that Zdeněk Kopal, who studied under Freundlich in Prague, was later in Manchester to be assisted for three decades by a capable assistant, born Ellen Finlay.

Yours faithfully,
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2007, St Andrew's Day

Quite a Three-Body Problem

I read with interest Virginia Trimble's review[1] of Dennis Danielson's biography of Georg Joachim Rheticus, *The First Copernican*[2]. Professor Trimble recalls that early in *A Study in Scarlet* Sherlock Holmes expresses ignorance of the Copernican theory, and she reflects on this incident to wonder how widely heliocentrism was accepted, even as late as the 19th Century.

A Study in Scarlet is not the only Holmes story to touch on matters astronomical[3]. Early in *The Greek Interpreter* Dr. Watson recalls how, after he and Holmes had taken tea on a summer evening, their conversation had roamed in a desultory, spasmodic fashion from 'golf clubs to the causes of the change of the obliquity of the ecliptic.' It is difficult to see how this latter topic could be discussed other than in a framework of heliocentrism and universal gravitation.

A Study in Scarlet was published in 1877 and *The Greek Interpreter* appeared in 1893[4]. It is possible that Holmes, or rather Conan Doyle, had a change of mind in the interim. More likely, though, is that a degree of artistic licence was being used. In *A Study in Scarlet* Holmes' indifference to matters astronomical is used to establish his single-minded attention to his work; in *The Greek Interpreter* the reader is being reminded of Holmes' credentials as a polymath prior to introducing his even more cerebral brother, Mycroft.

Of course we shall never know what may have been said in the lost tales; perhaps the case of Wilson, the notorious canary-trainer, or the singular affair of the aluminium crutch started with an astronomical digression before a hansom cab was called and the game was again afoot.

Yours faithfully,
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References

(1) V. Trimble, *The Observatory*, **127**, 340, 2007.
(2) D. Danielson, *The First Copernican: Georg Joachim Rheticus and the Rise of the The Copernican Revolution* (Walker and Company, New York), 2006.
(3) For both stories mentioned the edition consulted was: Sir Arthur Conan Doyle, *The Penguin Complete Sherlock Holmes* (Penguin, London), 1981.
(4) See, for example, URL: http://www.sherlockian.net/canon/index.html.

REVIEWS

Oxford Dictionary of Astronomy, edited by Ian Ridpath (Oxford University Press), 2007. Pp. 561, 19·5 × 13 cm. Price £11·99 (paperback; ISBN 0 199 21493 X).

Where do you turn when you need that elusive astronomical fact that you can't quite remember, or have come across some obscure term whose meaning escapes you? I suppose many of us now go to our computer and ask Google, but there are sometimes issues about the reliability of internet sources, and if the computer is not switched on (yes, sometimes mine isn't — and there are still people who don't use one regularly) it may be easier to reach for a reference book on a handy shelf. For many years I have had the 1985 edition of the *Macmillan Dictionary of Astronomy* on my shelf at home and the first (1997) edition of the *Oxford Dictionary of Astronomy* in my university office, and they have both served me well. However, neither is very up to date now, so it is a good time to look at a new offering and see how it compares.

This new edition of the *Oxford Dictionary of Astronomy* claims to be "the most expansive dictionary of its kind", so I did a couple of spot checks. At ~ 4200 entries it certainly beats the 4th (2000) edition of the Macmillan dictionary by about 500 entries, and is much larger than the more recent (2005) *Philip's A–Z Dictionary*, which has "more than 1000 entries", so it probably does have the largest range of entries — and amazingly manages to do so while being slightly thinner than its 1997 version, despite having 25 more pages. It has some 5% more entries than that edition, and there has also been considerable revision of some existing entries and tables; for example, the number of members of the Local Group of galaxies has increased from 31 to 36, and members are now usefully annotated by whether they are satellites of Andromeda or the Milky Way. Similarly, the tables of brightest and nearest stars have been updated using data from *Hipparcos*, and entries such as galaxy evolution, gamma-ray bursts, and SETI have been updated and expanded, sometimes very considerably. Many new satellites and telescopes appear, while some older satellites have disappeared (including, suprisingly, *Sputnik*, although *IUE* is still there and the *Cosmos* satellite entry still refers to *Sputniks*). There is a useful new table of abbreviations for variable-star types, although the entry for R (RR Lyrae star) is incorrect: it actually means a close binary with a strong reflection effect. As far as I can tell, it is up to date; certainly, it includes the latest definition of Pluto as a dwarf planet, and defines both Plutinos and Cubewanos. As in the previous edition, there is good cross-referencing between entries. For example, 'harvest Moon' refers the reader to 'retardation', which discusses the general phenomenon of the daily change in the rise time of the Moon (although sadly the misprint of 50′·4 for the average delay is uncorrected in the latest edition). One interesting innovation is the addition of weblinks for more detail on certain entries — is the editor foreseeing an electronic version in the future?

It is, of course, easy to pick holes in a reference work, and there are certainly a few holes in my particular speciality (*e.g.*, no separate entry for 'intermediate polar'), but I think the improvements and additions considerably outweigh the holes and typos, and I shall be glad to have this new edition on my shelf. — ROBERT CONNON SMITH.

The Sun Kings: The Unexpected Tragedy of Richard Carrington and the Tale of How Modern Astronomy Began, by S. Clark (Princeton University Press, Oxford), 2007. Pp. 211, 24 × 16·5 cm. Price £15·95/$24·95 (hardbound; ISBN 978 0 691 12660 9).

This is a fascinating and fast-paced narrative which is, in effect, a history of solar physics, beginning with Sir William Herschel's pioneering investigations into the nature of invisible solar radiation in the 1790s and concluding with modern particle-physics research at CERN and elsewhere after 2000. It is a story told with vivid effect, encompassing the new phenomena that took the contemporary scientific world by storm and the personal clashes within the community of astronomers and physicists, and culminating, as far as its dramatic narrative is concerned, with the tragic deaths of Richard Christopher Carrington and his beautiful young wife Rosa in the early 1870s. But more of Carrington anon.

Scientifically speaking, the story really begins at the start of 1859 September, when magnetic instruments went awry on a global scale, spectacular and sustained aurorae were seen across both hemispheres, and electric telegraphs were either shut down or made potentially dangerous to their operators by an unknown natural force. For on 1859 September 1, at 11·23 a.m. (GMT), Carrington, working from his excellently-equipped private observatory at Redhill, Surrey, noticed an intensely bright arched filament, 35 000 miles long, extending over a sunspot.

By 1859, of course, there was a long-established interest in the possible relationship between sunspots, aurorae, magnetic disturbances, and the Earth's weather and longer-term climate changes, but nothing could be pinned down precisely. What Stuart Clark does is to take us step by step through a whole sequence of questions and discoveries in and around the wider issue of solar periodicity and magnetism. Clark is a very clear and well-thought-out writer, and I very much like the way he discusses and interprets the events taking place in the science in each period, and places them within a wider intellectual and historical context. For in my view, we do not make sense of the past by forcing it into contrived sociological 'models', but by studying the interrelationships of historical circumstance and scientific development, and seeing them wrestled with either by individuals or by focussed groups of scientists. Stuart Clark also takes this pragmatic approach to historical evidence, and does so with great clarity and verve.

There are, however, one or two points on which I would wish to comment specifically. Tangential though Clark's treatment is of the 1846 discovery of Neptune, I do not believe that one can rightly explain it in terms of a 'bungling' John Challis at the Cambridge Observatory being so hidebound as not to notice Neptune when he saw it. The whole Neptune affair is far too complex for such a stark treatment. Likewise, it is incorrect to think of Edward Walter Maunder's being appointed to the Greenwich staff without possessing a university degree as unusual: indeed, *none* of Airy's staff, except the Cambridge-educated Chief Assistants, were university men, for Airy's Assistants were all taken on as young lads straight from school at around 16 years old, and were trained within the Observatory by an 'apprenticeship' system. I would also take issue with the statement, on p. 112, that until solar physics took off, academic astronomy was a matter of 'glancing through the telescope and speculating'. It had *never* been that. What it *had* been occupied with was the gruelling process of meticulous celestial cartography aimed not just at the perfection of nautical tables, but at the business of providing data which would form the foundation for analyses in gravitational mechanics.

And on the subject of the discovery and early study of solar prominences, I was surprised that Stuart Clark began with the 1851 eclipse (observed by Carrington) rather than with that witnessed by scores of astronomers across northern Italy and parts of Austria in 1842. For it had been at the 'Turin eclipse' of that year that Sir George Airy and many other astronomers made well-substantiated observations of the 'pink protuberances' which they saw surrounding the Sun at totality.

But it is really Carrington who is, quite dramatically, the hero of *The Sun Kings*. Carrington was a wealthy English 'Grand Amateur' astronomer: FRS, RAS Gold Medallist, celestial cartographer, and meticulous student of the Sun's surface, who more or less set himself the lifelong task of unravelling the relationship between sunspots, their causes, and their possible terrestrial effects. His epoch-making observation of the great flare of 1859 has already been mentioned. His Cambridge High Wranglership and his brilliance as an observer and a mathematical analyst had placed him at the very heart of British astronomy by his early 30s. Yet it appears that he was not always easy to get on with. He fell out with several leading Fellows of the RAS, and then sold off his first, Redhill, observatory in something resembling a fit of pique when circumstances required him to devote himself to running the family brewery. But when he eventually sold off the brewery to set up a new solar observatory at Churt, things went so badly wrong that one suspects Carrington was naïve in the extreme when it came to noticing imperfections in human nature as opposed to those on the Sun. For he fell in love with one Rosa Helen Rodway, an illiterate beauty who was living as common-law wife with an ex-Dragoon Guardsman who she convinced Carrington was her older brother. And it is at this point that solar physics turns into a soap-opera drama, with marriage, followed by barefaced cuckoldry, attempted murder, and two conjectured suicides.

