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EDITED BY

D. J. STICKLAND

R. W. ARGYLE

S. J. FOSSEY

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2008 March 14 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

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

The President. Welcome to the RAS Ordinary Meeting. Our first item is the presentation of the RAS Keith Runcorn Prize, sponsored by Wiley–Blackwell. The presentation to the winner, Sophie Bassett of Durham University, is going to be made by David Nicholson.

Mr. D. Nicholson. I would like to say a few words before making the presentation. A colleague of mine, who used to present this Prize, said that there are only three things that ever matter in anything that you may have to say. So today, my three things are: firstly, it is a particular honour to do this as it is the first time this Prize has been awarded under the sponsorship of Wiley–Blackwell, as opposed to Blackwell Publishing, following the merger of our organizations last year. Secondly, it illustrates the strength of our on-going relations with the RAS, and we are very proud to be the publisher of its journals, one of our longest and most established and successful publishing relations. And then finally, as a science publisher, what we do really comes down to the service we provide to individuals, to the authors and readers of the books and journals that we publish, so it is a great pleasure actually to meet one today and offer our congratulations and to wish Dr. Bassett every success in the future. Thank you. [Applause.]

The President. I should say that this Prize is awarded for Sophie's thesis, and as our first speaker, Sophie will be talking on the subject of her prize-winning thesis, 'Modelling sea-level observations to investigate the source and magnitude of major meltwater pulses during Termination 1.'

Dr. Sophie Bassett. First of all, I would like to express my thanks to Wiley–Blackwell and the RAS for the award of this Prize, and for the opportunity to speak at this meeting.

This talk represents a brief summary of work carried out as part of my PhD, completed in 2006 at Durham University, working with Glenn Milne in the Department of Earth Sciences. This work is concerned with utilizing available near- and far-field sea-level records to provide constraints on the major rapid sea-level-rise events that occurred during the most recent period of deglaciation (Termination 1).

Recent Earth history has been defined by the existence of large continental ice sheets which have grown and melted in a quasi-periodic fashion. Global ice volume reached its maximum around 21 000 years ago, when there was extensive glacial coverage of high-latitude continental regions, referred to as the Last Glacial Maximum (LGM). The causes of glacial cycles and the triggers for moving from glaciated to interglacial periods are governed by a number of factors. The Milankovitch theory, governing the variation in solar radiation received by the Earth, partly explains the long-period cycles of glaciation and deglaciation. There are, however, a number of more rapid transition events between glacial and interglacial events which are not explained by this theory: 'terminations', occurring over ~ 10 kyr, as well as events that occur in ~ 1 kyr or less, referred to as 'millennial-scale' events. The Earth's climate and the growth and melting of ice sheets are linked through a number of processes including, for example, surface albedo, topographic effects, meltwater discharge, and iceberg calving. Constraining the history of major ice reservoirs is an important step towards interpreting past climate records and thus is a useful tool for understanding the effect of glacial cycles on future climate change.

The surface ice–water mass redistribution associated with glaciation and deglaciation acts to perturb both the solid surface of the Earth and the gravity field, resulting in distinct spatial patterns in sea-level change. These 'sea-level fingerprints' can provide important constraints on both the space–time history of past ice sheets and the geophysical properties of the Earth's interior. Sea-level records in regions close to areas of previous glaciation (near-field sites) are more sensitive to variations in local ice history and associated isostatic response of the solid Earth. However, observations from far-field sites (regions distant from major glaciation centres) have a dominant signal arising from the influx of glacial meltwater.

The far-field data sets from Barbados and Sunda Shelf resolve a rapid rise in sea level of ~ 25 m over ~ 500 years occurring around 14·5 cal. kyr BP (calibrated kyr before present). This is termed meltwater pulse IA (mwp-IA) and represents a sea-level rise of greater than ten times that observed today. A further, smaller, more controversial meltwater pulse can also be observed in the Barbados record at ~ 11 cal. kyr BP. Although this is clearly visible in the Barbados record there is no evidence for this event in the other far-field sea-level records at this time (*e.g.*, Huon and Tahiti). The impact of rapid and large-magnitude events such as mwp-IA and mwp-IB would have major impacts on the climate system. For example, recent research suggested that a significant southern-hemisphere source for mwp-IA may explain the onset of the Bølling Allerød warm interval and the Younger Dryas cold interval.

Previous efforts to fit available far-field sea-level records from Barbados, Sunda Shelf, Bonaparte Gulf, Tahiti, and Huon Peninsula using global ice models and glacial-isostatic adjustment (GIA)-induced sea-level change have resulted in significant misfits at key sites particularly during the Late-glacial period (~ 14–9 cal. kyr BP). These misfits can be dramatically reduced by exploring both the dominant meltwater source and the Earth-viscosity structure.

Previously, the Laurentide ice sheet has been viewed as the most likely contributor to mwp-IA due to its large volume, and so this scenario is explored first. A range of models was explored, based on an ice history using a dominant North American source for mwp-IA and varying the lower-mantle viscosity structure within a range suggested by previous GIA studies. Each model was fit to the sea-level record at Barbados by modifying the ice-deglaciation model and the resulting fit at the remaining sites was examined with particular attention to the Late-glacial period. A lower-mantle viscosity at the higher end of the range considered (3–4 $\times 10^{22}$ Pa s) is seen to reduce dramatically the Late-glacial misfit (by $\sim$ 50%). A model with lower-mantle viscosity of 4 $\times 10^{22}$ Pa s also provides a good fit to the LGM data available from Bonaparte Gulf; however, this model overpredicts the LGM low-stand at Sunda Shelf. The origin of this misfit is unclear but may be a result of the oldest data at this location being biassed too old due to the bulk-sediment analysis used to obtain the data.

Exploring the viscosity structure reduces the Late-glacial misfit but there still remains a significant overprediction of sea-level rise that occurs given a dominant Laurentide source for mwp-IA. By exploring the source for mwp-IA and increasing the Antarctic contribution from 0 m to $\sim$ 15 m, a significant improvement in fit to all sea-level records is obtained. The existence of mwp-IB was also explored by comparing the best-fit model already obtained to models which include a pulse at 11·5 cal. kyr BP with different source contributions from the North American and Antarctic ice sheets. Although including mwp-IB improves the fit to the early Holocene at Barbados, this introduces large discrepancies at the remaining locations. This analysis does not, therefore, support the existence of mwp-IB. A more in-depth discussion of this work is presented by Bassett *et al.* in *Science* in 2005.

In order to test the scenario of a dominant Antarctic source for mwp-IA, the available Antarctic near-field sea-level records were examined. The data from this region sample only the last 12 kyr with relatively poor spatial coverage, and as such there are likely to be a number of ice-history models which would provide adequate fits to the data. The primary aim of this study was to test whether the available data could rule out a dominant Antarctic contribution to mwp-IA. A range of Earth and ice-history models were tested and the results indicate that a significant contribution from the Antarctic to mwp-IA cannot be ruled out. A more in-depth discussion of this work is presented by Bassett *et al.* in *Quaternary Science Reviews* in 2007.

To conclude, long-debated discrepancies between predictions and observations of Late-glacial sea levels at far-field locations can be resolved by adopting a relatively high lower-mantle viscosity and including a large ($\sim$ 15-m eustatic) contribution to mwp-IA from the Antarctic ice sheet, which cannot be ruled out by available Antarctic near-field data.

The President. Thank you very much for this elegant account of a very impressive piece of work. I think, as we perhaps stand on the edge of another meltwater pulse event, it is fascinating to see this. Do we have any questions?

Mr. M. F. Osmaston. You don't mention the possible intervention of tectonic events affecting sea-level; this is well documented in the whole sequence of seismic stratigraphy that goes right back into the Mesozoic. At times when there was no melt-water variation, for example, in the late Jurassic, there were many sea-level falls which were very rapid and tectonic in origin. So far as I can tell, these tectonic events are only drops of sea level, but nonetheless, the possibility that one of these has overlapped what you are studying seems to be something that you really should bear in mind.

Dr. Bassett. Yes, except that the sea-level rise can actually be seen at a variety of locations.

Mr. Osmaston. But this would be a global drop in sea-level.

Dr. Bassett. We have corrected for any tectonic information that we have got, but you're right, there could be something else going on.

Mr. Osmaston. I'm not disputing what you're saying, merely saying that this is a possible factor in changing the apparent rate of sea-level rise.

Dr. Bassett. It could be a factor, yes.

The President. Where else can the ice come from if not Antarctica? You're saying it could be Antarctica even though the far-field data suggests it is not?

Dr. Bassett. We really don't have a great deal of near-field data, and as the near-field modelling suggests, it could still be a possibility; we do look at the actual data that come from those locations and if it can be proved wrong from these data, then fair enough! But it is all based on having a decent model of the Antarctic ice-sheets and ice-melt history, and working with that, which is why we used Phillipe Huybrechts' model of the Antarctic to start with. Having said that, there are very varied models of the Antarctic and the Antarctic melt history, so there is more modelling research to be done; but there are also more data, we hope, that should be coming in.

Mr. M. Hepburn. You have given figures of sea-level changes of 120 metres, about 400 feet, for Barbados, a huge amount of water; and you also gave those without continuing data for the Sunda Islands. Does that mean that your mangrove remains at Sunda are now 400 feet below sea level, or is it just that you normalized it to the same figure as Barbados? You see what I mean — that the actual data that you quoted at Sunda extend over about 40 m and then there's a big gap with no data in it. Are those mangrove remains that far down?

Dr. Bassett. I'm afraid that is a data issue and I have limited knowledge of the data, enough that I know what the sea-level locations are.

Mr. Hepburn. But at least in Barbados, there's 400 feet of water that's gone up the side of the island, so the island used to be further out than it is now!

Dr. Bassett. Yes.

The President. Do you have a view on whether Greenland ice is about to melt?

Dr. Bassett. Well, I was looking at the Antarctic ice-sheets; I have little knowledge of the Greenland ice-sheets.

The President. What about the Antarctic ice-sheets, are they at all vulnerable?

Dr. Bassett. Yes, there is evidence to suggest they are melting, but there's also been some recent research to suggest that the East Antarctic ice-sheet has actually grown somewhat as well. It is very difficult to get any indication on that. It isn't my area, but there is follow-on work from what I've done and what's been done at Durham University looking specifically at the melt of the Greenland ice-sheets, and at what the sea-level rise at Greenland can indicate for climate change and ice-melt that would have happened without carbon pollution.

The President. Right, I think we'll stop there. Thank you very much indeed. I hope we have a few hundred years before any of this inundation comes again!

The President. Our next speaker is Professor Harold McAlister, Director of the Mount Wilson Institute, who is going to talk about 'A second century for Mount Wilson Observatory.' Of course, in the first century, Mt. Wilson made such big contributions.

Professor H. A. McAlister. For nearly half a century after its founding by George Ellery Hale in 1904, Mount Wilson Observatory was the premier astronomical facility in the world. With the opening of the 60-inch telescope in 1908, new standards were set in the design of telescopes as well as in the structures that housed them. Hale's crusade for ever larger telescopes that would culminate in his lifetime with the 100-inch and beyond his death with the 200-inch was launched from Mount Wilson. The revolutionary tools for daytime and night-time astronomy he built in the San Gabriel Mountains enabled new science that inevitably led to remarkable discoveries that today remain fundamental to our astrophysical and cosmological understanding of the Universe. Among those landmark discoveries are: Hale's own detection of the magnetic field of the Sun and the rôle it plays in the solar cycle; Shapley's displacement of the Sun from the centre of the Milky Way; Michelson's first successful measurement of the angular diameters of stars through interferometry; Hubble's conversion of M31 from Galactic nebula to external galaxy, followed by his redshift-*versus*-distance relation; and Baade's discovery of stellar populations that inspired our understanding of stellar evolution. There is, of course, so much more that could be added to this list, as has been so beautifully described by Allan Sandage in Volume I of the *Centennial History of the Carnegie Institution of Washington*, and published by CUP in 2004. The scientific heritage left us by those astronomers fortunate to have access to Hale's creation is unrivalled by any observatory yet built, and Hale would be thrilled by the continuing irrepressible urge to build ever larger telescopes to unlock more doors in the cosmic house whose front door was first opened from Mount Wilson.

The ever increasing light pollution from the Los Angeles basin, the development of modern instrumentation in Chile by Carnegie Observatories, and the finite size of astronomy budgets led to the closure of Mount Wilson in 1985. In 1987, the Carnegie Institution of Washington, which to this day owns the original facilities on the mountain, turned operation and control of Mount Wilson over to the Mount Wilson Institute (MWI), a non-profit corporation specifically created by Pasadena attorney C. Robert Ferguson and Arthur Vaughan, a former Carnegie astronomer then working at the Jet Propulsion Laboratory, with the goal of keeping Mount Wilson alive as a functioning scientific institution. Vaughan served as MWI Director from its founding until Robert Jastrow was recruited for that position in 1992. Jastrow set about reinvigorating the 100-inch telescope through the development of a modern adaptive-optics system that for a time had the best performance of any such system in the world. There was an 'if you build it, they will come' optimism that this new capability would attract new users who would bring new financial resources to allow the Observatory to flourish. Regrettably, the realities of how astronomy is funded in the US never permitted the anticipated subscription level to develop, but adequate income was derived from site fees paid by the principal investigators heading up other projects on the mountain, some of which used the Carnegie instruments while others developed new facilities of their own. These projects today include: solar work carried out at the 60-ft and 150-ft solar towers by Edward Rhodes (University of Southern California) and Roger Ulrich (University of California, Los Angeles); infrared interferometry at the *Infrared Spatial Interferometer* developed by Charles Townes (University of California, Berkeley); and Georgia State University's *CHARA Array* long-baseline optical/infrared interferometer, led by me.