As I said, *The Sun Kings* is a fascinating read, operating as it does on several levels. As a history of the key events in solar physics over the last 200 years, it is sound; but if you want to find out what happened to the astronomer, the beautiful temptress, and the passion-inflamed ex-Dragoon Guardsman with a large flick-knife, then I must refer you to Chapter 9. — ALLAN CHAPMAN.

Cape Landscapes: Sir John Herschel's Sketches 1834–1838, by Brian Warner (University of Cape Town Press), 2006. Pp. 179, 29 × 30 cm. Price R750/£75 (hardbound; ISBN 1 919 71375 1).

My *Concise Oxford Dictionary* defines 'dynasty' as a 'line of hereditary rulers', so I imagine that, in so far as astronomy can be said to be 'ruled' by the movers and shakers of the subject, the Herschels can be thought of as a British astronomical dynasty (albeit a rather short-lived one, compared with the Struves, for example). Certainly William was a hard act to follow, but John was a brilliant polymath who made major contributions not only to astronomy but to many other areas, including photography. But before that interest took off in a big way, one of his numerous talents was to be found in sketching, with landscapes as a particular forte.

We now have the opportunity to enjoy Sir John Herschel's skill in this beautiful volume of sketches made during his sojourn in and around Cape Town between 1834 and 1838, when he went to South Africa to extend his father's sky survey to the southern hemisphere. Brian Warner has long had a passionate interest in Herschel, with several books to his credit, but this one in particular is a visual delight. We are taken on a journey with Sir John and his wife Margaret and three children from England to Cape Town where they set up home (and an observatory) at Feldhausen; we then accompany them on a four-year exploration of the surrounding district; and finally, with some clear regret, we return with them to Kent (now with six children) and to something of a hero's welcome. There is, therefore, in the text sufficient here for the biographical enthusiast, although it should be noted that, aside from one sample page of astronomical drawings, this book is not about astronomy. Several additional asides from the author embellish the story *vis-á-vis* the life and times of a colonial pioneer.

However, the real meat of this coffee-table volume is to be found in the exquisitely reproduced sketches depicting the Cape scenery. I was amazed at the once luxuriant foliage depicted by Herschel in areas that are now intensely built up, and by the wealth of fine detail (especially in some of the drawings of Table Mountain, with which Sir John clearly had a fixation) in some of the sketches; others are rather less-finished outlines. But all are accurate, often constructed with the aid of a *camera lucida*, which Warner describes and illustrates.

I imagine that this impressive work will be of especial interest to folk living in the vicinity of Cape Town, revealing as it does how their neighbourhood has changed in the 170 years since Herschel's visit. It will also be a delight to historians, geographers, and probably botanists too, for its depiction of the flora of the locale. Readers from further afield would probably have benefitted from a modern map of the area with the places mentioned in the text pinpointed (the Cape does have a confusing geography for those who naïvely imagine it simply as the southern tip of Africa). And, given Sir John's rôle in the founding of the Royal Astronomical Society, it may be of interest to present-day Fellows, who are entitled to a discount (to £60; contact the Society's Librarian). — DAVID STICKLAND.

Quirky Sides of Scientists: True Tales of Ingenuity and Error from Physics and Astronomy, by D. R. Topper (Springer, Heidelberg), 2007. Pp. 210, 24 × 16 cm. Price £24·50/$39·95/€32·95 (hardbound; ISBN 978 0 387 71018 1).

As astrophysicists we immodestly take for granted the fact that science is the best and most powerful means of explaining and understanding the external world, and we pride ourselves that at the heart of science is the quest for truth and perfection. But there is one obvious snag with this view. The practitioners of this worthy endeavour are humans, and thus have all the typical human vicissitudes. We try hard to be rational, but often fail. Even when we advance it is usually by taking three steps forward and then two back. We have a tendency to fall in love with our hypotheses, and we give them up, if and when they are proven to be inadequate, only with considerable reluctance. And when we eventually get down to putting our ideas on paper, we try to maximise the impression of our cleverness and efficiency, and minimize the time-wasting effects of the blind alleyways we have only too happily rushed down on the way.

Our textbooks do not help. These present astronomical progresses as a steady, relentless, unidirectional unfolding of the truth; but science is not like that, and it never was. Textbooks distil the good, and the lasting, from the often mistaken ramblings of the original scientific writings.

David R. Topper is an historian of science at the University of Winnipeg in Canada. Fortunately for us his speciality is the behavioural quirks and oddities of old astronomers and astrophysicists, and his eminently readable book underlines the fact that when it comes to quirkiness astronomers were certainly not at the back of the queue when this characteristic was being handed out. Topper treats us to a series of pen portraits of the intellectual endeavours of Ptolemy, Copernicus, Galileo, Kepler, Newton, and Einstein. His general thesis is that their scientific performances might seem to be obvious and clear today, in retrospect, but it was far from clear at the time. The acceptance of a scientific truth (such as the inverse-square law of force, the ellipticity of planetary orbits, and the expansion of the Universe) is often a slow and tortuous process. History teaches us that many of the views that were strongly held in the past were clearly wrong. But it also underlines the fact that many of our all-too-human views today are probably incorrect as well, and this propensity for being wrong much of the time will most likely continue well into the future.

I got a great deal of pleasure from reading this fast-flowing, well-referenced, and refreshingly different book, and I was also introduced to many unfamiliar and thought-provoking aspects of the history of our subject's progress and the oddities of some of the major proponents of the past. — DAVID W. HUGHES.

The Sky is Your Laboratory, by R. K. Buchheim (Springer, Heidelberg), 2007. Pp. 328, 24 × 17 cm. Price £19·50/$34·95/€26·95 (paperback; ISBN 978 0 387 71822 4).

Many amateur astronomers have equipment powerful enough to make significant contributions to research. Their number has increased enormously in recent years with the advent of affordable yet sensitive CCDs and powerful home computers. Many others with more modest equipment are also keen to pursue research. The present book outlines eighteen worthwhile projects for the amateur. Some can be pursued on cloudy nights with no equipment except access to the World Wide Web. Others require little equipment except pencil and paper, *e.g.*, naked-eye studies of meteors: the observer requires a good site, ample free time, and the perseverance to carry on for months, not only during the major showers. Experience and skill will gradually improve over time. There is always the possibility of discovering new showers or catching an old one at an unexpectedly high rate. The search for comets is similar but requires even more perseverance.

Many amateurs are willing to travel long distances to reach the track of a grazing occultation by the Moon, or an occultation by a minor planet, while their professional colleagues are tied to their well-equipped but immobile observatories. For both types of occultation, the value of the observed profile is greatly enhanced when several observers combine their efforts, and there are many groups active in this way. The same observations may also reveal asteroids that have satellites. In fact, asteroids provide a rich field for the amateur. Tracing light curves over several nights can reveal the asteroid's shape; several are known to be triaxial. Photometric observations over one opposition can fix an asteroid's phase curve which, in turn, is symptomatic of its surface structure.

Although most amateur astronomers have a day job which limits their observing time, they have a distinct advantage over professional astronomers who must apply, months in advance, for time on a telescope. The time-assignment committee is only happy when it is told exactly what is going to be discovered beforehand. Thus searches for stochastic phenomena are where amateurs can contribute most. Supernova patrols have proved immensely successful and all forms of nova, whether they be classical, recurrent, or dwarf, are rewarding objects for study. However, every patrol must be carefully checked to remove false alarms, because even one false alarm will have the same effect as the thirteenth stroke of a clock.

Transits of exoplanets are at the cutting edge of research. Because the loss in light is so small, every effort must be made to maintain as high a signal-to-noise ratio as possible. There is a clear and detailed description of the principles behind the evaluation of signal-to-noise and warnings against the many insidious errors which can creep in. This field is limited to people with powerful equipment, but the scientific rewards are high.

The book ends with some valuable appendices. The first, on time, reveals some fascinating things which are not widely known. Some GPS receivers may give an error of several seconds because of delays in compensating for leap seconds or in refreshing the display. If one wishes to keep a computer running on UTC, it is best to fix its site in Casablanca rather than Greenwich. Casablanca has no daylight-saving time (B.S.T.) and so there is no danger of the clock advancing spontaneously in spring. Another valuable appendix stresses the need for good record keeping. The final one describes statistics of measurement and the nature of error. It makes the proper distinction between accuracy and precision but makes no mention of Poissonian noise which affects small-number statistics, *e.g.*, counts of sporadic meteors in one hour bins. Apart from this, the book contains few errors; a minor one is to confuse Tycho's supernova (1572) with Kepler's (1604). The book's outstanding quality is the infectious enthusiasm of the writing. It is highly recommended. — DEREK JONES.

Artificial Gravity, edited by G. Clément & A. Bukley (Springer, Heidelberg), 2007. Pp. 387, 24 × 16 cm. Price £77/$129/€99·95 (hardbound; ISBN 978 0 387 70712 9).

Exposure to microgravity produces a wide range of physiological and psychological disturbances in astronauts, and designing effective counter-measures for them will be important for planning future long-duration space missions. In the context of planetary exploration, such as is planned for both the Moon and Mars in the coming decades, it is essential that human crews arrive in a sufficiently fit and healthy state to pursue exploration-related activities on the surface — merely surviving the journey is not enough. One possible way to ensure this is to provide the astronauts with access to artificial gravity, either by spinning the entire space vehicle or by equipping it with centrifuges so that crew members may be given a daily 'dose' of artificial gravity to off-set the adverse effects of weightlessness. These topics form the subject of this book.

The book has grown out of the work of the ESA Topical Team on Artificial Gravity, which issued its Final Report in 2006. It begins with introductory chapters on the physiological consequences of microgravity, the physics of artificial gravity, and the history of the artificial-gravity concept. These early chapters are written by the two editors in collaboration with Dr. William Paloski from the NASA Johnson Space Center (who also contributed the Foreword). These are followed by more specialized chapters on the medical benefits (or otherwise) of artificial gravity for specific physiological systems (*e.g.*, the cardiovascular system, the vestibular system, the immune system, bone growth, *etc.*). The book concludes with a chapter on future research priorities.