When Robert Jastrow retired as director in 2003 [Editor's note: Professor Jastrow died on 2008 February 8], I took on the directorship of MWI. It soon became clear that the goal of keeping Mount Wilson on a research focus is no longer realistic, and, if the Observatory is to survive, a new business plan must be developed. Thus, as of this writing, MWI is planning to launch a capital-fund-raising campaign whose purpose is to provide the means to continue to support science from Mount Wilson while refocussing the primary enterprise to historic preservation and public outreach. The specific goals of this 'Second Century Campaign' are fourfold: restore and preserve the historic facilities; ensure that the site remains suitable for on-going scientific research; build a new Visitor Centre and other on-site facilities to foster public understanding of Mount Wilson's heritage and influence on contemporary astronomy; and create on-site and internet-based outreach programmes that attract the interest of amateur astronomers and help inspire a re-awakening of scientific interest among young people.

Mount Wilson Observatory is truly a world-class science-heritage site. It is immediately adjacent to an enormous population base above which the mountain looms as an icon. There is no better-placed or more deserving institution to be developed for public appreciation and public support than this hallowed ground where Hale forever changed our view of the Universe and of our place in it.

Mr. M. Hepburn. Couldn't you interest Bill Gates in it?

Professor McAlister. Well, that's another advantage of southern California: there are a lot of wealthy people there. My wife Susan operates the 60-inch telescope programme; it is the largest telescope in the world that is routinely made available for public observing on Friday and Saturday nights. If you contact Susan you can reserve a night for a fee, but people don't choke on the fee and occasionally we've had movie stars show up unexpectedly. Such visits could lead to miracle gifts, or maybe not. But we're not counting on miracles, so we're going to visit foundations and various corporations that should have an interest in Mount Wilson. George Ellery Hale was responsible for many things: he created the American Astronomical Society, the National Research Council in the US, CalTech basically was his brainchild, the Huntington Library in Los Angeles — he talked Mr. Huntington into giving his fortune to fund this wonderful library and he was a very important figure in southern California. You can make a strong argument that the entire aerospace industry and higher education in southern California traces back to Mt. Wilson, so we think it is a very powerful attractant to the wealthy and influential people of southern California.

Dr. S. Mitton. You might find it interesting to research what's happened at the Observatoire de Nice, if you have not been there, because the astronomers did walk away and the brambles were allowed to take over. Then Jean-Claude Pecker decided that this was a bad thing and initially he used volunteers to clear the site. But now they have reached the point where they are developing it into a serious tourist attraction — it has a big telescope dome constructed by Eiffel and they have started to do outreach programmes.

Professor McAlister. Thank you very much, I'll certainly investigate that programme!

A Fellow. The movie you tried to show us — is it on the internet?

Professor McAlister. Not yet, but we'll put a low-resolution version on our website soon. Our website is easy to remember: it is www.mtwilson.org.

The Fellow. I don't think that's easy to remember! [Laughter.]

The President. Well, thank you very much; I'm very sorry your AV presentation didn't work out.

Our next speaker is Dr. Apostolos Christou from Armagh Observatory, who will talk about observing the satellites of Uranus at equinox.

Dr. A. Christou. First of all, I would like to thank the Society and particularly Helen Walker for providing me with the opportunity to talk about this, let's say, unfortunately-named planet of our Solar System. I would also like to acknowledge the many contributors to this work, without whose help I would not be here showing you data.

Our story begins with a simple equation. On the left-hand side we have the first President of the Society plus a rather shabby-looking back garden in Bath, that would be 19 New King Street. This equals on the right-hand side the first new planet of our Solar System, Uranus, discovered by Sir William Herschel in the late

18th Century.

In the 200 years that followed we managed to eke out some basic information about the planet. Its rotation axis is practically lying on its orbital plane, so the planet suffers from extreme seasonal changes in insolation. It has five major satellites, four of which were discovered by two former presidents of the Society: Titania and Oberon by William Herschel himself, and the next two in, Ariel and Umbriel, by William Lassell from Liverpool. The last one, Miranda, was discovered by Gerard Kuiper in the middle of the 20th Century. More recently, we discovered that the planet has also rings similar to those around Jupiter and Saturn.

Then the space age came, and with it the *Voyager 2* flyby of Uranus in 1986. *Voyager 2* wrote the book on Uranus. In fact, it wrote more than one as I have two of those resting on my office bookshelf. Above all, it revealed to us a complex system that one cannot really hope to understand with a single flyby. There are many such systems in existence both inside and outside our Solar System. The Uranian one belongs to the very few that we have in our own backyard of the Universe. It shares many similarities with other known systems, but also holds important differences. Consequently, it has a lot to teach us about what works and doesn't work in Nature.

Despite its obvious value to comparative planetology, poor Uranus has an image problem. It's bland-looking compared to the splendour of the rings of Saturn or the festoons in Jupiter's atmosphere. It is more difficult to reach than those two planets, and there are other reasons I won't go into. Partly to blame for this state of affairs was the unfortunate timing of the *Voyager 2* encounter. It occurred during winter solstice when the planet's south pole was facing the Sun. Consequently, the northern hemispheres of Uranus and its satellites were in constant darkness at that time. Moreover, the 'bullseye' encounter geometry prevented close approaches to any of the satellites except the innermost one, Miranda, and a moderately close one with the next one out, Ariel. It is, perhaps, not a coincidence that those two satellites turned out to be the most geologically interesting of the five.

Available maps of the satellites are woefully lacking in that their entire northern hemispheres are *terra incognita*. In addition, knowledge of fundamental physical and orbital parameters of the satellites, including the masses, mean densities, and orbital inclinations are poor at best. Mass estimates are currently uncertain by 10% or more. This can be compared with an uncertainty three orders of magnitude lower for the masses of Enceladus, Tethys, and Dione, three satellites of Saturn of similar size to those of Uranus, from pre-*Cassini* Saturn Orbit Insertion (SOI) ground-based and spacecraft data. The result is that models of the formation, internal structure, and orbital evolution of these bodies are poorly constrained. The latter two are inextricably linked as satellite systems, such as those of the giant planets, are driven by steady and episodic exchanges of angular momentum and energy due to tidal forces and passages through mean-motion resonances.

In hindsight, it would have seemed preferable (albeit impractical) to delay *Voyager*'s arrival at Uranus by 20-odd years or send a new probe to Uranus when lighting conditions and encounter geometry would allow a more comprehensive study of the Uranian system. Nevertheless, in 2007 the planet reached its spring equinox. A worldwide observational campaign was organized and is now in progress to observe rare changes, both physical and apparent, in Uranus and its environment. If initial results are anything to go by, Uranus' image as the 'boring' giant is becoming a thing of the past.

Here I will discuss our own contribution to this campaign, having to do with observations of the major Uranian satellites as they undergo so-called mutual events. Mutual events are apparent phenomena (*i.e.*, no physical change is involved) that occur when two satellites line up with either the Earth or the Sun. In the case of the former we have an occultation (the disc of the satellite in front hides the disc of the satellite in the background) and, in the case of the latter, an eclipse (the shadow of the satellite in front is cast upon the satellite further away). In both cases, photometric monitoring of the event will show a dip in the brightness of the satellite or satellite pair in question. The value of these events is that they allow the astrometric determination of the relative position of one satellite relative to the other with a precision several times better than that of conventional (*i.e.*, centroiding or PSF-fitting) astrometry. As such, they are sensitive to the minute gravitational tugs between the satellites that are responsible for periodic changes of their orbits with time. These are excellent proxies for determining the satellites' masses.

This type of measurement is not new in the study of satellite systems. Mutual-event observing campaigns of the Galilean satellites of Jupiter have been carried out since 1973, and of the Saturnian satellites since 1980. In fact, I was able to record several mutual events between the Galilean satellites of Jupiter using a small telescope and video camera set up on the roof of the Armagh Observatory. In the case of Uranus, the last planetary equinox was in 1965. The tools necessary to carry out such observations simply weren't available then. The 2007/08 equinox provides the first opportunity to observe and record mutual events between the Uranian satellites.

To recap on the objectives of this work: using a world-wide observing network, we aim to acquire light curves of as many mutual events as possible in order to fit improved estimates of the satellites' physical and orbital parameters.

To prosecute such a programme of observations, one requires two essential elements. The first one is telescope time. We have been fortunate to be granted access to a wide range of instruments, namely the two 2-m *Faulkes* telescopes on the mountain of Haleakala in the island of Maui, Hawaii, and at Siding Spring Observatory in Australia; the 10-m *Southern African Large Telescope* in Sutherland, South Africa; and a 0·4-m instrument owned by a private school in the outskirts of Athens, Greece. We mainly utilized observing time on those instruments that would have otherwise gone unused. We also needed predictions of the events based on ephemerides (models) of the satellites' motion. The predictions would not be perfect, otherwise the reason for carrying out the observations would be negated. Rather, their function is to identify intervals of time (as narrow as possible, usually 20-minutes or half-an-hour long) within which an event is likely to occur.

Having these predictions in hand, one can then form a general picture of the mutual-event season. From the point of view of an inhabitant of Uranus, the Sun crosses the equatorial plane from south to north in 2007 December, while the Earth does so three times, in 2007 May and August, and in 2008 February. All these crossings are characterized by roughly one-month intervals rich in mutual events, either occultations (Earth crossing) or eclipses (Sun crossing).

Hence the first opportunity to observe such an event was in May last year. We used the *Faulkes Telescope South* in Australia to observe an occultation of Oberon by Umbriel on 2007 May 4. The weather cooperated and we were fortunate in obtaining a light curve of the event showing a dip near one of the available predictions, the first observation of its kind. The model fit to this observation demonstrates what I mentioned earlier: that the positional uncertainty in mutual-event observations, translated from the uncertainty in timing the mid-event instant and the magnitude of the light-curve dip, is significantly smaller (here an order of magnitude smaller) than the uncertainty in the predictions. This result gives us confidence that the sought-after improvement in the system parameters that govern the motion of the satellites is achievable.

Now I would like to show another observation, this one carried out with a different type of telescope, a 0·4-m Schmidt-Cassegrain that is owned by a private school in the outskirts of Athens. In fact, one of the observers of this event (Kostas Gourgouliatos) is right here in the audience. The event (an occultation of Umbriel by Oberon) was positively detected; the data are relatively noisy as you see in the preliminary light-curve that I show here. As it happened, our detection was confirmed by a light curve of the same event produced by an amateur astronomer and published on a mailing list, showing a dip at the same time.

The next step, on the data-processing side of things, is for us to reduce the observations, around ten observed events in total for our part, into positional information for the satellites. These data will then by sent to IMCCE in Paris, France, and/or JPL to become the input for the orbit-fitting process. We would also like to explore other ways in which the data may be useful, for example, to carry out conventional astrometry of the satellites in the several-thousand CCD frames of the Uranian system that were acquired during the course of this work. One tantalizing, yet more speculative, prospect, is to use the light curves we obtained as input to an inversion process that will yield low-resolution maps of the unseen hemispheres of the satellites. This is not a new idea; it was used previously to construct a map of the Charon-facing hemisphere of Pluto in the pre-*Hubble* era using light curves acquired during the Pluto–Charon mutual-event season between 1985 and 1990. In our case, it all depends on the quality of the light curves obtained as the inversion process is quite sensitive to the noise in the data.

On the observational side, the mutual-event season is not over yet. There are several events predicted to take place between now and early 2009. These are actually quite valuable as they serve to extend our observational time baseline by a factor of several. If you have observing time during those periods and you don't know what to do with it, here is an idea. After 2009, the next major event in the Uranian calendar is summer solstice in 2028 where the northern hemispheres of Uranus and its satellites will be in constant sunlight. Twenty-one years after that, in 2049, the next equinox takes place. I hope that by adopting a healthy lifestyle I will find myself back here in 42 years' time, perhaps addressing a slightly different audience and telling them about the wonderful science we are able to do by combining data from two consecutive Uranian equinoxes.

The President. I did enjoy the reference to 2049! Any questions?

Dr. J. G. Morgan. The light curves you displayed there are well short of having the quality of photometric resolution and time resolution to do the albedo inversions. What are the prospects technically and financially to be able to do it in 40 years' time, do you think?

Dr. Christou. Well, the advantage of doing it twice in a row is that you have a very long time baseline and you can limit small errors in the fit that have time to run off in the intervening period.

Dr. Morgan. Well, that would be for the orbit, I was referring to surface features.

Dr. Christou. Who knows? I could actually be living on Uranus in 2049. [Laughter.]

The President. Well, thank you very much for your talk. [Applause.] The last speaker for today is Dr. Chris Lintott of the University of Oxford, and he is here to talk about 'First results from Galaxy Zoo, or what to do with 125 000 research assistants.'

Dr. C. J. Lintott. I'm pleased to have the opportunity to present some of the initial results from the Galaxy Zoo project, which must be the world's largest astronomical collaboration. First I need to thank my collaborators from the University of Oxford, the University of Portsmouth, Johns Hopkins University, and from Fingerprint Digital Media in Belfast.

Galaxy Zoo was designed as a solution to a simple problem, a problem caused by the advent of large-scale imaging surveys such as the Sloan Digital Sky Survey (SDSS). SDSS is based in New Mexico, where a 2·4-m robotic telescope has imaged approximately half of the northern sky, capturing images of almost 1 million galaxies. An accompanying spectroscopic survey has measured the redshift of many of these systems, but we are first of all concerned with the images. A first glance reveals a wide variety of galaxy shapes and types, ranging from simple spirals to large ellipticals and merging or irregular systems. Such a distribution in morphology is, of course, not new; the first systematic attempt to classify galaxies by shape was made by Hubble who introduced his famous tuning-fork diagram. While we no longer think of the tuning fork as an evolutionary sequence, it is remarkable that a wide variety of physical properties, including gas content and star-formation history, have turned out to be correlated with galaxy morphology.

In past galaxy surveys, it was possible for a small team of expert astronomers to sort through every galaxy, leading to the sterling work of de Vaucouleurs, for example. This is impossible, at least in practice, for surveys of the size of the SDSS, yet working with Kevin Schawinski (whose DPhil is based on visual classification of 50 000 SDSS galaxies), it became clear that this would be desirable. Hence the launch of galaxyzoo.org.

The website was designed by Phil Murray at Fingerprint Digital Media, who donated his time freely, but gave the website a professional look. The heart of the site is reached after users read a short tutorial and pass a brief test, and consists of the analysis page. An image from SDSS is displayed alongside buttons allowing classification in one of six categories: a spiral with anticlockwise arms, a spiral with clockwise arms, an edge-on spiral, an elliptical, a merging system, or 'don't know'.

The project was launched on BBC Radio 4's *Today* programme and was covered on the BBC News website. Despite initial expectations of a few thousand classifiers, the site was quickly overwhelmed in a valuable lesson as to how the internet works! Blogs and other sites quickly picked up on the project, a *Wikipedia* article was created and newspapers around the world picked up on the story. Thanks to sterling efforts from the team at Johns Hopkins University, melted servers were quickly replaced and less than 24 hours after launch 40 000 galaxies an hour were being classified. This compares to the largest published data set (from Fukugita *et al.*) of 9000 classifications, and to Kevin's 50 000. A peak was reached in two days with almost 70 000 classifications per hour, but in less than six months more than 100 000 users completed more than 30 million classifications.

These vast numbers of classifications give us a huge advantage: by obtaining multiple classifications it is possible to quantify the uncertainty in a particular galaxy's classification. One then has the option of deciding between a smaller, very clean sample, or a larger sample within which the individual classifications will be much less reliable. For most purposes, we have been using a 'clean' sample which requires roughly 80% agreement in classifications.

The first paper to be submitted (Land *et al.*, astro-ph 0803.3247) deals with the large-scale distribution of spiral galaxies. It follows the work of Longo (astro-ph 0707.3793) who found an excess of anticlockwise spiral galaxies in a study of $\sim$ 2500 SDSS galaxies. The excess was aligned with the so-called 'axis of evil' in the cosmic microwave background. Our results confirm such an excess at the 5-σ level, corresponding to $\sim$ 52% of spiral galaxies being anticlockwise. However, when mirror images were used the effect remained, indicating the presence of a subtle bias, either innate or induced by the website design. Once such a bias is taken into account, our results are consistent with there being no such excess.

A second example project using Galaxy Zoo classifications is the study of overlapping galaxies, led by Bill Keel of the University of Alabama. As it is possible to use the more distant galaxy of a pair as a backlight, overlapping galaxy pairs are excellent laboratories to measure the distribution and properties of dust in a wide variety of systems. Work by Galaxy Zoo users has produced a catalogue of more than 800 such systems, an order of magnitude larger than previous work, and five nights of follow-up time are scheduled on the 3·5-m *WIYN* telescope in April.

A major difference between visual determination of morphology and methods depending on proxies such as concentration or colour is the identification by the former method of a population of apparently star-forming ellipticals. Galaxy Zoo has made it possible to constrain the properties of this population at sufficiently low redshifts that we can be confident of minimal contamination from spiral galaxies. Despite their apparent elliptical morphology, these systems have Hα star-formation rates of between 0·5 and 50 $M_{\odot}$ per year, compared to the Milky Way value of 1–3.

It has long been known that, on average, elliptical galaxies dominate dense environments, whereas spirals are more common in less-dense fields. While the morphology–density function can be constrained at higher redshifts, doing so locally requires coverage of a large area of sky and hence can only be achieved with Galaxy Zoo and the SDSS. We find a smooth morphology–density function in contrast to previous studies, and furthermore find that the form of this function is due to an underlying colour–density function. Interestingly, the population of red spirals ('red' being defined according to the criteria given by Baldry *et al.* in 2006) has a definite peak in frequency at densities corresponding to the edges of clusters.

The four projects listed already are only a flavour of what is possible with Galaxy Zoo. I want finally to illustrate the ability of visual classifiers to identify the unusual, using an object known as Hanny's *Voorwerp* as an example. The *Voorwerp* (Dutch for 'object') was first identified as a blue blob in the vicinity of IC 2497 by Hanny von Arkel, a Dutch school teacher. Spectral follow up with the *WHT* indicated that the gas was highly ionized at a temperature of $\sim$ 15 000 K but with few if any stars present. Observations with the UV and X-ray telescopes on board *Swift* indicate the absence of any sufficiently strong ionizing source in the present-day system, and lead us to conclude that IC 2497 must have undergone recent dramatic changes. Some 100 000 years ago it would have been the brightest quasar in the sky. However, the outburst ended and it faded, leaving the *Voorwerp* as a light echo. If this can be confirmed (and detailed IR and radio images are necessary) then we will be able to probe the evolution of quasars on time scales which have hitherto been completely inaccessible.

This is just a flavour of what can be achieved *via* 'citizen science': the active participation of the public in the first stages of data analysis. With the advent of widespread broadband connections, such techniques are capable of detailed analysis of large sets of data, as well as involving large numbers of the general public in cutting-edge science.

The President. Well, thank you for that superb talk. I was already thinking about the 125 000 participants: the RAS Council have been discussing the idea of a new class of membership of the RAS, which should be called 'Friends of the RAS' or something like that. This would be for people who are supportive of astronomy and might like to read *Astronomy & Geophysics*, but not necessarily to attend these meetings and enjoy all our other benefits. So don't delete the list [laughter], obviously this is the first group of people that we could target! I thought modestly that we could aim for perhaps an extra 10 000 members of this broader class, but now we can be much more ambitious!

Dr. Lintott. Well, we'll cut a deal: as long as everyone in the RAS has to classify 1000 galaxies we could do something [laughter].

The President. I actually have a scientific comment which is about your blob. I think it would be very interesting if one could get *Spitzer* data on, not the blob, but the galaxy, because if there was a hidden AGN seen edge on, you would see this in the infrared signature, and I think this would be a good test as to whether there's an AGN that's edge on and hidden from us, or, as you say, a dead AGN.

Dr. Lintott. We're certainly looking at *Spitzer* and you can do some really useful things with *UKIRT* as well. The infrared is the big gap. We've got coverage of almost every other part of the spectrum.

The President. You need to get out to ten microns.

Dr. Lintott. I'm actually looking for somebody to write the proposal at the minute, so that's another thing you've volunteered for! We'd also love to get some decent radio data, because it's not clear whether this thing is a dwarf galaxy that just happened to be in the wrong place at the wrong time, or whether there is a lot more disturbed but cold material around, so mapping in the radio is essential as well.

Professor R. S. Ellis. The accuracy of the galaxy classification depends on size of the galaxy, and the Sloan catalogue contains large galaxies and small galaxies, so how is the uncertainty in all these classifications going to be taken into account?

Dr. Lintott. It depends on what you want to do. The most sophisticated thing we've done to date was this morphology–density relation.

This only works when you have lots of data, such as the big catalogue that we have, and what we do is split the data into bins in redshift, in magnitude, in absolute magnitude, and in apparent size on the sky. In this case we're interested in the ratio of ellipticals to spirals. We can assume there's no evolution, so we assume the lowest redshift bin which has significant galaxies has the most correct value of the spiral to elliptical ratio; you can then use that to get a correction as a function of magnitude and size, and then apply that to the rest of the galaxies. So that's fine for the population; if you then want to look at a particular galaxy, you want a classification, and it gives you an error. If it's a spiral and it looks like a spiral then it's almost certainly a spiral. If it's an elliptical, then you have a measured uncertainty of the odds of it accidentally being a spiral that was just too faint or fuzzy. So when we release the data, you have your list of ellipticals and you have the option to cut at whatever level of error you think is acceptable, depending on how many galaxies you want. The punch-line is that out to a redshift of about 0·05 we should have caught almost all the spirals that are there; the uncertainty gets significant after that.

Mr. C. J. North. Do you think it will possible to automate this and get a computer program that will do the same job?

Dr. Lintott. I always get this question. The best version I know of, I think, is

Ball *et al.*, who looked at using a neural network to do this. They agreed with human classification something like 80% of the time, which is pretty good, but the worry is that the 20% of the time isn't smoothly distributed across all galaxies, so for now humans are winning. We're planning to put a challenge out to the computer-science community, where we could give them the data but not the right answers, and let them compete to see what's out there. But to be honest, if we try to write a program to do this or develop that, that would have taken longer, been more expensive, and wouldn't have had the outreach dimension, so for now we'll stick to this.

The President. I also think it wouldn't give you the information on the interacting galaxies or the blob.

Dr. Lintott. Exactly. Finding the unexpected is a huge advantage. I should say we've got about sixty gravitational-lens candidates as well, for example. Some of them won't be lenses, but some of them look very convincing, so they're a by-product as well, and it's a huge advantage.

Dr. Mitton. I've got a comment. This is all deeply impressive because you're an astronomer, you've done some astronomy here, but rather like the SETI people before, you're actually developing a completely new way of doing science, that's to say, using huge networks of computers, being used by human beings who just need a little bit of training in order to conduct mass-observation science. The thing that occurs to me is that if you take a completely different area, ornithology or bird migration, then the kind of science you and your colleagues have developed can actually be applied in a different way to other areas of scientific enquiry which need huge amounts of data that's collected in a fairly uniform way. What I also find impressive is the way in which you've involved the general public; that's something we're all being asked to do all the time.

Dr. Lintott. Let me just quickly respond to that. Thank you for the compliment and thank you for the comment as well, because it reminded me to say that I couldn't show the slide that credited the people who thought of this, because we're not the first. The story is that the *Stardust* probe that flew past Comet Wild 2 collected dust grains and brought them back, and within those dust grains there should be a few interstellar ones, and the best way to identify candidates was to look. What triggered this was that they got 40 000 people to look at 20 million images of dust grains, but I remember thinking, if people will look at dust grains in their spare time ... [laughter]. But yes, there are other applications, and a particular one that springs to mind is that zoologists have got very good at putting cameras on small animals but need people to watch the footage until the animals do something interesting, and this would be ideal for that.

The President. I think it is such a good story for astronomy, and at a time when we're under some pressure. Very good news.

Professor Jocelyn Bell-Burnell. You talked about how each galaxy is classified by 30 or 40 people independently, you've talked about the agreement. There'll also be interesting data on where there is a lot of disagreement or dispersion and there'll be interesting stuff in that as well, some of it obvious, some of it probably not obvious.

Dr. Lintott. I agree. It's something that's unfortunate about the weighting we've used. At the minute, for what we've needed to do, we've only really rewarded those astronomers who've agreed with the majority — we're using those data. But the system's set up so that you can pick out users with particular characteristics, so with the merger sample, because we didn't train people to look for mergers very well, we have a mix of things. But, Daniel Darg, a PhD student at Oxford, has picked out the ones that we want, so we can choose to listen to the users who agree with him, and if you roll that through the database, suddenly it's as if he's looked at the whole sample. The statistics are such that we can tune our database to do almost any job given these votes, so looking at places where people disagree would be amazing.

Professor D. Lynden-Bell. You say that there's 80% agreement between people and computers; when Ofer Lahav was doing a similar sort of thing, he found that the experts only had a certain percentage of agreement among themselves, so it seems to me that 80% is quite good; but what's the agreement between observer and observer in your sample — is it more than 80%?

Dr. Lintott. I'm not sure of that figure. I know the paper you are referring to and it's a fantastic description and it shows that people are more likely to classify galaxies in the same way as their supervisor, if I remember correctly [laughter]. But they were doing a slightly more difficult job: they were doing more detailed classifications. The 80% figure from Ball *et al.* is for sorting ellipticals from spirals. Our agreement between users is above that level for about 300 000 of the galaxies in the Sloan survey. Most of the others are faint and fuzzy, and lots of them will be very strange. We can compare with professional samples, we can compare with Fukugita *et al.*, and we can compare with Longo's results and other papers; we get as good an agreement between our data and them as they get with each other. So our data is as good as having a couple of professionals look through. Of course, the advantage remains that if you're a professional and you do it yourself, you're 100% convinced that you're right, but we're asking people to believe that the public can do this.

Professor Lynden-Bell. But I'm asking you public *versus* public. Do you know what that figure is?

Dr. Lintott. Not across the whole sample. The figure that's in my head is that 300 000 of the galaxies in the Sloan survey have a better than 80% agreement between the public, but I haven't done any statistical population study on the data yet. As I said, this was a side project that got out of hand [laughter].

The President. Last question. John?

Professor J. Barrow. When a member of the public participates, do you have any little test that you offer to check their eyesight quality? For example, you could look at the low-brightness counts changing as the observer's eyesight fades in later life. How do you know who the people are?

Dr. Lintott. We don't yet. We're just beginning. Jordan Raddick at Johns Hopkins, who's an outreach specialist, is beginning a study of our users so we can find out who they are. We do require people to pass a test, so we at least have some confidence of their ability before they get let loose on the database, but I say from experience that it's not eyesight, it's monitor quality that matters, so if you can buy all of our users a big, nice, glossy, 24-inch plasma screen to attach to their computer, our results will hugely improve. I'll look forward to applying to STFC for that.

The President. Some of them are probably trying to do it on their mobile phones [laughter]. Thank you very much, Chris, that was wonderful. [Applause.] The next meeting will be on Friday May 9, starting at 2 pm, the Annual General Meeting.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Thursday 2008 April 3 at $14^h\ 00^m$
in the Larmor Lecture Theatre, Queen's University Belfast

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

The President. Welcome to this ordinary meeting of the RAS at the Belfast NAM. I am going to give a brief report on what the RAS has been doing, and then we have the awards ceremony, and two lectures, by Mike Bode and Joe Silk.