As a non-specialist I had assumed that I would learn most from the introductory chapters, but I have to say that I found them to be rather disappointing. There are several reasons for this. Firstly, for what is supposed to be an up-to-date review, many of the references in these chapters are rather dated. Thus, on p. 14 we read that "due to limited in-flight data we do not know whether in-flight levels [of the immune system] stabilize…and whether recovery will occur after extended duration missions (Taylor 1993)." This would seem to be an important question and I was left wondering whether it can really be the case that no progress has been made on it in the last 14 years (and if not I would like to know why not?). Similarly, on p. 44 we find a 1981 reference for the statement that "Russian scientists suggest that the minimum level of effective artificial gravity stimulation in humans is about 0·3g" — surely more work must have been done on this in the last 25 years, and in fact the later chapters of the book make it clear that this is still an active area of research with no definitive answer in sight. The impression of being a bit dated is further reinforced by the statement on p. 66 that "Gerard K. O'Neill [originator of a concept of spinning orbital space colonies generally named after him] is another futurist thinker … he is an American physicist at the Institute for Advanced Study", when in fact O'Neill passed away in 1992!

My main disappointment with these early chapters, however, concerned the discussion of the physics of artificial gravity, which is rather confused and doesn't inspire confidence that the authors fully understand the physical principles on which their work is based. For example, on p. 37 we read that "centrifugal force results from the centripetal acceleration," which I suppose is true in a sense but conflates a fictitious (centrifugal) force observed within the rotating system with a centripetal acceleration observed from an external (inertial) frame of reference. Worse, the statement on p. 39 (and several others like it elsewhere) to the effect that "the centripetal acceleration is … always directed radially outwards from the centre of the rotating body" is just plain wrong. This confusion regarding the basic physics is extended to the discussion of the Coriolis force, the sense of which is shown incorrectly in Figure 2-06 despite being indicated correctly in Figure 2-08 only two pages later. There really isn't any excuse for getting these basic physical definitions and principles confused in a chapter entitled 'The physics of artificial gravity.'

A further worry concerns the short sections in the same chapter on possible methods of generating artificial gravity by means other than rotation — in particular, sections 1.2.3 and 1.2.4 on generating gravity by means of magnetic fields have no physical basis at all as far as I'm aware and should be deleted from any future edition. I suspect that part of the problem is that, as acknowledged on p. 34, some of these sections were compiled with the help of *Wikipedia*, which hardly seems appropriate for a technical volume such as this.

The later, more specialized, chapters are much better, and I learned quite a lot from them. However, even these would have benefitted from more careful editing. The main problem is the omission of references cited in the text from the reference lists at the end of each chapter. Several chapters suffer from this problem, but the worst offender is the otherwise very interesting chapter on the cardiovascular system by G. Antonutto *et al.* (Chapter 5). As far as I can see none of the references cited in Sections 5.1 (describing important recent bed-rest studies), 5.2, 6.1, or 6.2 make it into the reference list at all, greatly compromising the value of this chapter as a resource for researchers. These omissions really just amount to poor scholarship, and I feel that the editors could have done a better job in checking things like this prior to publication.

Despite these limitations, the book does provide a useful summary of artificial-gravity research. The extent to which microgravity affects different physiological systems differently, and the complex manner in which they all interact, was a real eye-opener to me. Clearly there is a vast amount of research that still needs to be done before we could confidently send human crews to Mars and expect them not only to arrive alive but to be able to operate effectively as explorers on the surface. Improving our knowledge in this regard will be a key aspect of research on the *ISS*, and later on the Moon, in the coming decades. Given how important this whole field will be in the development of future space-exploration strategies, I feel that the human-spaceflight community deserved a more carefully edited volume than the present edition. Perhaps the Editorial Board of the Space Technology Library could have a look into it and commission a second edition that addresses some of the deficiencies in the present volume. This would greatly increase its value as a resource for those engaged in the planning of future human space exploration. — Ian Crawford.

Exploring the Secrets of the Aurora, 2nd Edition, by S.-I. Akasofu (Springer, Heidelberg), 2007. Pp. 288, 23·5 × 15·5 cm. Price £36/$59·95/€46·95 (paperback; ISBN 978 0 387 45094 0).

Akasofu's book on the 'secrets of the aurora' is a somewhat unusual work, the like of which I am hard pressed to recall any direct parallel. Its subject, at one level, is that of solar-terrestrial relations, concerned with the dynamic plasma physical processes that occur within the solar atmosphere, the propagation of their effects through the interplanetary medium, and their influence on the Earth's coupled magnetosphere–ionosphere system, one manifestation of which is the Earth's dynamic aurora. However, this book is not a conventional text-book or research monograph on these topics. Rather, at a deeper level, the central topic of Akasofu's book is Akasofu himself. Not that this is a conventional scientific biography either, but rather consists mainly of an exposition and synthesis of the researches with which he has been associated for nearly fifty years, including sections on magnetospheric physics, especially substorms, storms, and current systems, solar and heliospheric physics, and 'space weather' research, interspersed with personal anecdotes and philosophical musing. It should be said, for those not familiar with the development of solar-terrestrial physics over this interval, that the significance of Akasofu's central personal contribution to this research field is not in doubt. In brief, in his seminal paper of 1964 describing analyses of all-sky-camera auroral images obtained during the International Geophysical Year (1957–58), and in subsequent works, Akasofu set up much of the phenomenological framework for the recognition and study of the auroral and magnetospheric substorm as a specific dynamical process. The discussion of these early researches, his collaboration with the 'retired' Sydney Chapman at the University of Alaska, and the controversies with Nobel laureate Hannes Alfvén, are of considerable historical interest. One hears again the distant thunder of scientific battles long past, here from a perspective other than the generally more vocal Scandinavian quarter. In more recent years, however, Akasofu has largely stood aside from mainstream developments in terrestrial magnetospheric physics, a fact he acknowledges with apparent satisfaction, stating on p. 25 that "It may be that I have an instinctive tendency to avoid a popular view." Thus, for example, one finds no coherent description in this book of reconnection-mediated, interplanetary-magnetic-field vector-modulated flux transport as a centrally important magnetospheric process leading to storms and substorms. Although important questions remain as topics of on-going research to be sure, the developing understanding of these phenomena over the past twenty-five years, in part in exquisite detail, has been a major success of the international research effort in this area. All rather unfortunate for the later Akasofu, then, if the 'popular views' he has instinctively rejected happen in the end to prove right. — Stan Cowley.

Cometography: A Catalog of Comets, Volume 3: 1900 – 1932, by G. W. Kronk (Cambridge University Press), 2007. Pp. 650, 26 × 18 cm. Price £150/$280 (hardbound; ISBN 978 0 521 58506 4).

Gary Kronk continues his encyclopaedic and thorough collection of the astronomical observations of all recorded comets with this third volume of his impressive and extremely useful series.

Volume 1 brought us from antiquity (October 675 BC) to AD 1798 in 563 pages. Volume 2, AD 1801 – AD 1899, took 837 pages to cover a mere century; not only had the number of recorded comets per decade increased considerably, but also the number of astronomical records per comet had leapt up as well. Both these characteristics have continued into the 20th Century. To say that Kronk leaves no stone unturned is an understatement. Take the 24 pages devoted to the most famous periodic comet 1P/ Halley (1909 R1, 1910 II, 1909c) at its last but one visit to the Sun. We are first treated to a detailed review of the return predictions made by A. J. Angström (1863), P. G. Le Doulcet (1864), A. C. D. Crommelin & P. H. Cowell (1907/8), and A. A. Ivanov (1909). The latter was nearly too late because M. F. J. C. Wolf recovered the comet photographically using the 72-cm Heidelberg reflector on 1909 September 12, some 3·42 AU from the Sun and just over seven months from perihelion passage. We are then informed about a veritable host of positional measurements, coma-size estimates, coma magnitudes, tail lengths, nucleus magnitudes, and jet details, until the comet faded from view on 1911 June 16 when it was beyond the orbit of Jupiter. Daylight observations using binoculars, the Earth's passage through the tail, and the comet's much-looked-for but unobserved passage across the solar disc are also discussed. Everything is carefully referenced.

As a cometary scientist I greatly appreciate Kronk's labour of love. I especially enjoyed his appendix of 'uncertain objects'. Comet discovery is not easy and it is amazing how many photographic plate blemishes, faintly recorded asteroids, and distant nebulae get mistaken for comets. This can be rather sad. Imagine that you are just about to bask in the glory of having a new comet named after you, only to be told that what you saw was (expletive deleted) NGC 2261!

Kronk's catalogues are truly impressive and extremely useful, and I would like to give him every encouragement to continue the good work. Only another 75 years and about 1500 pages to go to get up to date! — David W. Hughes.

Cassini at Saturn: Huygens Results, by D. M. Harland (Springer, Heidelberg), 2007. Pp. 435, 24 × 17 cm. Price £22/$39·95/€32·95 (paperback; ISBN 0 387 26129 X).

David Harland is either a very brave or a very foolhardy man. He has undertaken the task of chronicling the NASA/ESA/ASI *Cassini–Huygens* mission some two years after *Huygens* landed on Titan but only around half way through *Cassini*'s mission in the Saturnian system. The only certainty, as a result, is that the book will be out of date almost as soon as it is published. However, it's a risk worth taking. Data from *Cassini–Huygens* will be analysed and interpreted for years to come — the wait for the definitive results will be a long one, so David Harland has decided to jump right in — and it was a good decision!