On what the RAS has been doing: when I announced to you, on the ballot paper two years ago and in an article in *A&G*, that I wanted to move the RAS in the direction of greater involvement in science policy, I don't think I quite knew what I was letting myself in for! We have obviously been very active — we'll discuss the funding crisis later. In the year leading up to the CSR announcement last November, I lobbied in the Treasury and in OSI and argued the case for blue-skies research and the benefits for the UK economy, both direct and indirect, of our work. And I think this was effective — it may not seem so, given the outcome — but it was effective in that Ministers consistently expressed their support for basic science, and so has the Treasury in its budget statements, even though they've been reluctant to admit that the CSR settlement is a problem for us. We'll come back to this later, of course. But remember that at any time John Denham or Ian Pearson could have said "I think we've been spending too much on basic science", but they've never said that, a very positive feature. And we have to keep making that case for pure science, of course.

So, some of the things we have been doing this past year: we made a joint submission with the Institute of Physics (IoP) to the House of Commons Select Committee on Innovation, Universities and Skills, and I gave oral evidence to the Committee. Through this period we have, of course, been pressing STFC on issues of consultation and we are beginning to see the fruits of that. Through all of this, I have been very strongly supported by the RAS Council, who have given good advice, and by our Executive Secretary, David Elliott, and our excellent Policy and Press Officer, Robert Massey. We put out press statements on *Gemini*, the grants cut, the STFC development plan, and the programmatic review — and each of these releases has led to multiple press and media stories. I've given numerous interviews, and Robert has done even more, and I believe there have been more than 130 press stories worldwide about this NAM, an indication of how well he and his team have been doing. We have also been talking to Professor Bill Wakeham about his review and we will be giving evidence to that later. Over the year I have sent you a number of e-mails to try and explain what I think is going on, as in some areas I have been as much in the dark as you.

The RAS has been active in numerous other areas. One thing that has been very important in the past year is education and outreach. One of the milestones is that the Education Committee completed kite-marking of web-based education resources: there is a very important web page where schoolteachers and others can find out which are the really good resources in astronomy and space science on the web. We have also started a new public-lecture series at Burlington House, which has been a great success, and for the most recent lecture the queues went right out to Piccadilly and were about ten times bigger than the queues to the Royal Academy! This shows you how valued astronomy is by the public.

And the RAS, through Ian Robson, is leading in a co-ordinating rôle the activities for the International Year of Astronomy 2009, which is a very big opportunity for us all.

We've tried to expand our membership: we currently have, I believe, 3300 members. After the AAS, we are easily the largest astronomical society in the world. We have introduced new concessions for postdoctoral students and we are thinking about a new category of membership or association — something like 'Friends of Astronomy' — for members of the public who might support us. We have established an International Committee to serve our approximately 1000 overseas members, and apart from the IAU, we are the largest international society, with more international members than the AAS. The purpose of this committee is to deal with the many enquiries we get from other international societies, but also to start thinking about capacity-building in the developing world; there are already some very good projects — I know of one at Oxford for example — and extremely important ideas about how we can do something in the developing world to build up their involvement in science.

The Burlington House refurbishment was completed this year, and if you haven't been you should go there. It is a very nice place to go for a meeting, we have rooms equipped with WiFi, and we plan to improve the services further in the future. It was completed on budget and on time.

Finally, I know that there are a lot of people here who are not members of the RAS, and I do urge you to join because we need you, we need to be able to say that we speak for the whole community. For the postgraduates, there is a wonderful offer of a £1 subscription for the first year — how can you not join, at least for a year [laughter]? Make me a happy man and join after this meeting.

Professor C. Frenk. How many people do you think are in the UK who are not members and could be encouraged to join?

The President. We believe over 90% of the professoriate are members, amongst academic staff we believe it is about 70%, and among postdoctoral and postgraduate students it is somewhat less than that. Our professional membership at the moment is something over a thousand, 1200 or something like that, and the maximum, if everybody in the professional community were to join, might be 2000. On the idea of 'Friends of Astronomy', I think an indication has been given by Chris Lintott's 'Galaxy Zoo' project, where 100 000 people logged-in to their web page to classify galaxies, and I believe that all those people are clearly interested in astronomy and might wish to become members and receive their copy of *A&G* and so on.

We will press on with our programme. The first speaker is Professor Mike Bode of Liverpool John Moores University, and he's going to be talking to us about 'ASTRONET: towards a strategic plan for European astronomy.'

Professor M. F. Bode. It is a great pleasure to be invited here today to give an update on the work of the ASTRONET project in developing a long-term plan for European astronomy. ASTRONET is funded by the EU as an ERA-net whose general aim is to improve coherence and co-ordination of research across Europe. The project was established by a group of the major funding agencies in Europe and now comprises members from the majority of European countries.

It is obvious that our plans for the development of our subject over the next two decades are (reassuringly) ambitious. However, they involve the design, construction, operation, and exploitation of facilities costing in total several billion Euros. Most of this cost will be borne by the agencies and hence it was quite natural for them to want to see a coherent plan. In addition, if we are successful, one would hope to secure additional funding for our science by demonstrating its importance and pan-European nature at governmental levels.

ASTRONET has a very wide remit covering everything from the Solar System to cosmology and encompassing both ground- and space-based observation at all wavelengths, plus astroparticle physics and gravitational-wave techniques. In addition, it includes theory, computing, and the Virtual Observatory, plus public outreach, education, and industrial links.

Several 'work packages' exist within the project, which began in 2005 and ends in 2009 September. Of these, perhaps those of most immediate interest to this meeting are the Science Vision and the Infrastructure Roadmap. The Science Vision sought to set out the main questions that astronomy is likely to be addressing over a 20-year horizon. It has been produced by a working group and four panels, with each panel focussing on a different main question, and involving around 50 of Europe's leading astronomers. Following publication of a draft, and wide consultation (including a Symposium in Poitiers in 2007 January), the final Science Vision document was published in September last year.

The primary purpose of the Roadmap is to define the set of facilities and other infrastructures that are needed to deliver the Science Vision. A similar structure to that used to develop the Science Vision has been used to develop the Roadmap. The five Roadmap panels include three that are effectively technique and wavelength-range based plus one covering theory, computation, *etc.*, and the other outreach, education, and industrial links. The panels report to a working group that oversees and synthesizes their work and altogether over 60 European scientists are panel or working-group members. In addition, links to the agencies (including ESA and ESO, who are in any case formal ASTRONET members) and related projects (such as OPTICON, RadioNet, and ASPERA) are strong and have proved invaluable.

The panels and working group began their work in earnest just over a year ago and have held around 30 meetings in this period, including one recently with the funding agencies where a preliminary draft of the Roadmap was discussed. However, as with the Science Vision, a crucial part of the process is the involvement of the astronomical community as a whole in formulating the conclusions. The public draft of the Roadmap document will therefore be available about a month from now, which will be the start of the discussion and consultation period. The centrepiece of this process is the Liverpool Symposium in 2008 June, for which registration opened yesterday and we have already had great demand for places. I would therefore urge everyone who is interested in making contributions to register for the Symposium as soon as possible, and I look forward to seeing many of you in Liverpool in June for what promises to be a very important and lively meeting.

The President. Are there any questions?

Professor Monica Grady. I'd just like to ask about the laboratory studies that you mentioned. Are you still taking evidence or acquiring information about that aspect?

Professor Bode. Yes, but not in terms of the draft, because that's in an advanced state; but once you see the draft, you can contribute anything that we have missed, either on the web, through the forum, or by attending the Symposium, and we will take it into account. We hope we have done a reasonably thorough job, but it will certainly not be complete and this is why community input will be particularly important.

The President. Let's thank Mike again. [Applause.]

We now come to the RAS awards, and for this part of the meeting I'm assisted by Kate Maguire and Neale Gibson, who are PhD students at Queen's University Belfast. First, the Gold Medal for Astronomy is presented to Professor Joseph Silk, of the Nuclear and Astrophysics Laboratory, Oxford.

Mr. N. Gibson. Joe Silk has been at the forefront of astrophysical research for 40 years. In a series of papers beginning in 1967, he provided key predictions on the angular fluctuations expected in the temperature of the cosmic microwave background, including the effects of curved space and now incorporating cold dark matter. He also predicted what is now known as Silk damping, the damping scale of the baryonic condensations. He has made significant contributions in the areas of massive weakly-interacting particles, high-energy astrophysics, the physics of the intergalactic medium, the origin of dwarf galaxies, star formation, and in a heavily-cited paper with Martin Rees, he developed the concept of AGN feedback, and derived the relationship between the mass of the central black hole and the mass of the host galaxy. For a lifetime of outstanding contributions, Joe Silk is awarded the 2007 RAS Gold Medal for Astronomy. [Applause.]

Professor J. Silk. This is a very great surprise and a wonderful honour; thank you very much [applause].

The President. The Herschel Medal is awarded to Professor Max Pettini, of the University of Cambridge.

Mr. Gibson. Max Pettini has made several seminal contributions to extragalactic astronomy. His most important achievement has been the development of a comprehensive picture of cosmic chemical evolution using observations of galaxies and the intergalactic medium. In addition he was a co-discoverer of the Lyman-break galaxies, the first population of young galaxies directly observed in the rest-frame ultraviolet with redshifts greater than 3. This group has since made one ground-breaking discovery after another, characterizing the star-formation properties, stellar masses, chemical compositions, internal kinematics, and structures of the dominant galaxy population in the first 3 billion years of cosmic history. Max Pettini has demonstrated a consistent record of scientific excellence, creativity, and impact, and the RAS is pleased to award Professor Max Pettini the 2007 Herschel Medal. [Applause.]

Professor M. Pettini. I'm delighted of course, and thoroughly surprised, so thank you very much [applause].

The President. The Chapman Medal is awarded to Professor André Balogh of Imperial College London.

Ms. Kate Maguire. André Balogh has led several investigations of outstanding merit in the area of solar-terrestrial physics. He was until his recent retirement Principal Investigator of the magnetometer on the ground-breaking, four-spacecraft, *Cluster* mission. Under his leadership, the *Cluster*-science group at Imperial College London has made many important discoveries on the bow shock, the magnetopause, cusps, and solar wind, using pioneering multi-spacecraft techniques. He was also latterly Principal Investigator for the *Ulysses* magnetometer. Here, data from his instrument has again played a world-leading rôle in determining, for the first time, the unexpected high-latitude structure of the solar and heliospheric magnetic fields, and the large-scale three-dimensional structure of the heliosphere. Because of these pioneering contributions to solar-terrestrial physics, and in particular for his contribution to the discoveries flowing from the highly successful *Cluster* mission, André Balogh is richly deserving of the Chapman Medal. [Applause.]

Professor A. Balogh. Thank you, Mr. President [applause].

The President. The Award for Service to Astronomy is to Dr. Günther Eichhorn, formerly of the Smithsonian Astrophysical Observatory.

Mr. Gibson. Günther Eichhorn has been until 2007 the project manager for NASA's Astrophysics Data System, leading the small team of six who develop and operate it. He himself is a hands-on programmer and developed much or most of this specialized database system and its world-wide-web interface. Through his efforts, most of the astronomical literature is available on-line through the web. It is no exaggeration to say that his work has revolutionized the way that astronomical research is carried on. Almost every astronomical reference is available, in seconds, without leaving one's desk. His creation has affected the life of the nation, in fact the life of the global scientific community, by making primary scientific literature available to all, including amateurs and teachers. His work has been outstanding, advancing astronomy as a science, rather than advancing astronomy by increasing its knowledge base through research — hence the award to Dr. Günther Eichhorn for Service to Astronomy. [Applause.]

Dr. G. Eichhorn. Of course, I'm deeply honoured and surprised to get this award. Running the ADS was fun over the fifteen years that I was there, and it was very rewarding seeing something come to life that really affects astronomy, and I was very happy to be able to do that. [Applause.]

The President. The Fowler Award for Astronomy is made to Dr. William Percival of the Institute of Cosmology and Gravitation at the University of Portsmouth.

Ms. Maguire. Will Percival has made outstanding contributions to observational cosmology that have had a major international impact. He led a key analysis of the *Two-degree-Field* Galaxy Redshift Survey (2dFGRS) and was the first-author of a paper on the 2dFGRS, on the power spectrum and matter content of the Universe, that has acquired over 400 citations. The result of this paper has been used extensively by the *2dF* team and many others, most notably by the *WMAP* team. He and the *2dF* team achieved an important breakthrough with the detection of baryon oscillations in the galaxy power spectrum. He recently led a comprehensive analysis of baryon oscillations and the shape of the galaxy power spectrum. Will Percival has completed several projects of outstanding quality and scientific impact at an early stage in his scientific career, and is awarded the 2007 Fowler Award for Astronomy. [Applause.]

Dr. W. Percival. I'm honoured; thank you very much.

The President. The Fowler Award for Geophysics is made to Dr. Christine Thomas of the University of Liverpool.

Mr. Gibson. Christine 'Tine' Thomas is a leading figure in the new generation of seismologists. Her speciality is array seismology, which involves the use of dense seismic networks to image the fine-scale structure of the Earth's deep interior. Much of Tine's work is concentrated on the enigmatic thermo-chemical boundary layer, that lies at the base of the Earth's mantle, which influences such diverse phenomena as plate tectonics and the generation of the Earth's magnetic field. She has developed novel methods to image this region and has been highly successful in collaborating with scientists from other disciplines in order to understand better the nature of the lower-most mantle. Tine Thomas is a committee member of the British Geophysics Association and serves as Chair of the Education Board. For her outstanding work, Christine Thomas is awarded the Fowler award in Geophysics. [Applause.]

Dr. Christine Thomas. I'm very honoured to receive this award, and thank you very much [applause].

The President. The Group Achievement Award is made to the *2dF* Galaxy Redshift Survey team.

Ms. Maguire. The *2dF* Galaxy Redshift Survey is surely the largest observational project undertaken to date by primarily UK astronomers without direct personal hardware involvement. Taking advantage of the unique capabilities of the instrument, the 2dFGRS was uniquely ambitious, timely, farsighted, and it targetted carefully assessed and important scientific goals. The recently completed observational side of the project led to the accumulation of almost a quarter of a million galaxy redshifts, far more than any previous spectroscopic survey, whose analysis by a world-leading team has led to more than 40 papers from the *2dF* team alone. Similar numbers of papers have been published by other authors based on the public release of the *2dF* dataset. In recognition of the work of an essentially Anglo-Australian team of more than 40 scientists, the Group Achievement Award is given to the whole team, and Professor John Peacock is invited to accept the award on their behalf. [Applause.]

Professor J. Peacock. We should say that because this is an award for the whole 2dFGRS team, and since there are a good number of them here today, I'd just like them to stand in their seats and receive their applause. [Applause.]

The President. We come now to the RAS NAM poster prizes. First of all I'm going to announce the runners up, who will receive book tokens provided by the Cambridge University Press, and I'll ask them just to stand up as I read them out. The runners up are Ms. Avril Day-Jones of the University of Hertfordshire, for her poster 'Hunting for widely-separated ultra-cool companions to white dwarfs and sub-giants', and to Mr. David Pérez-Suárez of Armagh Observatory, for his poster 'What's cooking inside a bright point?'. Would they like to stand up? [Applause.]

The two winners, who will each receive a laptop presented by David Elliott, regional manager of Dell, Northern Ireland, are: Mr. Jaz Pearson, of the University of Central Lancashire, for his poster 'Multi-vantage-point observations of the coronal mass ejection from initiation out to $\sim$ 200 solar radii' [somewhat out of breath; laughter]; and Mr. Tom Hughes of Cardiff University for his poster 'The migration from the blue to the red sequence of Virgo giant spirals: internal or environmental process?'. [Applause.]

That concludes the award ceremony. I'd now like to express the thanks of the RAS to the organizers of the conference. First of all to Queen's University Belfast for hosting the conference, and specifically the Vice-Chancellor, Professor Peter Gregson; the local and scientific organizing committees: Professor Philip Dufton, Chair of the LOC, Professor Stephen Smartt, Chair of the SOC, Professor Alan Fitzsimmons, Dr. David Jess, Dr. Mihalis Mathioudakis, Dr. Don Pollacco, Dr. Cathy Ramsbottom, Dr. Robert Ryans, Dr. Carrie Trundle, Mr. David Young, Mrs. Jenny McCabe, Dr. Andrew Kavanagh, and Professor Tom Millar; for the website, databases, computing, conference booklet, and registration, Dr. Robert Ryans; secretarial and administration support, Mrs. Jenny McCabe, Ms. Wendy Rutherford, Ms. Nora Lagan, and Mrs. Angela Frew; for the audiovisuals, Dr. Robert Ryans, Mr. Sam Duddy, and Mr. Ian Todd; and finally, last but not least, for general support and assistance, all the postdocs and PhD students of the Astrophysics Research Centre. Thank you to you all. [Applause.]

The President. Now it's my pleasure to invite Professor Joe Silk, of the Nuclear and Astrophysics Laboratory, Oxford, our Gold Medal winner, to give a talk on 'Feedback and galaxy formation'.

Professor J. Silk. [The speaker started by reviewing some of the relevant work done in early star-formation theory. It was Newton who first recognized the notion of gravitational instability and thought that he might be able to make stars, but he realized he could not distinguish between luminous stars and non-luminous planets. Jeans realized that there is competition between gravitational instability and thermal-pressure gradients and on a grand enough scale, gravity would win. Eventually Eddington considered spheres of increasing mass until radiation pressure dominated, which allowed him to predict the mass range of stars. Finally Silk found that in a condensing cold cloud of matter there was a very simple expression which incorporated the gravitational fine-structure constant, α_g, and the fundamental size of fragments, the Jeans mass, which turned out to be 0.01 $M_\odot$; this is the minimum mass in opacity-limited fragments. These fragments act as centres of accretion, which in turn is a simple function of sound speed. The temperature of molecular clouds will determine the mass of forming stars — at 10 K small stars are formed whilst at 1000 K in molecular clouds with a greater sound speed more massive stars are preferentially created. It is currently recognized that accretion cannot fully explain the wide range of cloud masses, so there must be something else which stops accretion: a feedback mechanism. This is thought to be the presence of magnetic fields which thread into the medium and wind up in the forming protostellar clouds.

The speaker then turned his attention to the formation of galaxies and the difficulty of explaining the diverse range of galaxies which we observe, such as the Sombrero Galaxy and M 31. Some are actively forming stars whilst others like the spheroidal systems consist of old stars which are formed very quickly. In disc galaxies this process takes much longer so it seems that some mechanism is slowing down the collapse — some sort of feedback. Possibilities include the combined effect of many massive stars ($> 8\ M_\odot$) going supernova, heating up the gas, and stopping star formation. It appears that the star-formation rate, when compared with the total amount of atomic and molecular gas, provides the basis for a simple relation which fits a wide range of galaxies.

There has been some remarkable success in applying these concepts to the situation in the early Universe. Theory predicts that clouds in the early Universe are massive enough for the gas to cool down, but there are far too many of them compared with what we can see. This can be explained if the effect of exploding stars on small galaxies is very damaging. The number of those galaxies massive enough to retain their gas is small compared with what we see. This happened a long time ago but we still see traces today, especially thanks to the Sloan Survey where we are finding more small galaxies. This work is led today by the IoA group at Cambridge which uses star counts to detect very faint galaxies, a dozen or so of which have been found in the last few years and are likely relics of what would have been dwarf galaxies if they had been massive enough. However, even applying the supernova-feedback model does not explain the observed number fully. The theory also gives too many big galaxies as well as too many small galaxies but it does give the average mass for galaxies.

Only two timescales are involved in the fate of a galaxy — the collapse time and the cooling time, and since the cooling time is short compared to the collapse time it is a necessary condition but not a guarantee of star formation. We can relate the two timescales to a mass and this depends on α_g. In massive galaxies the SN momentum input cannot lift the gas out of the potential well so we must appeal to something else. There is a black hole at the centre of massive galaxies which accretes gas and turns into a quasar with an enormous output of energy. So we have a gas-rich galaxy forming stars, but when does the central outflow become big enough to clean up the gas and quench the star formation?

There is one more surprise, known as 'down-sizing'. It is natural for smaller galaxies to form before bigger galaxies when the Universe was denser and younger, but exactly the opposite is seen. By comparing the elements in the stellar components of SN II (Mg) with those from the ejecta of SN I (Fe) it seems that the massive galaxies formed earlier and from X-ray surveys we find that the more-luminous QSOs form before the less-luminous ones. The massive outflow from QSOs can trigger more rapid star formation by compressing the interstellar clouds.

The speaker summarized his talk by stressing that this is an immensely successful theory for large-scale structure, and there is a nice agreement between surveys and simulations; but problems still exist on small scales. This is a complex problem, dealing as it does with multi-phase outflows with perhaps a combination of the effects of supernovae and massive black holes.]

The President. Questions?

Professor Frenk. You showed one of my plots earlier on, and I want to make a clarification about it. A topic that came up this morning in the *Gaia* meeting concerned the luminosity function of satellites of the Milky Way, and you showed the exciting new results from the IoA-led group, and you had some theoretical curves; you pointed out that the curve from the work by Benson and others at Durham was in agreement with the data at small masses but disagreed with the data at large masses. However, this plot is made by observers who understand about observational uncertainties but not theoretical uncertainties. If you actually look at the original paper — which, by the way, predicted the existence of the satellites before they were discovered — there is a theoretical error bar, and the statement in that paper is that objects like the LMC are possible but rare, if you include the uncertainties. So I think the disagreement when you take into account theoretical error bars at the bright end of the standard luminosity function is only for objects like the LMC in this model; but the other model predictions seem to be right. I wonder if you could comment on this.

Professor Silk. The answer is to get samples that go well beyond the LMC in luminosity, and our Local Group in volume. Clearly, the current error bars are huge. I would think that we will soon have deeper surveys that can really go down all the way to the LMC and beyond, to much larger volumes, but we should wait for those before reading too much into the disagreement. I would agree with you, given the present error bars. I would not treat this as my highest-priority problem, I think there are others that might be more serious.

The President. Any other questions?

Dr. A. Marcolini. About the Local Group, there are a few papers recently that discovered that the least-massive galaxies in the Local Group, like Leo III or Phoenix, were able to form stars up to 100 million years ago, indicating star formation has lasted for 13 Gyr; so how can a feedback mechanism for removing the gas and stopping star formation work for these galaxies?

Professor Silk. You're touching on a very important issue, and no-one knows the answer definitively. What is clear is that all the supernova-feedback arguments I gave you have been highly criticized for the following reason: that around any particular dwarf, formed early, there's a big cloud of gas; as star formation starts and blows out the gas, it runs into other gas and it will fall back in later. No-one has really addressed this problem, studies have stopped at redshift 4 or 5, nobody has followed this down to the present time and no-one knows what happens between then and now. It may well be that star formation could continue at some low level.

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

Before we break for tea, I want to put to you a couple of consumer-satisfaction questions, to do with the extent to which you are satisfied with the RAS. Let me put two questions to you. The first question is: do you welcome the RAS's move towards greater involvement in science policy? Those who welcome this, could you please show? Those who do not welcome it? [Vote in favour.] We're going in the right direction, and one needs to know this. The second question is a bit more specific: are you broadly satisfied with what the RAS has been doing during the current funding crisis? Those broadly satisfied, please show. Those not satisfied? [Vote in favour.] Thank you very much; it's very encouraging that there is such a strong level of support. Obviously, those who are not happy should talk to us; I know what some of the grievances are. But anyway, thank you very much.

I have one announcement: the next NAM, which will be jointly with JENAM, will be at the University of Hertfordshire on 2009 April 19–24.

Summary of the RAS Discussion Meeting

ACCURATE RADIAL VELOCITIES AND THE MASSES OF STARS

This meeting, held on 2008 March 14 in the newly refurbished lecture theatre of the RAS premises in Burlington House, celebrated the remarkable contributions made to accurate radial velocities by Professor Roger Griffin, whose series in *The Observatory* reached Paper 200 in June.

By way of introduction, Professor Lynden-Bell explained that when accurate radial-velocity orbits are combined with accurate separations on the sky, it becomes possible to calculate the masses of the binary-star components. Thus, through his persistence and accuracy, Professor Griffin had laid the foundation for determining several hundred accurate stellar masses and distances. He went on to say that we were delighted to have with us Professor Hal McAlister, from whose CHARA interferometric telescope array on Mt. Wilson we hope to get the accurate angular separations required for stellar masses and distances.

Dr. David Stickland chaired the morning session on 'The masses of normal stars'. The first speaker was Dr. Pierre Maxted (Keele) who spoke on 'Testing stellar models *via* observations of binaries'. He presented several examples of recent results in binary-star research to illustrate the techniques and methods used to derive fundamental stellar parameters that can be used to test models of stars. The fraction of binary stars in a population of stars and their properties can give a powerful insight into their formation and evolution. The manner of formation of very-low-mass stars and brown dwarfs is very uncertain. High-resolution imaging has shown that there is a lack of such stars with wide orbits. A recent survey of the young stars and brown dwarfs in Orion by Maxted has now shown that there is also a lack of spectroscopic binaries among very-low-mass stars and brown dwarfs compared to more massive stars (*MNRAS*, **385**, 2210, 2008).

A recent study of the unseen companions to subdwarf-B stars by Geier (*AN*, **328**, 708, 2007) illustrates the constraints that can be placed on the properties of the companion to a single-lined spectroscopic binary. An estimate of the radius of the star combined with the mass function and projected rotational velocity suggests that the companions may be neutron stars or black holes. It is not possible in general to measure the masses of the stars in non-eclipsing double-lined spectroscopic binaries because the inclinations of the systems are not known. WD 0137–349 provides a counter-example, in which the mass of the white dwarf is known from an analysis of its spectrum, and the mass of the brown-dwarf companion can be inferred (*Nature*, **442**, 543, 2006). The mass derived is confirmed by the gravitational redshift of the white dwarf in this exceptional binary star. The most stringent tests of stellar models come from eclipsing binary stars. A recent study by Southworth *et al.* (*A&A*, **467**, 1251, 2007) of the bright eclipsing binary star β Aurigae illustrates that a precision of 1% or better in the masses and radii can be achieved. That is a nearby star, so two independent estimates of the distance confirm that it is possible to find the distances to some eclipsing binaries to an accuracy of about 2%. That will be particularly valuable in the case of the two eclipsing binaries in the cluster h Per (*MNRAS*, **349**, 547, 2004), where observations are on-going to establish the age, distance, and composition of the cluster from the properties of those binaries alone. The advent of long-baseline interferometers now makes it possible to measure masses and distances for many more binary stars. This is particularly valuable for stars such as the Wolf–Rayet star γ^2 Velorum, which forms a binary with an O-star, for which it is difficult to measure accurate masses in any other way.

Dr. Maxted was followed by Dr. David W. Latham (Harvard–Smithsonian, CfA) who spoke on 'Testing the mass–radius relation for M dwarfs'. He said that fundamental mass and radius determinations accurate to 2% or better are available from four eclipsing double-lined spectroscopic-binary M dwarfs: CM Dra, YY Gem, CU Cnc, and GU Boo. The luminosities for those stars match the predictions of stellar models, but their radii come out too large by about 10% and the effective temperatures too cool by a compensating 5%. Wide-field photometric searches for transiting planets have turned up dozens of single-lined eclipsing binaries consisting of an F- or G-star primary and an unseen M-dwarf secondary. Those systems allow the mass and radius of the M dwarf to be determined relative to the primary. If synchronization can be assumed between the rotation of the primary and the orbital period, then the observed rotational broadening of its lines can be used to set its radius, thus avoiding the need to invoke stellar models to estimate the mass of the primary. Masses and radii derived for M dwarfs that way support the results from the double-lined systems.