His book is meticulously researched, as I would have expected from having read some of his earlier works on space-related subjects. However, I was surprised to find only three references in Chapter 1, a historical review of the Saturnian system from the invention of the telescope to the space age! This contrasts with over 100 references for each of Chapters 3, 4, and 5. And in Chapter 2, I was surprised that the observations of Titan by Comas-Solá and the atmospheric modelling by James Jeans were well described but not referenced at all. However, the more recent work described in the later chapters appears very well referenced. In addition, Chapter 5 encompasses nearly 50% of the entire text of the book. It would surely have benefitted from a further subdivision into other chapters.

This (pedantic) reviewer found the units used to be, at times, mixed! Within a few lines, we find the speed of the *Huygens* probe variously described as 6·2 km/s, 6150 metres per second, and 360 kilometres per hour! Spectrometer characteristics are described using, variously, microns, nanometres, and Angstroms.

Factually, I noted that the *Huygens* probe was described as "designed to float". This was never the case — it wasn't even 'designed to land'! And Guy Israel, one of the *Huygens* Principal Investigators, is from the French *Service d'Aeronomie* not *Service d'Astronomie*. Further, I couldn't find acknowledgements or references for the many excellent diagrams and images. However, in a book of this size, some factual errors are probably inevitable. This shouldn't detract from an excellent and thorough treatment which I am sure I'll use as a reference for a long time to come. It's a 'must' for planetary and space scientists and is even quite reasonably priced! — JOHN ZARNECKI.

Robotic Exploration of the Solar System: Part I, The Golden Age 1957–1982, by P. Ulivi & D. M. Harland (Springer, Heidelberg), 2007. Pp. 596, 24 × 17 cm. Price £24·50/$39·95/€32·95 (paperback; ISBN 978 0 387 49326 8).

With this year marking the 50th anniversary of *Sputnik*, the first spaceflight, one would envisage that a book entitled *Robotic Exploration of the Solar System* would address events that had happened over the period from 1957 up to the present day. In his preface, the senior author, Ulivi, describes how in the late summer of 1981, aged ten, he was inspired by the images of Saturn collected by *Voyager 2* and how it led to a 25-year fascination with robotic spacecraft. And yet this book is devoted to everything that went on prior to his conversion and involvement in the subject. He calls the period covered, 1957–1982 (the first 25 years), "the golden age" of robots. I think many would argue that a lot of the unmanned spacecraft that have flown since 1982 were just as much, if not more so, a part of an era that could be considered as pre-eminent. Still, this book is described only as Part I (it might have been called Part II since a detailed coverage of lunar exploration, manned and unmanned, by the same team has already appeared), so expect more to come.

Having been slightly critical, I have to say that this is a fantastic book — there is much in it that I, as a devotee of the subject for longer than either of the authors, can draw from it. It really is encyclopaedic in its coverage and is a wonderful work for old timers, who have forgotten the details of the US and USSR programmes, and for students, new to the field, to consult to get up to speed. It is copiously referenced to original source material, even if the style of citation is somewhat novel. It is absolutely marvellous value for the price of around £25.

If I have another small grumble, it is not with the authors, but with the publishers. I accept it would be financially prohibitive to produce 500-plus pages in full colour, but it would have been appropriate to include a section of colour plates, especially as amongst the illustrations are a considerable number of works of space art and artistic impressions, for which justice cannot be done in black and white.

Nevertheless, I thoroughly recommend the book and look forward to Part II (or Part III as I would call it), which I hope will not be too long in coming, given the trouble the authors have gone to in their efforts to provide such a large amount of detail. — Colin Pillinger.

Annual Review of Earth and Planetary Sciences, Vol. 35, 2007, edited by R. Jeanloz, A. L. Albee, K. C. Burke & K. H. Freeman (Annual Reviews, Palo Alto), 2007. Pp. 807, 24 × 19·5 cm. Price $205 (institutions, about £110), $85 (individual, about £45) (hardbound; ISBN 0 824 32035 5).

There is surely something to whet everyone's appetite in this year's volume. I recommend curling up with it in front of the fire when you feel the need to exploit your head-in-the-clouds scientist image for a brief escape.

Think of this as the 'black volume'. However, since we're told that children favour stories with dark endings, this doesn't mean it's not a fun read. The chapter on reaction dynamics by Casey & Rustad is guaranteed to shock and awe, suggesting as it does that geochemical data are better acquired by computing than by either experiment or field sampling. Does this mean that we have no further need for all those expensive mass spectrometers? Those who bemoan that some geologists now think that rocks are lists of numbers on a spreadsheet must now face their being relegated to simple equations! And if that doesn't appal you enough, check out Micklin's chapter on the horrifying ecological disaster of the Aral Sea, which pales the famous Mono Lake shocker almost into insignificance. And we haven't even got on to climate change proper yet.

Mann reviews climate change over the past two millennia and confirms that the warmth we are currently enjoying appears to be unprecedented in the context of the last 2000 years. Alley delivers more bad news in that apparently abrupt changes in North Atlantic circulation, sea ice, and climate do in fact occur. In case there is anyone left out there who still grasps for straws, Houghton adds that oceanic and terrestrial carbon sinks are becoming less effective. Skinner reviews insidious health hazards that may arise from geological factors such as atmospheric pollution, rock dust, asbestos, arsenic, and flourine, and Lynch & colleagues review the palaeorecord of climate and fire interactions in Australia. After all this, the message of Satake & Atwater, that future catastrophes can be anticipated by studying past earthquakes and tsunamis, sounds almost cheerful. On a further positive note Jahren's description of the exquisite deciduous forest that existed in the Canadian Arctic in the middle Eocene reassures us that global warming might not be so bad if you happen to be a plant.

My own pet subject — plume scepticism — is as usual fed by several chapters. Rowley & Garzione review the current state of play of stable isotope-based palaeoaltimetry, which makes interesting reading for anyone for whom the vertical motions of Earth's surface are important. An interesting synthesis of the Permo-Triassic accretion of east Asia is given by Ernst & colleagues, and at once it sprang to my mind whether the remarkable accretion events described could explain the Emeishan basalts. These occur further west and are generally explained using the mantle plume model, but they erupted during the collision period discussed in this chapter. Walters intrigues over the hemispheric dichotomy of Mars, which we still seem to be far from understanding.

But if you're simply not in the mood for either doom and gloom or beating up on plumes, don't worry. Check out Hughes's refreshingly elegant chapter on the evolution of trilobite body patterning. It's a charmer. — Gillian Foulger.

Massive Stars in Interacting Binaries (ASP Conference Series, Vol. 367), edited by N. St-Louis & A. F. J. Moffat (Astronomical Society of the Pacific, San Francisco), 2007. Pp. 748, 23·5 × 15·5 cm. Price $77 (about £38) (hardbound; ISBN 978 1 58381 235 8).

Tony Moffat and his colleagues at Montréal have a track record of organizing successful meetings that are distinctly *work* shops, with lots of discussion, at interesting Canadian venues. 'Massive Stars' was held at a lakeside hotel in the forests of Quebec, a location which, combined with a topic of interest, ensured a good attendance in 2004 August. Three years on, was it worth the wait for the unusually hefty proceedings volume? For me, the answer is 'yes', for two reasons. First, many of the reviews are solid, lasting contributions, covering topics such as binary- and single-star evolution, metallicity and environmental factors, and colliding winds. Secondly, a significant contributor to the overall heft is the extensive, essentially verbatim transcripts of discussions. Not only do these provide an entertaining record of the humour and occasional tetchiness of contributors, but also (and more importantly, of course) they document the areas of uncertainty and controversy far better than bland written contributions alone ever could, thereby highlighting exactly those topics of greatest interest. The proceedings are worth perusing if only to find some quotable quotes; I'll pass on just two to give a flavour: "Are you convinced that 0·4±0·1% is different from 0·2±0·1%?", and "I say let's discuss it — but I would be very, very skeptical discussing it".

With all the effort that evidently went in to recording the discussions, it's disappointing and inconvenient that no object index is provided (just an author index), but otherwise this is a very worthy record of what was evidently a lively, interesting meeting. — Ian D. Howarth.

The Astrophysics of Emission-Line Stars, by T. Kogure & K.-C. Leung (Springer, Heidelberg), 2007. Pp. 551, 24 × 16 cm. Price £115·50/$189/ €149·95 (hardbound; ISBN 978 0 387 34500 0).

Now that we have access to most of the electromagnetic spectrum, there are probably few stars that don't exhibit emission lines somewhere (although A-type main-sequence stars keep a fairly low profile!), so a book with the title of this one could be expected to give a fairly comprehensive review of stellar astronomy. In fact, the authors set out to give particular attention to those stars that display strong emission lines in the optical region but in practice this text, based on a book published in Japanese by the first author, does indeed cover a wide sweep of the H–R diagram and is certainly not devoid of references to other parts of the spectrum.

The first four chapters, comprising about a third of the book and essentially a course in spectroscopy and stellar atmospheres, could well form the basis of — or at least be supplementary reading for — an advanced undergraduate-level course. Following a short introduction to emission-line stars, the basics of radiation transfer in a 'stable' atmosphere are discussed comprehensively, so the formation of absorption lines is covered too. Then dynamic processes are considered, starting with convection but moving on to stellar winds, accretion phenomena, and shocks. These all form the platform upon which the formation of emission lines (including forbidden lines) take place, which is treated in Chapter 4.

With this theoretical background, the remaining three substantial chapters examine the astrophysics of the stars that show emission lines. Chapter 5 takes on the early-type stars, beginning with Wolf-Rayets and moving through the other hot denizens with strong wind activity (Of, Oe, Be, and LBV stars), with a presentation of their observational characteristics and their evolutionary status. Chapter 6 does the same for late-type stars, where chromospheric and coronal activity comes into play, but also includes long-period variables and a range of interacting binary systems (Algols, RS CVn, cataclysmic, and symbiotic stars). The only category of late-type object I might have expected to find and didn't was the cool hypergiants, such as (my favourite) HR 8752. Finally, in Chapter 7 we meet the pre-main-sequence stars whose activity is driven by their youth, particularly rapid rotation. Throughout we get interesting case histories and copious references from the literature. These chapters will be of value to the research community as well as to students.

The volume is rounded out with a supplementary reading list with entries right up to 2007 (but with a strange running header: *Astrology of Emission-Line Stars Supplement*!), and indices of authors, subjects, and objects. In places the spelling is somewhat unorthodox (Leynolds number for Reynolds number, for instance, on pp. 12 and 13), and the use of the definite article is in places haphazard, betraying the non-English origin of the work (and the failure of the Astrophysics and Space Science Library Editorial Board and the publishers to do anything about it). Nonetheless, this should prove to be a useful volume where stellar astrophysics is taught or practised. — DAVID STICKLAND.

Stars with the B[e] Phenomenon (ASP Conference Series, Vol. 355), edited by M. Kraus & A. S. Miroshnichenko (Astronomical Society of the Pacific, San Francisco), 2006. Pp. 385, 23·5 × 15·5 cm. Price $77 (about £38) (hardbound; ISBN 1 583 81223 7).

B[e] stars are characterized by IR excesses arising from dust emission, and forbidden emission lines in the optical. Unfortunately, this phenomenological classification doesn't correspond to a unique evolutionary stage, or set of physical properties; it just happens that stars with temperatures around 10 – 20kK are hot enough to warm any local dust, and ionize any local gas, while not too hot to blast away the cirumstellar environment. As a result, following the recognition of the B[e] phenomenon in the 1970s (consequent on advances in infrared photometry), a lot of effort went into studying individual objects, while collectively 'B[e] stars' remained a rather mixed bag. Only in the last decade or so has some sort of order has been brought to all this, with Lamers and others proposing a finer, and more physically founded, classification scheme of sgB[e] (supergiants), HAeB[e] (analogues of young Herbig AeBe stars), cPNB[e] (compact planetary nebulae), SymbB[e] (symbiotics), and unclB[e] (unclassified; the γ-ray source CI Cam may be the prototype of yet another class, X-ray binary B[e] stars).

This rather long list emphasizes the diversity of B[e] stars, and may suggest large numbers of examples; in fact, only 65 objects were listed in the seminal Lamers *et al.* review — and more than half of those as 'unclB[e]'. Further statistics: the workshop dedicated to these stars, held in the Netherlands in 2005 July, attracted 38 registered participants, and the printed proceedings contains 43 papers, plus five summaries of discussion sessions. Of course, this doesn't really mean that three stars are discussed for every two contributions, but the fact that $\sim$ 40% of papers feature individual stars in their titles does reinforce my impression that a lot of work is still going into understanding B[e] stars as unique individuals. Nonetheless, the organizers clearly put effort into organizing the workshop around more physical themes (reflected in the proceedings), the invited reviews are appropriately synoptic, and the written summaries of discussions are particularly useful records of half-formed (half-baked?) ideas. A nicely produced volume, though I'd guess of interest mainly to the converted. — IAN D. HOWARTH.

Understanding Variable Stars, by J. R. Percy (Cambridge University Press), 2007. Pp. 350, 25·5 × 18 cm. Price £30/$55 (hardbound; ISBN 0 521 23253 8).

This book describes itself as "a concise overview of variable stars", aimed at "anyone with some background knowledge of astronomy, but is especially suitable for undergraduate students and experienced amateur astronomers." It opens with a brief history of variable-star observations, and then spends two chapters introducing concepts such as magnitudes and the H–R diagram, as well as a brief overview of the life of a star. The level of this introduction is consistent with the book's stated target readership.

Following this, the author has subdivided the variable stars into various categories which, by his own admission, reflects the author's own research interests a little too heavily — the section on pulsating variables is more than twice as long as any other. The level of the text is somewhat variable, but whereas the introduction seems to imply that the book is aimed at people with very little experience of astronomy, the actual meat of the book is rather more involved and those new to astronomy may find themselves struggling at times.

The bulk of the book provides varying levels of detail on different types of variable star (rotating, eclipsing, pulsating, eruptive, *etc.*), concentrating mainly on the observational characteristics rather than the physical causes. The physical causes of the variations tend to be introduced *via* the observational characteristics, effectively taking the reader through what they see, to where it comes from. While this is a reasonable approach for a book aimed at the amateur astronomer, it can at times be frustrating — try following the index to the section on novae, and you will have to read six pages before finding out that a nova is caused by thermonuclear runaway.

In general, the book achieves its aim of providing an overview of variable-star astronomy, and given the size of the field this is no mean feat. It comes with an excellent index, and I suspect will be used primarily as a reference book (since I've forgotten what a Herbig Ae star is, I'll just look it up).

For the student just starting a PhD in variable stars, it gives a reasonable overview, but it will be chiefly of interest to amateurs who have been supplying data to the BAA or AAVSO for years, and want to know more about the stars they've been so faithfully observing. — PHIL EVANS.

Stellar Evolution at Low Metallicity: Mass Loss, Explosions, Cosmology (ASP Conference Series, Vol. 353), edited by H. J. G. L. M. Lamers, N. Langer, T. Nugis & K. Annuk (Astronomical Society of the Pacific, San Francisco), 2006. Pp. 430, 23·5 × 15·5 cm. Price $77 (about £38)(hardbound; ISBN 1 583 81221 0).

The "cosmology" in the title of this workshop was partly motivated by consideration of the rôle of primordial stars in re-ionizing the Universe. There's a strong nod in the direction of this issue, in the form of a full section (one of eight) on "cosmological consequences of low metallicity stars". However, the overall bias in this meeting and its proceedings is towards stellar astrophysics, and massive-star astrophysics in particular, from moderately to extremely low metallicity (SMC to first generation). Within that framework, there's perhaps rather less emphasis than I'd expected on supernovae and gamma-ray bursts, and quite a lot more on Wolf-Rayet stars. Overall, though, the proceedings provide a useful summary of observational and theoretical work on the effects of low metallicity on the intertwined topics of mass loss and evolution.

We know for sure that stars were born, lived, and died within a Gyr of the Big Bang, but obviously what we *don't* know is more interesting. For this reader, therefore, introductory reviews with this common subtitle, "what we don't know", by editors Lamers ('Mass loss from massive stars') and Langer ('Massive-star evolution at low metallicity'), stand out as informed and thought-provoking contributions. The venue is also of note: Tartu, Estonia, close to the old observatory where F. G. W. Struve worked. Who would've expected a Professor of Astrophysics there (and one with a very respectable publication record) to be Speaker of the national parliament? If only an astronomer held as influential a position in UK politics in these turbulent times. — Ian D. Howarth.

Solar and Stellar Physics through Eclipses (ASP Conference Series, Vol. 370), edited by O. Demircan, S. O. Selam & B. Albayrak (Astronomical Society of the Pacific, San Francisco), 2007. Pp. 380, 23·5 × 15·5 cm. Price $77 (about £38) (hardbound; ISBN 1 583 81238 5).

This volume of the ASP Conference Series deals with the proceedings of a workshop held at the Ankara University ORSEM campus at Side, Antalya, Turkey on 2006 March 27–29. The workshop took place immediately before a total eclipse of the Sun, providing a fitting finale for the meeting. This was a memorable total eclipse partly for the magnificent view of the corona but also for the fact that it was the second time totality had crossed the Turkish mainland within seven years. It is also interesting to note that what was being observed in the skies above Turkey was actually an occultation of the Sun by the Moon rather than a true eclipse.

The book is split into three sections, the first dealing with Solar System physics, the second with stellar physics, and the third with poster papers. The first section features invited reviews on helioseismology and plasma physics, eclipses and planetary transits, the internal structure of asteroids, and solar variations and climate on planets. It has to be said that the connection between some of these papers, both invited and contributed, and eclipses is somewhat tenuous to say the least. However, the invited paper on eclipses and planetary transits was a useful review.

The second section includes invited reviews on topics such as the power of eclipses — advances from ancient times to artificial intelligence, solar and stellar eclipse mapping, evidence on secular dynamical evolution of detached active-binary orbits, and contact-binary formation. This section does have a much stronger connection with eclipses and goes some way to make up for the shortcomings of the first section. The review papers were useful, as are some of the contributed papers. From a personal point of view, it was interesting to see a paper on upsilon Sagittarii, an object observed nightly by this reviewer and his colleagues for several months in South Africa, looking in vain for evidence of it being an eclipsing binary system. Sadly, our evidence for irregular pulsation was not referenced in the paper.

The poster papers in the third section cover a wide variety of topics including one on predicting the solar corona during the upcoming eclipse, one of two posters with a solar-eclipse connection. Again, some of the posters appear to have little to do with eclipses and one cannot help feeling that a significant fraction of this workshop might be described as 'padding'. At around £38, this book is probably a reasonable addition to a research library but I would question whether it should be purchased by individuals researching in eclipse-related topics. — Steve Bell.

The Road to Galaxy Formation, 2nd Edition, by W. C. Keel (Springer, Heidelberg), 2007. Pp. 284, 24·5 × 17·5 cm. Price £54/$99/€69·95 (hardbound; ISBN 978 3 540 72534 3).

Within living memory (meaning mine) galaxy formation was something that had happened long ago (unlike on-going star formation in Orion and all) and which would, therefore, be exceedingly difficult to learn anything about. The second half of this is still true. Indeed the first half is also, in the sense that virtually all the galaxies that can be studied in sufficient detail turn out to have at least a few very old stars in them. What has changed, and what Keel explores in this book, is that the assembly of galaxies as we now see them has occurred continuously over the past 12 or 13 Gyr and can be studied in at least two ways: by looking far back, at large redshifts, and by winkling out the oldest stars surviving in the Milky Way and other nearby galaxies.

The treatment is strongly oriented toward observations and their interpretation; you will learn more about NGC numbers than about N-body simulations, and more about de Vaucouleurs profiles than about the core/cusp problem, though the cover image shows the distribution of neutral hydrogen at $z = 4$ from a Princeton simulation.