Professor H. A. McAlister then spoke on 'Interferometric observations of spectroscopic binaries'. He said that he was pleased and honoured to participate in this celebration of Roger Griffin's landmark 200th paper to appear in *The Observatory*. Professor Griffin had been a colleague and friend for many years, whose unrivalled productivity in the field of spectroscopic-binary stars had been an inspiration to him in his efforts to resolve some of those systems. It is a well-known fact that observations of binary stars are essentially our only means of getting stellar masses, which are the linchpin of stellar-evolutionary theory. Regrettably, no single observational technique can alone provide masses (except in the exceedingly uncommon situation wherein absolute astrometry yields the visual orbit, parallax, and mass ratio). The most common pairing of techniques applies to the case of double-lined spectroscopic binaries (SB2s) for which the inclination of the orbital plane leads to eclipses that are observed photometrically. Indeed, that approach has produced the majority of high-quality masses we now have in hand.

Another approach that has been less productive to date is the direct resolution of SB2s by astrometric techniques. In addition to yielding the individual masses, an orbital parallax falls out of the combined solution and hence we have the luminosities as well. Here, the selection effects of the complementary approaches have classically provided little overlap in accessibility, *i.e.*, spectroscopy tends to favour short periods while astrometry thrives on longer ones. Roger Griffin's revolutionary advance of photoelectric radial-velocity techniques combined with his insatiable and indefatigable desire to carry out his approach at the telescope has led to many excellent orbits with longer periods that tantalize practitioners of interferometry to add the visual orbital elements to Dr. Griffin's spectroscopic values.

Speckle interferometry showed the potential for 'bridging the period gap', and one may point to the case of the 984-day system 81 Cancri (HR 3650). The remarkable visual interferometrist W. S. Finsen discovered that binary with the Johannesburg refractor in 1959 and proceeded to measure the orbital motion with his eyepiece interferometer for over three revolutions. The angular separation in his measurements never exceeded 0·15 arcsec. Between 1976 and 1982 the Griffins obtained at Cambridge and Palomar radial-velocity observations, from which they were able to resolve the double lines at critical phases to determine the velocity amplitudes and hence the mass ratio. They published a joint solution in 1982 (*The Observatory*, **102**, 217, 1982) which challenged further speckle and spectroscopic observations to obtain accurate masses for the component stars. By the time of the Griffins' paper, the speaker's speckle-interferometry group had already resolved the system at eight epochs, and many subsequent speckle measurements have been obtained up to nearly the present time (see the *Fourth Catalog of Interferometric Measurements of Binary Stars*). The entire body of spectroscopic and interferometric data was examined and a joint solution published (*AJ*, **112**, 276, 1996) in which the masses, orbital parallaxes, and luminosities were presented. This is a very elegant example of combining precise velocities with high-angular-resolution measurements.

Of course, speckle interferometry, even when carried out at the largest available telescopes, is limited to angular separations exceeding 25 milli-arcseconds. The great majority of SB2s remain unresolved at this limit. But, with the advent of long-baseline interferometers capable of sub-milli-arcsecond resolution, the gap between the spectroscopic and 'visual' binaries is now fully bridged except for the very closest, contact systems. In his 148th paper in the series (*The Observatory*, **119**, 272, 1999), Griffin called attention to the 523-day SB2 comprising HR 7955, a bright star for which there is inexplicably no constellation designation, and noted that it should be "an easy object for optical interferometry". The star had never been included in any of the several speckle duplicity surveys, but when it was observed by Georgia State astronomy graduate student Christopher Farrington at the *CHARA Array*, he clearly saw double fringe packets that have allowed him thus far to do the orbital astrometry with modest phase coverage, finding an angular semi-major axis of 66 milli-arcseconds and an inclination of 28°. This system is not yet ripe for a complete combined orbital analysis, but it is getting there, thanks to Roger Griffin!

Interferometry has made remarkable strides during the last decade, and excellent studies of resolved SB2s have resulted from the handful of interferometers around the world. While this is by no means intended to be a review of that very productive field, two recent developments at the *CHARA Array* are worth noting. The first is the resolution of σ^2 CrB. This bright star has a beautiful double-lined orbit contributed to by several individuals and groups, most recently the as-yet-unpublished observations of G. Torres and D. Latham at the Harvard–Smithsonian Center for Astrophysics. Another CHARA graduate student, Deepak Raghavan, has carefully observed it with the longest baselines of the *CHARA Array*, which are required to resolve this 1·14-day system, which has the shortest period of any binary yet resolved interferometrically, and whose semi-major axis is only 1·2 milli-arcseconds. An even more dramatic demonstration from CHARA in collaboration with Dr. John Monnier of the University of Michigan is the recent, and unpublished, imaging at several phases of the complex binary system *β* Lyrae.

The speaker emphasized how Roger Griffin's monumentally productive career has revolutionized the way in which spectroscopic binaries are studied. His work triggered continuing developments of radial-velocity techniques that have culminated in the ever-increasing collection of extra-solar planets. His accomplishments are an inspiration and challenge to us all to use the wonderful modern tools at hand to extract many additional accurate values for that most fundamental of stellar parameters — the mass.

Professor Roger Griffin (The Observatories, Cambridge) then addressed the meeting on 'Spectroscopic binaries of long period'. First he thanked everyone for coming to the meeting and said that it was a great honour that the RAS was holding it, and that he was most grateful to Professor Lynden-Bell for making all the arrangements. He particularly thanked those speakers who had come from almost astronomical distances on purpose to be present and talk to us today. He said that in what the audience might think was a diversionary tactic designed to conceal his inability to tell them anything new, and in what they might in a minute see as a rather insensitive abuse of the occasion, he thought he would start by recalling a little about the early history of measurement of radial velocities by cross-correlation of spectra, and about the reception of that technique by the RAS. "I should say at the outset that the idea was not mine, but is to be credited to Fellgett (*Optica Acta*, **2**, 9, 1953), who described it at a conference in 1953 and was on the staff of the Cambridge Observatories when I started there as a research student more than 50 years ago. So the idea was in the air there already. It was only after I obtained a very junior position on the staff at Cambridge in 1962 that I began seriously to try to implement the technique myself. I was some way along with the development before I discussed with Professor Redman, the Director, exactly what I hoped to get out of it. I thought that, when it was persuaded to work, we ought to be able to measure with the local 36-inch telescope fifty ninth-magnitude stars to 3 kilometres per second r.m.s. per night. Redman, who did not by any means have a reputation for being much given to celebration, and who furthermore recalled from his own experience that it took two hours' exposure on the DAO 72-inch telescope to obtain a spectrum of a seventh-magnitude star that could be measured to an accuracy of 6 kilometres per second, roared with laughter and said that, if I ever did that, he would supply a magnum of champagne in celebration! Only eighteen months later I collected!

It was in February 1966 that I got the system working as well as I could reasonably hope to do with the original experimental instrument. I wrote to the RAS and offered to describe the technique at a meeting during that spring; I did not receive a response. Redman encouraged me to send the initial paper to America, and it was published in the *Astrophysical Journal*, but when subsequent papers were submitted to the *Monthly Notices* I discovered that behind the scenes there were people who could not bear to hear about cross-correlation radial velocities — though how we would ever have obtained the evidence for things as diverse as black holes and extra-terrestrial planets if we were still having to measure velocities line-by-line with a measuring machine I don't know. Some of the experts who had spent their lives measuring spectra in the old way would have liked to discredit the new way. They said that the whole process was flawed and invalid. It is true that certain spectral lines were well known by the experts to be 'bad for radial velocities', by which they meant that they were blends with non-coincident components whose relative strengths varied with spectral type and which, therefore, could not be attributed a fixed wavelength. I too knew that, but nevertheless designed the physical mask which served as the cross-correlation template to include all lines deeper than a certain limit without any regard to their origin or individual suitability. The initial paper carefully included a statistical argument showing that that did not matter when one was cross-correlating hundreds of lines simultaneously, but that argument seemed to fall on deaf ears. People also seemed incensed at the idea that you could turn the telescope to just any star, without prior knowledge of its spectral type or anything, and if you obtained any cross-correlation at all you had got the right answer!

Two of the early papers were thus held up an unconscionable time in the refereeing process. In those days the editors of the *Monthly Notices* were not empowered to reject papers submitted by Fellows — only the Council could do that. It appeared to be an embarrassment to the editors to ask the Council to do it, so before resorting to doing so they tried to browbeat me into withdrawing the papers, which I declined to do. I was quite young and I told them to go right ahead! I'd do exactly the same now! The first time, they shot themselves in the foot, because the paper was passed, at the cost of trivial amendment. I thought, therefore, that the anonymous opposition had collapsed, and indeed the next paper went through all right. But the objectors had evidently re-grouped by the time the third one went in, and after a long delay that too went to the Council for rejection. It was at the time that Hoyle was President. I am not in general an admirer of Hoyle, but I did think he handled that matter with uncommon adroitness. The week after the matter had been taken to the Council, Hoyle came to see me, without appointment, in my office — he did not send for me, he came in person to see me. He just stood in the doorway. He remarked that I seemed to be having trouble again with one of my papers. I could only agree. He explained that he had been delegated by the Council to sort the matter out. But then to my surprise he did not discuss it with me at all. All he said was, "That paper is perfectly all right, isn't it?" — and of course I answered yes, at which he simply said that he would so report to the Council and the editors, and then he forthwith turned round and left! He must have read the paper himself before he came to see me. It took about ten years, by which time the situation must have been painfully clear, before the Establishment finally conceded and embraced the cross-correlation method themselves. And after about another ten years, there was hardly anybody left who could remember how to measure velocities any other way!"

By way of illustration of the Cambridge instruments and of some results that have been accumulating on particular stars more or less throughout the 40-odd years since the development of the method, Professor Griffin showed a few slides of the original instrument and of the current one, and also some diagrams of spectroscopic orbits with periods of unusual length.

After lunch, Professor Lynden-Bell took the chair for the afternoon session devoted to 'Pulsar binaries and relativity'. As Professor Andrew Lyne was recovering from surgery, the chairman said he was most grateful to Dr. Ben Stappers (Jodrell Bank, Manchester) for agreeing to talk in his place on 'The masses of pulsars and their companions.'

The speaker said that pulsars, and in particular the so-called millisecond pulsars, are often found to be in binary systems. Their companions cover a wide range in mass and evolutionary status and include planets, brown dwarfs, white dwarfs, neutron stars, main-sequence stars, and theoretically a black hole. Determining if a pulsar is a member of a binary system requires that one can measure the pulse-arrival times with sufficient accuracy. The motion of the pulsar about the common centre of mass of the system causes a change in the pulse-arrival times, which can be likened to the changes in the line positions in a single-lined spectroscopic binary. That then allows one to measure the mass function, which in the absence of any further information on the inclination or masses is as much as we can determine.

Pulsar binaries afford a number of different ways in which the masses of both components can be determined. In orbits with two compact objects, either pulsar–white-dwarf or pulsar–neutron-star systems which have sufficiently tight orbits, it might be possible to measure the post-Keplerian parameters in the arrival times of the pulses. Under the assumption of a particular theory of gravity, measuring two of those parameters allows one to determine the two masses. Measuring a third parameter then allows one to test theories of gravity. In systems with low-mass brown dwarfs in tight orbits, the impinging pulsar wind can heat the companion, and if the brown dwarf is also distorted in shape owing to the gravitational potential, then one expects the optical flux to exhibit ellipsoidal variations which, when modelled, can be used to constrain the inclination of the system. In systems where the white-dwarf or main-sequence-star companions are sufficiently bright it may be possible to perform spectroscopy of those stars and thereby effectively make the system a double-lined spectroscopic binary. However, that is challenging, not only because most of the companions are too distant and thus faint, but because in the case of white dwarfs the large gravity broadens the lines and in the main-sequence binaries the velocity of the star is so small that measuring the radial velocity is not possible.

White dwarfs are the most common companions to radio pulsars and they fall into two broad categories, the He white dwarfs and the CO/ONeMg white dwarfs, of which there are approximately 80 and 16, respectively. The He white dwarfs typically have masses in the range of $0 \cdot 45 - 0 \cdot 6\ M_{\odot}$, while the CO/ONeMg are quite a bit more massive, up to the Chandrasekhar limit. The next-most-common companions are the brown dwarfs, although these systems are preferentially found in globular clusters. The brown-dwarf companions have very low masses lying in the range $0 \cdot 001$ to $0 \cdot 08\ M_{\odot}$. There are just four main-sequence-star companions to radio pulsars known and they are all likely to be B or Be stars and have masses in the range 3 to 11 $M_{\odot}$. The masses of pulsars and their neutron-star companions were until recently best studied in the double-neutron-star systems. In the first four best-studied of these systems it had been shown that the masses fall into a very narrow range of just $1 \cdot 35 \pm 0 \cdot 04\ M_{\odot}$. However, the discovery of three new double-neutron-star systems and improved timing of one other system show that the masses of the neutron stars in these systems can be as low as $1 \cdot 18\ M_{\odot}$ and as high as $1 \cdot 51\ M_{\odot}$, significantly broadening the range and also providing vital input for the evolutionary scenarios in these systems. A further broadening of the neutron-star-mass range seems to have come from the study of a few systems found in globular clusters. A statistical analysis seems to show that at least one of the neutron stars in those white-dwarf–neutron-star binaries must have a mass above $1 \cdot 44\ M_{\odot}$. As we discover and study better more pulsars, we find that they occupy a rich variety of binary systems. Determining the masses of the constituent parts of these systems provides valuable parameters for understanding the evolution of massive stars and binary systems. It also allows us to understand their future evolution and the potential that they have to become gravitational-wave sources in the future.