Keel's style is conversational; indeed the book is delightfully written, and the annotations to the bibliographic items pithy and informative. But he is remarkably unobtrusive. We are not told just why the book was written (it does not give the impression of having been meant as a text for some specific course, though it probably could be). He is first author of only one of dozens of journal articles mentioned in the chapter-by-chapter bibliographies. It deals with the very low oxygen abundances found in small galaxies near larger ones that host active nuclei, and so is part of the answer to the key question of "when did galaxies form?", the answer being then, now, and in fact on into the future, but with the largest heavy-element abundances in the most massive objects at any given time, now often called down-sizing. And when it comes to capsule descriptions of other astronomers (Beatrice Tinsley and Fritz Zwicky, for instance) these are drawn from the writings of the person being described, rather than the author's recollections. Did you know that the only full biography of Zwicky was published in his home canton of Glarus? Well, now you do!

Some of the residual difficulties are spotlighted, for instance, the puzzle of why big elliptical galaxies don't seem to show a wide range of star ages associated with multiple mergers as they built up. And the final chapter deals with what we might hope to learn from the next generation of ever-larger telescopes (*JWST*, *ALMA*, the various possibilities for 30-metre-class ground-based telescopes, and, sadly, some missions like *Constellation-X* that no longer seem to be in the queue).

One thing I would have liked is more tables. There is a good one of globular-cluster populations, but it would have been nice to have one page with a list of masses, luminosities, characteristic core radii, composition gradients, and so forth, of either prototype galaxies or classes. Maybe in the third edition! — Virginia Trimble.

An Introduction to the Standard Model of Particle Physics, 2nd Edition, by W. N. Cottingham & D. A. Greenham (Cambridge University Press), 2007. Pp. 292, 24·5 × 17·5 cm. Price £30/$65 (hardbound; ISBN 0 521 85249 8).

The established author-duo Cottingham & Greenham are well-known for their widely used graduate textbooks. Their book called *An Introduction to Nuclear Physics* I know well and very much appreciate. However, I was not aware that they also published the textbook discussed here, even though it deals with my own field of research. I am glad that this oversight on my part has been corrected, as this volume is true to their style: it is a clearly written, concise book that serves as an outstanding introduction to the physics of the Standard Model. They do not shy away from the mathematical details, but embrace them and therefore manage to explain beautifully some detailed features of the Standard Model in this relatively short text. As a help to the student, the more advanced mathematical notions needed are explained in more detail in the appendices.

This second edition of this graduate textbook has been updated to cover recent developments in the field. However, not all chapters seem to have been thoroughly revised. Most notably, the experimental limit on the Higgs mass is still quoted as being 64 GeV/c^2, instead of the current value of 115 GeV/c^2. This is especially ironic as the front cover shows the event display of a Higgs candidate in the *ALEPH* detector, one of the four *LEP* experiments setting the current experimental limit on the Higgs mass.

The biggest discovery in the field of particle physics in recent years is that neutrinos have mass and that the different types of neutrinos mix. The second edition of this book has therefore rightfully been expanded with three chapters on the physics of neutrinos: 'Neutrino masses and mixing', 'Neutrino masses and mixing: experimental results', and 'Majorana neutrinos'. I find the addition of an explicit chapter on the experimental side of the field peculiar, as it is in contrast with the rest of the book. Arguably, it does give some insight into the details involved with this new phenomenon of neutrino oscillations. However, as somebody who is active in this field of research, I find the choice of experiments that are being focussed on quite arbitrary and not very logical. Moreover, as experimental neutrino physics is still a rapidly evolving field, this chapter makes the book very dated, if not already outdated.

Even though the update does not seem to have been done with the same care as was put in the writing of the original version of the book, overall it is still a very clearly written and recommendable introduction to the Standard Model of particle physics. — Simon Peeters.

Electroweak Theory, by E. A. Paschos (Cambridge University Press), 2007. Pp. 245, 25·5 × 18 cm. Price £40/$75 (hardbound; ISBN 0 521 86098 9).

One of the developers of the field of electroweak theory, Paschos, has produced a book which will be of value to students, researchers, and teachers of high-energy particle physics. It is stated that the reader is assumed to be familiar with the methods of quantum electrodynamics, which gives a precise description of the level of the book: it is an advanced text.

The author clearly decided to produce a relatively compact book, which has both advantages and disadvantages. The work is unavoidably mathematical and to work through the book will require diligence. However, the author has an enviable skill of describing complicated phenomena in clear and concise prose.

As a teacher of the subject myself, his discussion of field theories in Part II has given me a deeper insight and a better understanding which I can give to my students. On the mathematical side, my personal taste would be to have some more detailed derivations of particular processes, but as is often the case, these are placed in the problems at the ends of the chapters. Again, presumably due to brevity and also the author's occupation as a theoretician, the coverage of experimental results is not as complete as it could be. The experimental results quoted are also not always the most up-to-date (even considering the lead-time in publishing a book) and not always referenced.

With the expected turn-on of the *Large Hadron Collider* (*LHC*) in CERN within the next year, the subject of this book is clearly timely. All particle physicists are hoping that the results to come out of the *LHC* will revolutionize our view of fundamental interactions: this new understanding will hopefully be covered in such a lucid form by Paschos in future editions. — MATTHEW WING.

Analytical Methods in Radiative Transfer Theory, by D. I. Nagirner (Cambridge Scientific Publishers, Cambridge), 2006. Pp. 439, 21·5 × 14 cm. Price £45 (paperback; ISBN 1 904 86851 7).

"Your web page says you do radiative transfer!" was the ominous postscript to the letter I received from the Managing Editor inviting me to review Nagirner's book. As a numericist who prefers to use random numbers to perform radiation transport calculations, I have (to my eternal shame) generally shied away from solving the transfer equation analytically. I do own a copy of Chandrasekhar's *Radiative Transfer*, and have occasionally had reason to delve into it, but for the most part it sits on my shelf in much the same way as copies of *A Brief History of a Time* used to adorn the coffee tables of the chattering classes. However, as a dedicated reviewer, and after a gentle nudge or two from the aforementioned Editor, I did settle down one evening with the intention of reading the entire book and passing judgement on it. I failed in the former ambition, but this will not prevent me attempting the latter.

The book follows a logical sequence, starting with a description of the monochromatic intensity, and the formal solution to the transfer equation, and moves on to monochromatic scattering in semi-infinite and finite planar layers. There are extensive descriptions of analytical approaches, including approximate and heuristic methods for solving scattering transport problems. More complex topics, such as line transfer in a moving medium, are also addressed. This is, of course, a mature field, and the reference list contains few articles from post-1980, and does not include many papers from the 'mainstream' astronomical journals. This tends to contradict the claim, made by the author in the foreword, that the book focusses on the 'most important and latest studies'. I suspect that the most challenging contemporary problems in radiation transfer, such as calculating the emission-line spectra of core-collapse supernovae, or the spectral-energy distributions of ultracool brown dwarfs, require extensive use of the numerical approaches that are only described superficially in the final chapter.

There are obvious comparisons to be made between this work and that of Chandrasekhar — both are densely written and indeed they have a strong resemblance in typographic style. They also naturally cover similar material, although admittedly at differing levels; for example, Nagirner has more detail on line transfer while Chandrasekhar has a more thorough treatment of polarized radiation. I was able to use Nagirner's book during the course of some recent research, and found the relevant section (on time-dependent transfer) to be clear and useful, and I recommend it as a companion to, rather than a replacement of, Chandrasekhar's definitive book. — TIM J. HARRIES.

Astrophysics in a Nutshell, by D. Maoz (Princeton University Press, Oxford), 2007. Pp. 249, 26 × 18 cm. Price £32·50 (hardbound; ISBN 0 691 12584 8).

In the words of the publishers, the *In a Nutshell* series are intended as "concise, accessible, and up-to-date textbooks for advanced undergraduates and graduate students on key subjects in the physical sciences". The present volume is based on a course of about forty 45-minute lectures given at Tel-Aviv University, where the author is Professor of Physics and Astronomy.

The text begins with a short introduction on observational techniques, followed by three chapters on stars. After a survey of basic stellar observations, there is a comprehensive treatment of the physics of spherically symmetric, homogeneous, gaseous stars. The following chapter on stellar evolution discusses briefly the movement of stars to the right of the Hertzsprung–Russell main sequence, but concentrates on the ultimate 'stellar remnants' — white dwarfs, supernovae, neutron stars, and black holes. There are concluding sections on interacting binary systems and accretion discs.

The next chapter is on the interstellar medium, especially in star-forming regions. Some basic dynamical concepts — gravitational collapse, the Jeans mass and density, fragmentation — are introduced, but most attention is rightly devoted to the physics: photo-ionization by newly formed early-type stars, yielding H II zones with the Strömgren radius; collisional ionization, and heating and cooling processes in general; photo-dissociation of molecular into atomic hydrogen; 21-cm emission by atomic hydrogen; astronomical masers; and the omnipresent interstellar dust.

The topic under discussion now moves further up the mass scale to the Milky Way and other galaxies. Along with the classical components — the stellar disc plus the gas-and-dust disc within, the stellar spheroid, the extended spheroidal halo of gas, stars, and the globular clusters, and the magnetically trapped cosmic rays — the student learns from the beginning of a central supermassive black hole, and of a dark halo of unknown composition, extending far beyond the visible components. The numerous dark-matter candidates are listed, followed by the basic theory of the gravitational-lensing diagnostic, and a brief mention of the modified Newtonian dynamics (MoND) alternative that tries to dispose of the need for dark matter. The chapter then summarizes the properties of the observed classes of galaxy — spirals, with or without a bar, ellipticals, and irregulars — and of active galactic nuclei and of quasars.

The hierarchical spatial correlations of galaxies into groups and clusters, terminating at the 100 Mpc scale, sets the scene for the final three chapters, devoted to Big Bang cosmology. The presentation is generally familiar — Olbers' Paradox, extragalactic distances, Hubble's Law, the age of the Universe, isotropy, the Friedmann–Robertson–Walker metric, leading on to dark energy and the accelerating universe. The last chapter, 'Tests and probes of Big Bang cosmology', treats Hubble (luminosity distance *versus* redshift) diagrams, the cosmic microwave background and its anisotropies, nucleosynthesis of light elements, and quasars as cosmological probes.

The presentation of so much material within 250 pages is done very skilfully, with a judicious balance between mathematical discussion and physical argument.

The pedagogic value of the text is greatly enhanced by the problems given at the end of each chapter. Altogether, the book lives well up to the publisher's declared aims. I hope that a paperback edition will soon become available. — LEON MESTEL.

Annual Review of Astronomy and Astrophysics, Volume 45, 2007, edited by R. Blandford, J. Kormendy & E. van Dishoeck (Annual Reviews, Palo Alto), 2007. Pp. 701, 24 × 19·5 cm. Price $197 to institutions (about £98), $80 to individuals (about £40) (plus shipping) (hardbound; ISBN 978 0 8243 0945 9).

After 30 years as editor of this valuable series, now-retired Geoffrey Burbidge has provided the lead article in the latest volume with a fairly comprehensive autobiography stretching from his childhood in Chipping Norton through to a number of prestigious posts in the USA. While not a great literary work, I was interested to read of his interaction with astropolitics in Britain, especially when Margaret Burbidge became Director of the Royal Greenwich Observatory at Herstmonceux (erroneously labelled by Geoffrey first as Royal Observatory Greenwich and then as the Royal Observatory at Greenwich). Unfortunately, there were not too many juicy details that we didn't know already.

As for the remainder of the volume, there is heavy emphasis on star formation this time around. Indeed, a fascinating account of the beginnings of infrared astronomy (mainly) by Frank Low and an up-to-date review of modern IR detectors by Rieke, set the scene by what could be regarded as laying out the tools of the trade for star-formation studies. Bergin & Tafalla then describe the materials to form stars in the shape of a discussion of cold dark clouds. Towards the end of the volume McKee & Eve Ostriker lay down the theory of the formation of stars for all masses, while Zinnecker & Yorke concentrate on massive-star formation. The student of such matters might like to compare and contrast their results!

For me, perhaps the most interesting review was Crowther's masterly account of the properties of Wolf-Rayet stars, an area in which I dabbled some years back. I think it is fair to say that they are now a much better-understood class of object (*vis-à-vis* temperatures and masses) than they were in my day. Another class of object that has moved from novelty to a certain maturity over the last decade is the exoplanet, and Udry & Santos outline the statistical properties of the 200 or so that have been detected by various means thus far. Further down the mass hierarchy are the little satellites of planets in the Solar System, the origin of which (capture) is treated by Jewitt & Haghighipour.

The plethora of spacecraft studying the Sun in recent years (*e.g., SoHO, Ulysses*) has prompted an analysis of the coupling of the Sun and the heliosphere by Zurbuchen, although results from *STEREO* may add some refinements in the near future. Also given an airing in Volume 45 is a study by McNamara & Nulsen of hot gas in galaxy clusters heated by energy from AGN (although I've not heard such gas referred to as an 'atmosphere' before). If you are worried about the mass budget of the Universe, see Bregman's paper on the search for nearby missing baryons, and a surprisingly confident account of, essentially, the detection of black holes by X-ray lines originating in accretion discs is given by Miller.

As usual, there is something for everyone here, and you can be sure of the reliability of each article by the rather bizarre (and politically correct?) 'Disclosure Statement' appended to them (aside from Burbidge's!) which says that "The author is not aware of any biases that might be perceived as affecting the objectivity of this review." — DAVID STICKLAND.

Astrophysics: Decoding the Cosmos, by Judith A. Irwin (John Wiley and Sons, Chichester), 2007. Pp. 417, 24 × 17 cm. Price £34·95 (paperback; ISBN 978 0 470 01306 9).

This volume, as described by its author, was developed from lectures given at Queen's University in Kingston, Canada. It is intended to be most useful for mid-level undergraduates with a modest background in physics and mathematics, but perhaps little astronomical knowledge, and as a primer for graduate students entering astronomy from other fields. The book's perspective is that of an observational astronomer who wishes to understand the physical processes that affect his observations; indeed, its approach to the subject is that of decoding signals from space.

It begins with a discussion of the nature of light and the astronomical magnitude system, and about telescopes and detectors. The astronomical definitions of flux, intensity, *etc.*, are used, rather than the physical ones (irradiance, *etc.*). The effects of the Earth's atmosphere are discussed, along with our attempts to deal with them by adaptive optics and other techniques. The first section concludes with a brief account of signal processing and visualization, including bias and flat-field corrections for CCD images.

The next section contains much of the basic physics of gases and radiation, including such things as the Maxwell–Boltzmann distribution, the Boltzmann and Saha equations, and the physics of black-body radiation, including the concept of brightness temperature. There is a brief account of grey bodies and planetary equilibrium temperatures, and longer ones of interstellar dust and cosmic rays.

Next the discussion turns to the interactions of electromagnetic waves with matter. Several scattering processes are described, both elastic and inelastic, as well as absorption, particularly by photoionization, with particular reference to H II regions. There is a useful section on refraction, particularly in plasmas. The concepts of opacity and optical depth are introduced, with concise descriptions of their effect on stellar pulsation and stellar winds. Next comes a discussion of radiative transfer, LTE, and related matters. Finally there is a treatment of the interaction of light with space, including the expansion redshift and gravitational redshift and refraction, *i.e.*, lensing.

A major portion of the remainder of the book is concerned with emission processes, both continuum and line. Some considerable space is devoted to free–free emission, including the concept of emission measure. Synchrotron and cyclotron emission follow, with considerable detail on synchrotron spectra and sources. The inverse Compton and Sunyaev–Zeldovich effects are briefly described.

The final chapter is devoted to the modelling of complex signals and of their sources. This is done mainly by use of examples — the Galactic Centre, a globular cluster, and a star-forming region.

Several appendices form an important part of this book, particularly those concerned with the hydrogen atom (with the introduction of some quantum concepts), scattering processes, and the Hubble relation. Each chapter of the text concludes with a set of problems, of a range of difficulty.

The text shows a small, but entirely understandable, bias towards matters of concern to radio astronomy and the interstellar medium, the author's primary research area. It is largely free of typographical errors, although a few remain, in addition to the long list of errata provided to correct one particular error made by the publisher. Some colour prints are collected in the middle, with black-and-white copies near the section of text that is relevant. This would be useful, but in many cases the most important details are visible only on the colour prints and not on the copies. The book's chief defect, to this reviewer, is the use of cgs units, with their inability to serve electromagnetic quantities properly. In that respect, it might prove unattractive as an introduction to astronomy for students of physics, who are brought up on S.I., and this may limit its sales. That would be a pity, for it is otherwise a fine text, and deserves better. — COLIN SCARFE.

Black Holes from Stars to Galaxies — Across the Range of Masses (IAU Symposium No. 238), edited by V. Karas & G. Matt (Cambridge University Press), 2007. Pp. 483, 25·5 × 18 cm. Price £62/$110 (hardbound; ISBN 978 0 521 86347 6).

According to lists provided by the editors, IAU Symposium 238 (part of the Prague General Assembly) had 378 authors, but only 176 participants. Anyone whose name appeared on a poster or on a submitted manuscript qualified as an author; only first authors counted as participants; and merely to have sat quietly through most of the sessions taking notes, or even to have asked a question that appears in the proceedings, did not qualify you (or me, or Elisabete de Gouveia Dal Pino, a more expert example) as a participant. None of these 378 (or 175 or 3) folks appears seriously to doubt the existence of astrophysical black holes (that is, real objects with sizes not much larger than their Schwarzschild radii and strong gravitational fields), though several of the posters suggest alternatives to dark matter that might yield space-times different from General Relativistic expectations inward of $R = 2GM/c^2$.

X-ray binary black holes are here, and galactic centres from the Milky Way up to $10^9\ M_\odot$ or more, while the still-somewhat-hypothetical intermediate-mass black holes got their own session. Nothing is said about the even-more-hypothetical primordial mini black holes. Given the amount of opposition to black-hole models for active galaxies in the late 1960s (Christmas-tree models) and for X-ray binaries in the early 1970s (triple systems; anomalous secondaries), this has to be called progress.

The volume will make a nice souvenir for those who were there, since it includes photos of most of the speakers and a number of scenes in and around Prague. And, of course, it is another item on the list of publications of each of those 378 authors (one of whom used exactly half of his two-page poster write-up to list 33 of his previous publications). I am in general a considerable enthusiast for conference proceedings but am not quite sure that this one will be very useful. The editors have been exceedingly egalitarian, allotting at most 8–10 pages for invited reviews, 4–6 for contributed talks, and two for each poster. Thus there has not been space for the reviewers to explain how the present state of understanding came about, what the alternatives might have been, or even much of where the subject is going; and the contributors necessarily resort to referring questioners to their published papers and preprints.

Perhaps the time has come to return to the fine old concept of the *rapporteur*, who was given perhaps as much as an hour to explore a subject thoroughly, with the understanding that the time was to be used almost entirely to describe other folks' work, and that folks who wanted their work mentioned outside the poster session must send their recent results to the relevant review speaker.

On the other hand, a dozen or so odd left pages display images of some of Tycho's observational instruments, of which my favorite is '*alivd instrvmentvm* *simile priori, pro distantiis*'. It must in honesty be said that IAU Symposium 240, for whose proceedings I actually paid, has much the same mix of attractive scenery, rather plain speakers, and only marginally longer invited reviews, achieved by relegating all but the abstracts of the posters to a web site and allowing papers to start on even-numbered pages, within the same quota of about 480. Perhaps the time has come for the IAU to rethink how it structures both its symposia and their proceedings. — VIRGINIA TRIMBLE.