The next speaker was Dr. Michael Kramer (Jodrell Bank, Manchester), who spoke on 'Relativistic effects in pulsar binaries'. He explained that since their discovery 40 years ago, pulsars have been remarkable tools for a large variety of applications in astrophysics and fundamental physics, ranging from the study of super-dense matter to cosmology and, in particular, high-precision tests of theories of gravity. This use of pulsars is possible owing to their nature as very stable cosmic clocks and a technique known as pulsar timing. Such observations reveal a plethora of relativistic effects that can be observed in pulsar binary systems. The best example to date is the unique Double Pulsar system, where we can observe simultaneously relativistic orbital precession, the curvature of space-time, gravitational redshift, the effects of gravitational radiation, and relativistic spin precession. Comparing the theory and independent measurement of these effects to the predictions of General Relativity leads to the best test of Einstein's theory in the strong-field régime. Other pulsar observations allow strong-field tests of the Strong Equivalence Principle, Lorentz invariance, and the conservation of momentum. In the future, we expect to probe and measure the properties of black holes by using pulsars to be discovered with the *Square Kilometre Array*.

Finally, Dr. Alberto Vecchio (Birmingham) looked to the future by speaking on 'Observing gravitational waves from binary systems'. He said that the nascent field of gravitational-wave astronomy offers exciting prospects for astrophysics and cosmology because it opens a new window on the Universe, complementary to traditional observations in the electromagnetic band. In the past few years, the first generation of large gravitational-wave interferometers — *LIGO*, *Virgo*, *GEO-600*, and *Tama-300* — has come on-line at progressively higher sensitivity and duty cycle. Recently *LIGO* has completed its fifth science run (S5), recording one integrated year of data in triple coincidence between the two 4-km-arm interferometers and the 2-km-arm interferometer at design sensitivity. In addition, *GEO-600* and *Virgo* were on-line for extended periods of S5. The analysis of the data is on-going and the first direct detection of gravitational waves, although not imminent, is becoming plausible. *LIGO* and *Virgo* are now being upgraded to the so-called enhanced configuration, leading to a new science run in 2009 at a strain sensitivity about a factor of two better than the present one. The more intrusive upgrade to advanced *LIGO*/*Virgo* will increase the sensitivity in amplitude by a factor of about ten across the whole frequency band and shift the low-frequency cut-off to about 10 Hz.

It is expected that by the time advanced *LIGO*/*Virgo* operate, gravitational waves from a variety of sources will be observed and monitored in detail. Ground-based instruments will provide direct observations of a whole range of stellar-mass compact objects (neutron stars and black holes) from about 1 to 100 $M_{\odot}$ and will allow the determination of their masses with an accuracy of 10% or better, together with other important astrophysical parameters (such as spins), traditionally difficult to measure.

In parallel, ESA and NASA are vigorously pursuing the *Laser Interferometer Space Antenna* (*LISA*), a 5-million-km-arm space-borne interferometer with launch date beyond 2018. *LISA* will observe tens of thousands of sources, from Galactic sub-solar-mass binaries to very-high-redshift massive black holes. For the determination of stellar masses, *LISA* will provide the largest census of Galactic white dwarfs, a fraction of which may yield the measurement of a particular combination of their masses (the so-called 'chirp mass'). *LISA* has also the potential of observing gravitational waves emitted by degenerate stars (neutron stars and white dwarfs) captured by massive black holes in the centres of galaxies at $z < 1$; the masses of these stars could be measured at the 1% level. — DONALD LYNDEN-BELL.

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 202: 31 AND 32 CYGNI*

By R. F. Griffin
Cambridge Observatories

The canonical trio of ζ Aurigae stars consists of ζ Aur itself plus 31 and 32 Cygni. They are eclipsing systems in which late-type supergiant primaries eclipse B-type companions, and they are of pivotal importance in our understanding of stellar chromospheres. Their radial velocities have been observed on and off for more than a century, and for the last 20 years by the writer, who particularly in recent years has maintained a monthly watch on them. That has produced the surprising revelation that, in addition to their orbital motion, which has already been tolerably well documented by others, with periods well known from the eclipses to be 3784 and 1147 days, respectively, they both show systematic runs of velocity residuals, typically of the order of 300 m s^{-1}, that correspond to periodicities of a little over two years. In the case of 31 Cyg, the repeating pattern of the residuals appears to have a complex form. In 32 Cyg the pattern does not hold phase for more than a few cycles, so it must be attributed to some transient phenomenon. The same is almost certainly true of 31 Cyg, but in its case the phasing has not completely broken down in 20 years. It is tempting to suppose that the patterns indicate the rotation periods of the supergiant stars and arise from large features that may be akin to prominences on the Sun or are, in the most non-commital terms, azimuthal inequalities, which persist only for a few rotations, but

*The stars are identified in this paper, as by most recent authors, by their Flamsteed numbers rather than the Bayer letters o^1 and o^2, owing to confusion (believed, however, now to be resolved) in the literature over those designations, which used often to be (mis?)applied to Fl. 30 and 31 Cygni, respectively. See the note for HR 7730 (30 Cyg) at the back of the *Bright Star Catalogue*[1].

(in the absence of a deliberate effort at coercion of the numbers) the rotational velocities seem to be slightly too small to correspond to 2-year periods.

31 and 32 Cyg are just under 1° apart in the sky, and are astonishingly similar to one another. They seem never to have been considered as a genuine double star previously except in a forgotten paper by Tremblot, but all the evidence (radial velocity, parallax, and proper motion) is consonant with their being actually related to one another despite the projected separation at their distance (which is of the order of 400 pc) being at least 6 pc.

Introduction — the ζ Aurigae stars

The ζ Aurigae stars are composite-spectrum binary systems that consist of an evolved late-type star (a giant or supergiant) in orbit with an upper-main-sequence object, and with the additional characteristic that they are eclipsing systems. For many years there were only four known members of the class, ζ Aur itself, 31 and 32 Cyg, and VV Cep. All of them possess supergiant primaries and B-type secondaries, and have long orbital periods by the usual standards of spectroscopic binaries: the shortest is that of the type star at 972 days. The composite nature of the four systems has been known for a long time; in two cases, those of ζ Aur and 31 Cyg, it was recognized as long ago as 1897 by Miss Maury, who included those stars in the very first listing[2] of such objects. In the other two, in which the late-type component is more dominant not only at visual wavelengths but even in the violet, the presence of the hot companion was not at first noticed. It must be recalled that, a hundred years ago, the transparency of optical glass declined seriously towards short wavelengths (often to zero in the near ultraviolet). Moreover, the prismatic dispersion of spectrographs increased rapidly shortwards, spreading such light as was available there ever more widely and thinly on the plate. It thereby compounded the fall-off in that direction of the photographic density of spectrograms in just the region where the early-type component of a composite system would begin to make a substantial contribution to the total light. The composite nature of 32 Cyg appears not to have been noticed until Miss Cannon classified it from objective-prism spectra for the *Henry Draper Catalogue*[3]; she called it K0 + A3 (HD 192909 and 192910, respectively) and added at the back of the volume the terse note "The spectrum is composite". The B star in VV Cep was not discovered until 1932, in circumstances related below.

Eclipses in binaries of long period are easily overlooked, because such stars spend so much of their time at their normal magnitudes and there seems no reason to watch them carefully. Of the four ζ Aur systems mentioned, the type star was the first to be recognized as eclipsing. Harper[4], writing in 1924, had had it under regular observation for the purpose of determining its orbit. The spectrum of the late-type star was veiled in the violet by the quasi-featureless continuum of the early-type companion, but in one particular exposure the whole contrast of the spectrum seemed to be increased, because the contribution of the B star was missing. Harper noted that the spectrogram concerned was obtained at a phase when the late-type star would have been in the part of its orbit nearest the Sun, and correctly conjectured that he had seen an eclipse. He did not recognize that at the time of the observation, but only when he came to work up the material and derive the orbit, so he could not immediately verify the existence of the eclipse. The determination of the orbit allowed future eclipses to be predicted, and one was duly observed[5] the next time that it occurred at a time of year when the star was well placed for observation. What created a lot of interest in the system was the discovery[6] that the late-type component was a huge object (the total eclipse lasted 37 days) and possessed an enormous chromosphere which impressed its own absorption lines upon the light of the B-type star for several days on either side of totality, when the line of sight passed close to the limb of the primary. The situation offered a golden opportunity to obtain height-resolved information about a stellar chromosphere, and subsequent eclipses were therefore carefully watched at a number of observatories.

For several years ζ Aur was the only known system of its type, but in 1936 VV Cep was identified as a comparable system. It had been known simply as an M star, but in 1921 Adams & Joy[7] noted that the spectrum showed hydrogen emission lines, whose positions did not correspond well with those of the absorption lines. A flurry of activity concerning the star began in 1932 when Merrill *et al.*[8] included it, without any comment, in a list of Be stars, notwithstanding that, only the previous year, Merrill[9] himself had given its type as M2ep. It was McLaughlin[10] who, after starting at Michigan a systematic campaign of photometry and spectroscopy of bright irregular variables, first distinctly suggested that the M star possessed a B-type companion. At much the same time, Christie[11] (Mount Wilson) proposed from somewhat fragmentary radial-velocity data that the M star showed a period of 18–20 years and invited other observers to pool any data they might have to fill a large gap in the observations; Harper[12] indeed produced the desired complementary data and then himself[13] computed the first orbit for the system. McLaughlin next announced[14] that in 1936 the hydrogen emission lines had disappeared, and moreover that the star had been unusually faint; then Gaposchkin[15], from a comprehensive photometric study of Harvard patrol plates extending back into the 1890s, definitely established it as an eclipsing variable with a 20·4-year period. The 1939 August issue of *The Observatory* opens with a comprehensive 17-page description[16] of the VV Cep system.

McLaughlin's long-term project to monitor luminous red stars put him in a good position to notice significant changes in their spectra, and it was he who discovered the eclipsing nature of both 31 and 32 Cyg as well as that of VV Cep. On 1949 November 1 he discovered that the early-type spectrum that normally diluted the short-wavelength part of the spectrum of 32 Cyg was missing; he promptly announced[17] that fact to enable other observatories to watch what was happening, which some of them immediately *did*. The eclipse appeared to be total until November 9; after that the early-type spectrum returned, first at wavelengths where the absorption of its light by the chromosphere of the supergiant was slight, and later — many days later in some cases — at the wavelengths of the chromospheric lines themselves. Thus the great depths of the stronger metallic lines, as observed in the spectrum of the supergiant primary when it was seen by itself during the total eclipse of the B star, were maintained long after the end of the bodily eclipse by absorption in the selfsame lines of the hot star's light by the chromosphere of the supergiant. The events were very much analogous to those that had been documented in ζ Aur, but they progressed more slowly by a factor of about four, and it was not until the turn of the year that the normal composite spectrum was largely restored. Meanwhile McLaughlin had been looking carefully to see whether there were other objects that might exhibit similar phenomena, and he discovered[18] that spectra of 31 Cyg taken in 1941 July appeared to show chromospheric enhancement of the *K* line; the orbit of the system had recently been (re-)determined[19] at Lick, and McLaughlin was able to forecast the next conjunction for the end of 1951. In the event it occurred much earlier, in August to October of that year, and proved to bring an eclipse that was total for as long as 62 days, but though occurring unexpectedly early it did not catch out the Dominion Astrophysical Observatory, where observations began in very good time, in May. It seems that, under the Director, J. A. Pearce (succeeded on August 1 of that year by R. M. Petrie), practically the whole staff of the Observatory, and the observing time on the 72-inch reflector, were largely dedicated to observing 31 Cyg for the rest of that calendar year — and to good purpose, because they obtained and wrote up a comprehensive record[20–23] of the eclipse and of the chromospheric events that preceded and succeeded it.

VV Cep has considerable differences from the other three stars: its primary is an extremely luminous M supergiant which has a somewhat unstable radial velocity, variable line profiles, and varying emission lines from species such as Fe II, quite apart from the Balmer emission from its Be-type companion, whereas the other three stars have relatively well-behaved late-K primaries of very high but not so extreme luminosity, and are so similar to one another in so many respects that they have long been regarded as a class of their own.

Viewed less restrictively, the ζ Aur class includes all composite-spectrum eclipsing systems that exhibit chromospheric absorption effects just outside total eclipse, and several more such objects have been identified in recent years. None of them has such comprehensive chromospheric effects as the type star and the Cygnus pair; in several cases the late-type component is a normal giant rather than a supergiant, and its chromosphere is correspondingly compact. In those cases the chromospheric absorption enhancements are very limited both in character and duration, and major additional difficulties of interpretation arise when the scale height of the chromosphere is small in comparison with the diameter of the early-type star. The brightest of all the relatively recently discovered systems is γ Per, whose 'visual' orbit[24] held promise of an eclipse whose date (in 1990 September) was forecast quite accurately from radial velocities by the present writer. He was very fortunate in being granted at short notice the use of the Palomar 200-inch reflector in morning twilights for as long as he needed it, and obtained[25] with that instrument and its coudé spectrograph a complete spectroscopic record of the week-long eclipse on the very first occasion that it was ever witnessed. He also alerted photometrists before the event and they too obtained a full record[26] of the eclipse, which was not to be repeated at a time of year when the star would be reasonably accessible to observation until 2019. Other ζ Aur systems in which the writer has had an interest are HR 6902[27], HD 223971[28], τ Per[29], and 22 Vul[30]; in the cases of the first two of those objects, the eclipses were discovered as a direct result of the determination of their respective dates of conjunction from then-unpublished spectroscopic orbits.

31 Cygni (HR 7735, HD 192577/8)

The literature on the ζ Aur stars is so extensive that it is not possible to give more than a condensed summary in anything short of a treatise on each individual object; this paper does not set out to constitute such a treatise.

31 and 32 Cyg are in a rich Milky-Way field, and the proximity of other stars to 31 Cyg resulted in its being listed as a visual multiple star: it features as BDS 10036 in Burnham's catalogue[31] and as ADS 13554 in Aitken's[32]. Both those catalogue compilers style it as o^2 Cygni and list three companions. There is a

very faint one at a distance of 36″, a 7^m B star (HD 192579) at 107″, and the 5^m A-type star 30 Cyg (HR 7730, HD 192514; also styled o^1 Cyg by Burnham and Aitken) about 6′ distant. 30 Cyg is definitely just an optical companion; it is not immediately possible to say the same about HD 192579, whose (very small) parallax and proper motion are not distinguishable from those of 31 Cyg, or about the faint star, for which little is known.