The Herschel Objects and How to Observe Them, by J. Mullaney (Springer, Heidelberg), 2007. Pp. 184, 23·5 × 18 cm. Price £19·50/$29·95/€24·95 (paperback; ISBN 978 0 387 68124 5).

James Mullaney has been writing about the Herschel objects for the last 20 years and by his own account was instrumental in getting the original Herschel clubs set up. Mullaney went through the list of 2500 objects listed as being discovered by William or Caroline Herschel and selected 615 objects that might prove to be interesting for amateurs to observe. The list was later reduced even further by the Astronomical League Herschel Club to 400, an act of which one gets the impression he does not altogether approve.

The book opens with a few short chapters on Herschel himself together with a brief introduction to observing techniques, and then the main part of the book is the seven chapters covering the showpiece objects from each of Herschel's class I, IV, V, VI, VII, and VIII along with some examples of the type of objects that can be found in classes II and III. Each object has a description that includes both Herschel's observation and the author's own description and occasionally some historical information. The one strange thing I found in these sections was that, for an observer who is upset that Herschel numbers are not included on modern star charts, he includes the questionable modern Caldwell numbers with the designations. Are not the NGC numbers definitive enough? The book is rounded out with some objects that the author regards as showpieces that were not discovered by Herschel. Any collection of these will of course be very subjective. In the last chapter the author discusses the class 7 objects from the RNGC. These are objects that the authors of the RNGC listed as not being found. I was surprised by this chapter as this issue has been covered in great detail by Brent Archinal both in a Webb Society monograph and in his definitive work with Steven Hynes on star clusters, neither of which the author appears to be aware. The book ends with an appendix comprising a target list of all 615 objects.

I found the book's reproductions to be a cut above the usual Springer ones and the book does offers something sufficiently different from both the earlier O'Meara *Herschel 400* volume and the Astronomical League guides to make it worth adding to your collection. — OWEN BRAZELL.

THESIS ABSTRACTS

Observational Constraints on Pre-Main-Sequence Evolution from Time-Series Analysis of Open Clusters

By Jonathan M. Irwin

Observational constraints on evolutionary models of low-mass stars on the pre-main sequence ($< 1 \cdot 0\ M_{\odot}$) are presently extremely scarce. Recent observational evidence has indicated substantial discrepancies between the predictions of these models and observations made in binary star systems, where the masses can be measured dynamically. It is clear that in order to resolve these issues, a larger sample of precise measurements will be needed to anchor the theory.

This work pursues two avenues to do this, using time-series photometric measurements in young open clusters (ages 1–200 Myr) obtained as part of the Monitor project, a large-scale survey using 2–4-m-class telescopes.

Stellar rotation periods are readily measured using photometry alone, and yet probe directly a fundamental stellar property: the angular velocity. These measurements place direct constraints on models of rotational evolution, and hence on the stellar models themselves. By examining rotation periods in six of the Monitor open clusters, I show that simple rotational evolution models can partially describe the data, with rapid rotators better described by a model assuming rotation as a solid body, and slower rotators by a model including differential rotation between the radiative core and convective envelope in stars with masses $> 0 \cdot 4\ M_{\odot}$, but more theoretical work is clearly needed to resolve discrepancies between the models and the data.

Eclipsing binary systems provide some of the most precise and accurate determinations of stellar masses and radii available, from combined analysis of radial-velocity and light curves. Searching for these systems is one of the primary science goals of Monitor. Dynamical solutions are presented for four of these systems, and two found to be on the pre-main sequence are compared to the predictions of the models, finding reasonable agreement in the mass–radius plane, but there may be significant discrepancies in the effective temperatures, as found by several other authors. — *University of Cambridge; accepted 2007 October.*

The full thesis is available electronically at: http://www.ast.cam.ac.uk/~jmi/thesis/

Effects of Solar-Particle Events on Geospace

By Martin John Birch

The Earth receives all of its external energy input (except for Galactic sources) from the Sun, which consists of both particles of various energies and electromagnetic radiation of various wavelengths. The particles emitted from the Sun constitute the *solar wind*, from which the interplanetary magnetic field originates. These particles transport magnetic, thermal, and kinetic energy from the Sun to the Earth. The solar wind has two modes: *slow* (typical speed 400 km s^{-1}), and *fast* (typical speed 800 km s^{-1}). Particles of various energies are released and accelerated into the heliosphere during (*i*) sporadic events involving both flares and coronal mass ejections, and (*ii*) recurrent events driven by co-rotating interaction regions associated with fast solar wind streams from trans-equatorial coronal holes. A small proportion of these particles, of which protons are by far the most significant for our purposes, deposit their magnetic and kinetic energy within geospace (defined herein as 'that which is within the magnetopause').

A survey of the solar energetic-particle (SEP) events and trans-equatorial coronal holes occurring during solar cycles 19 to 23 confirmed that the cumulative frequency of SEP occurrence and the cumulative monthly averages of sunspot number exhibit a strong correlation which improves with the inclusion of successive solar cycles. Furthermore, there is strong evidence that the emergence of low-latitude, open, coronal magnetic flux is also synchronized with the solar cycle. Four SEPs and one trans-equatorial coronal hole were then selected from cycle 23 for a detailed investigation of their effects on geospace. Specifically, the effects on the magnetosphere and the ionosphere are studied in relation to geomagnetic storms, cutoff-latitude depression, substorms, and polar-cap absorption.

The energetic proton populations of three selected SEP events from 2001 are used as 'tracers' to quantify the compression of the geomagnetic cavity during storm conditions. Empirical relations between the cutoff latitude and the ring current indices D_{st} and SYM-H are defined, and the compliance of the observations with the Tsyganenko T01 geomagnetic-field model is tested during storm conditions ranging from quiet to severe. The T01 model was found to be limited in its utility during severe storm conditions ($D_{st} < 100$ nT).

Another selected SEP event from 2003, the 'Halloween flare', resulted in a polar-cap absorption event for which empirical altitude-dependent relations have been derived between proton flux and D-region electron density. Values of the effective recombination coefficient have been determined for both day and night. It was found that, though chemical models predict that the production and recombination process should deviate from a square law, there is actually no evidence for this in the observations.

The D- and E-region effects of a co-rotating interaction region associated with the meridional crossing of a trans-equatorial coronal hole during late June of 2005 have been investigated during periods of substorm activity in relation to electron density, cosmic-radio-noise absorption, and energetic-electron fluxes. The hardening of the spectrum between the evening and morning periods is noted, and the movements of plasma during those periods are studied in detail, the motion being consistent with observations from previous related studies. The height and thickness of the absorption layer are estimated and the calculated total radio absorption is compared with measurement.

The same co-rotating interaction region provided the opportunity to investigate the spatial and temporal variations of the trapped and precipitated electrons in the auroral regions in terms of flux, spectra, and pitch angle. Differences of spectral hardness between the trapped and precipitated electrons, with the time of day, and with the intensity, are noted. It is observed that, for the population on the day-side, the precipitated/trapped flux ratio is highly variable and is energy dependent in a manner consistent with the theory of pitch-angle scattering. Another population, observed mainly on the night-side, shows near-isotropy at all energies observed. Diagrams of precipitated *versus* trapped flux ('pitch-o-grams') are found to be helpful in identifying various electron populations.

A method of determining the solar wind velocity from the delay in the N/S component of the IMF flux density is presented. The development of the event within the magnetosphere is described in detail, with particular reference to the results of the *EISCAT* radar observations. Comparisons are then drawn between a selection of fast solar wind streams and CMEs in relation to the L1 environment, the magnetospheric cutoff latitudes, and the energy deposited within the magnetosphere. Finally, the relative contributions of SEPs and fast solar wind streams to geomagnetic storms, cutoff-latitude depression, substorms, and polar-cap absorption are compared. — *University of Central Lancashire; accepted 2007 July.*

The Evolution of Galaxies in Massive Clusters

By John Philip Stott

We present a study of the evolution of galaxies in massive X-ray-selected clusters across half the age of the Universe. This encompasses galaxies on the red sequence from the brightest cluster galaxy (BCG) to the faint red population.

We begin at the tip of the red sequence with an investigation into the near-infrared evolution of BCGs since $z = 1$. By comparing the BCG Hubble diagram and near-infrared colour evolution to a set of stellar population and semi-analytic models we constrain the evolution and formation redshift of these massive galaxies.

Moving down in luminosity from the BCG, in Chapter 3 we study the build-up of the red sequence in massive clusters. To achieve this we compare the luminosity functions for red galaxies in a homogeneous sample of ten X-ray-luminous clusters at $z \sim 0{\cdot}5$ to a similarly selected X-ray cluster sample at $z \sim 0{\cdot}1$. We quantify this result by measuring the dwarf-to-giant ratio to ascertain whether faint galaxies have joined the red sequence over the last 5 Gyr.

In Chapter 4 we study the evolution of the red-sequence slope in massive clusters from $z = 1$ to present day. We compare our observed slope evolution to that predicted from semi-analytical models based on the Millennium simulation. We also look for trends between the red-sequence slope and other cluster observables, such as X-ray luminosity, to investigate whether this will affect cluster-detection methods which search for a colour–magnitude relation.

In the final science chapter we present the details of our own cluster-detection algorithm. This simple algorithm is based on finding clusters through the near-infrared and optical properties of the red sequence, drawing on our galaxy cluster-evolution research. We describe the application of the algorithm to object catalogues from the UKIDSS DXS fields in order to find clusters at $z \sim 1$. To confirm the presence of the clusters we employ deep multi-object spectroscopy on the photometric members. The clusters found in this study are fed back into the high-redshift régime of our galaxy-evolution research. — *University of Durham; accepted 2007 November.*

Here and There

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… space-born missions … — *ASP Conference Series*, **366**, 12, 2007.

NICE DATA

… measurements … provide a wealth of complimentary information … — *MNRAS*, **380**, 1488, 2007.

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

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

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