A star as bright as 31 Cyg has naturally been measured photometrically by a considerable number of observers. There will be occasion below to refer more particularly to photometry within and outside eclipses, but for the present it is noted that representative mean values outside eclipse are $V = 3^m{\cdot}79$, $(B-V) = 1^m{\cdot}28$, $(U-B) = 0^m{\cdot}42$.

The spectrum of 31 Cyg presented difficulties to the early observers and led them into error (Miss Maury being the distinguished exception). It was first classified in the *Draper Catalogue*[33] in 1890 as "K?", before Miss Maury recognized it as composite. She noted[2] (the present writer interpolates explanatory notes in square brackets), "The spectrum of this double star resembles that of γ Andromedae [a well-known visual multiple star], which combines the features of Group XV [K0] with those of the first [early] type. In the present case, however, the images are coincident throughout. [In γ And the spectra would be mutually displaced on the objective-prism plates by the non-coincidence of their sources.] The hydrogen lines Hδ, Hε, Hζ and Hη appear as in a first-type star, and the lines Hβ and Hγ are stronger than in normal stars of Group XV. The band K is reduced to an inconspicuous haze, and in the violet region many of the fainter lines characteristic of second [late-] type stars are lost." In the *Henry Draper Catalogue*[3] the star is attributed two numbers, 192577, type K0, and 192578, B8.

After the development of criteria for estimating spectroscopic parallaxes, 31 Cyg featured in several listings where its character created confusion and/or error. In the first one, in 1917, the Mount Wilson observers (Adams *et al.*[34]) gave spectral types both 'estimated' (in the usual way, by visual comparison with standard spectra) and 'measured' (by comparison of certain Balmer lines with neighbouring metallic lines). There was a severe discrepancy between the two types: the 'estimated' one was G9 (already earlier than it ought to have been, no doubt on account of the weakening of all the late-type lines by dilution with the continuum of the hot star), while the 'measured' one was as early as G1, being grossly affected by the admixture of the strong Balmer lines of the early-type component. It is a curious thing that the Mount Wilson observers, whose spectrograms must be supposed not to have penetrated sufficiently far into the violet to enable them to recognize composite spectra, never seemed to accept Harvard recognitions of compositeness, but preferred to plough their own furrow, misleading their readers and themselves; for 31 Cyg they gave an absolute magnitude of $+1{\cdot}2$. Two years later, however, they[35] had recognized similarities between spectra of 31 Cyg (and those of 17 other high-luminosity stars that they listed) and those of Cepheids, and considered that their absolute magnitudes must all be in the range -1 to -4. After a further two years, the star appeared again in a large new listing[36] of Mount Wilson spectroscopic parallaxes, this time with an estimated type of G9p and still with a 'measured' one of G1, but the absolute magnitude had changed to $-2{\cdot}0$. Young & Harper[37], independently working from spectrograms taken at the Dominion Astrophysical Observatory, Victoria (DAO), proposed $M_V = -1^m{\cdot}0$ and $-0^m{\cdot}7$, respectively; they unaccountably called the star "R31 Cygni" as well as by its Boss[38] number, 5187. Rimmer[39], using the 12-inch refractor of the Norman Lockyer Observatory and being less easily confused than most observers, showed its absolute magnitude as $-2^m{\cdot}5$.

In a seeming reversal of the tendency to see 31 Cyg purely as a late-type star, Miss Vibert Douglas[40] listed it as having a type of B9s in a substantial investigation which, remarkably, appears to be the fruit of only a four-month appointment as 'Volunteer Research Assistant' at Yerkes in the summer of 1925, the year in which she received her PhD from McGill (she had already received the OBE for work in England in the First World War!). It seems likely that there was confusion as to the identity of the star whose spectrum she had reviewed, but there is no clue to that in her paper. In the principal table of results it is listed as Boss 5187, which is unambiguously 31 Cyg; still more clearly, in another table (Table IX) in which the consistency of the spectroscopic absolute magnitudes of the components of visual double stars is investigated, Boss 5187 is noted explicitly as being 31 Cyg, the brighter component of a double star whose secondary is listed as Boss 5186 and as 30 Cyg.

In 1935 Adams *et al.*[41] had still another bite at the same cherry when they included 31 Cyg in their final listing of spectroscopic parallaxes, this time with type cK1 and noted as composite, and with $M_V = -1^{\rm m}\cdot 8$. Hynek[42] included the star as no. 90 in his comprehensive survey of composite spectra in 1938. He noted, "The velocity is variable, the star is a giant and has the combined color of a giant K1. Per[kins] plate shows very broad and shallow *K* line with no core whatever. At 4026 and 4471 lines appear which are stronger than the K0 lines found in Arcturus, etc., and are undoubtedly the HeI lines of the B star. The early star is probably somewhat earlier than B8 since no trace of the early *K* line is seen even though the star does not have pronounced "n" characteristics [broad lines] from the HeI lines. Obviously of class I." Hynek defined his Class I as "Composite spectrum arising from a physical binary whose component stars are not resolvable even with the largest instruments, yet whose relative motion is not great enough to establish definitely the pair as a typical spectroscopic binary." As the radial velocity had been known[43] since 1901 to be variable over a considerable range, it is surprising that Hynek seems not to have believed the system to be a spectroscopic binary.

The event that really promoted 31 Cyg to stardom in the celebrity sense was McLaughlin's discovery[18] in 1950, described in the *Introduction* above, of evidence of a chromospheric *K* line on archival plates taken in the summer of 1941. That discovery may well have been facilitated by the publication[19] in 1944 from the Lick Observatory of the orbit of 31 Cyg; the orbit would have indicated the approximate dates when possible eclipse phenomena might be sought. Owing in part to a 20-day (0·5%) error in the orbital period, coupled with the early date (1919) of the specified epoch of periastron, the date that McLaughlin predicted for the ensuing conjunction (1951 December 29) was three and a half months late; but that did not catch the DAO napping, owing to the early start of the observational programme there. The observers were rewarded by documenting[20–23] in detail an eclipse that was total for two months, together with chromospheric phenomena that lasted for a comparable duration on either side of it. It may be mentioned that the intensities of the chromospheric *K* lines noted by McLaughlin from three plates obtained in 1941 were themselves misleading and could well suggest that the conjunction happened in August of that year, whereas with hindsight we know that it must have occurred at the beginning of May.

R. H. Wilson[44] considered in 1950 that he had resolved 31 Cyg visually with an eyepiece interferometer at the Flower Observatory 18-inch telescope, at angular separations given as $0''\cdot 07$ and $0''\cdot 06$ which were actually about what Miss Vinter Hansen had indicated as likely; he made quite a lengthy comment, which we refrain from reproducing here, upon the measurements and upon the system. The following year he[45] gave some more measures, of 0″·05 and even 0″·04, with a less upbeat assessment of them: "The reality of these measures would seem more questionable than those [in the earlier paper]." In a third paper[46] he allowed that the star was unresolved — at a time when it was in total eclipse! We know now that the actual separation of the components of 31 Cyg was much smaller than Wilson asserted or than could have been detected (much less measured) with the telescope at his disposal. Thirty years previously the system had appeared unresolved to Merrill[47], who used the interferometer which had measured Capella (separation ~ 0″·05) with remarkable precision, but his effort might have been made at a bad time and in any case would (as Wilson's too could be expected to) be likely to fail on account of the large Δm at visual wavelengths. It is appropriate to mention here the *caveat* provided by van den Bos, who at the conclusion of a paper[48] that assessed the reliability of interferometric observations of close binaries in general, and those by Maggini in particular, referred to lists of such observations by Wilson. He said, "In view of the numerous spurious and erroneous results contained in these lists, even for objects which should not give any trouble, I find it rather difficult to accept Wilson's discoveries and measures of pairs with distances of the order of 0″·1 or less at their face value. As in Maggini's case, rather more caution in the interpretation of the phenomena observed, or believed to be observed, would seem desirable." Evidently unabashed, Wilson was back in print in 1954, reporting[49] that 31 Cyg again appeared single, and then in 1955, when he asserted[50] that he had measured it again at a separation of 0″·04 and noted, "The present measure would seem to confirm the visual p.a. announced in AJ 55, 158, 1950."

The 1951 eclipse was well observed at Michigan (mostly by Weston on behalf of McLaughlin rather than by the latter in person) as well as at the DAO. Reporting on it afterwards, McLaughlin[51] said that his prediction of the date of conjunction was really 1951 November 29, "incorrectly stated as December 29", and that the actual date of mid-eclipse, which he put at JD 2433901·4 (1951 September 11·9) was 78 days earlier. He added that "It appears unlikely that the period of 3903 [a misprint for 3803] days could be in error by an amount sufficient to accumulate to 78 days in only about two cycles." — and he proceeded to offer a speculative alternative suggestion. Actually the period was 'off' by 19 days (seven times its stated 'probable error'), and there had been about 3·1 (not two) cycles since the original epoch, which itself had a formal standard error of 41 days. Curiously enough, the elements of an orbit by Tremblot[52] that ante-dated Miss Vinter Hansen's but seems not to have come to the attention of McLaughlin (any more than it has done to the compilers of bibliographies[53] or orbit catalogues[54–56]) would genuinely have predicted a conjunction on the exact date (1951 December 29) originally given by McLaughlin. In a separate paper[57], McLaughlin discussed the apparent radial velocities of lines in the late-type star during and outside eclipse. During the chromospheric phases, the lines were blends of the photospheric and chromospheric components, and they exhibited variations that could not be understood in terms either of the slowly changing orbital velocity or of stellar rotation, but seemed likely to indicate the existence of "prominences on a grand scale".

The comprehensive DAO investigation was presented as four papers that run consecutively in Vol. **11** of that Observatory's *Publications*. The first[20], by McKellar & Petrie, is an overall description of the campaign, and provides many of the basic data about the event. They include the duration of totality, 61·2 ± 0·5 days, and of the partial phases, 1·7 ± 0·5 days, a revised period of

3781 ± 8 days (from which they predicted 1962 January 1 as the next mid-eclipse date), spectral types of K3 Ib and B3 V with absolute magnitudes −4·0 and −2·0, respectively, and stellar radii of 174 and 4·7 $R_{\odot}$. Positional measurements of nearly 100 chromospheric lines are presented, and it is concluded (as it was by McLaughlin[57]) that their shifts have to be understood in terms of independent mass motions of large bodies of gas in the atmosphere of the K star. McKellar *et al.*[22] discussed more particularly the chromospheric Ca II *K* line, which was seen to have variable structure and even to split into two, reinforcing the idea of masses of gas with independent motions of up to 100 km s^{-1} or more. Wright & Lee[21] dealt with the light-ratio between the components as a function of wavelength, and concerned themselves with the nature of the B-type component; their work is described in more detail below. Finally, Wright[23] presented a discussion of the spectrophotometric investigation of some 400 chromospheric lines.

At about the time of the eclipse, although not prompted by it, Bidelman[58] made the first of many efforts to classify the components of 31 Cyg on the MK system: he gave the types as K2 II and B3 V. Subsequent classifications have largely agreed with those types, although there has been a tendency for the K star to be classified later and its candle-power is now regarded as rather higher than would correspond with luminosity class II. There are so many classifications that it is not profitable to recite them all here; about the furthest from Bidelman's is that given in Wright's review paper[59] in 1970, K4 Ib + B4 V, so it may be taken that all other classifications are near or within those limits. It is true that *Simbad* records one paper[60] as giving the secondary as type A3 V, but that is due to a misunderstanding over what star is regarded as the secondary: the paper concerned was evidently intending to mean 30 Cyg, but gave the *HD* number 192578, that of the B star in 31 Cyg.

The orbital elements of the late-type star yield a mass function of almost exactly 1 $M_{\odot}$, demonstrating that both components must be massive stars, as indeed would be expected from their types. The qualitative fact of the eclipses guarantees that the factor $\sin^3 i$ that always occurs in spectroscopic mass determinations is for practical purposes unity, but to assign actual masses to the components requires knowledge of the velocity amplitude of the secondary as well as of the primary. It is no easy task to measure the velocity of the relatively faint B star in the presence of the strong and complex spectrum of the late-type primary. The Balmer lines are so wide and so cut up by the superimposed lines of the primary as to be impossible to measure accurately. It could well be supposed that the most promising avenue to explore would be to extend the spectroscopy into the ultraviolet, where the relative flux of the late-type star is much smaller than in the violet/blue region. Miss Vinter Hansen[19] reported that Moore had tried to do that, but found that the transparency of the optics at Lick made it quite impossible. She noted that the 'light 1-prism' spectrograph with a camera of focal length 12 inches on the 36-inch refractor gave a good spectrum of the K star from Hβ to Hγ in a 5-minute exposure, but even 150 minutes did not produce a useable exposure below λ 3750 Å. The transmission of the object-glass[61], by itself, is already down to about 50% at Hγ and no doubt falls off increasingly rapidly shortwards, which was a major consideration in prompting the design of the *New Mills Spectrograph* to centre the plate at λ 4500 Å rather than at Hγ as in the *Original Mills*. Moreover, although Miss Vinter Hansen did not allude to the problem (and may not have been fully alerted to it), chromatic aberration of the 36-inch objective would make it very difficult to focus the star image on the spectrograph slit at short wavelengths. A correcting lens not far before the focus was used[61] to offset the chromatic aberration over the limited wavelength range $\lambda\lambda$ 4300–4600 Å, but what effect it may have had shortward of that region seems not to have been published. The focus of the 36-inch lens alone lengthens by more than 70 mm between Hγ and *H* & *K*[61]; the focal ratio being about *f*/19, if the focus were correct for Hγ the image at the *K* line would be nearly 4 mm in diameter and very little of it would pass a slit, and at still shorter wavelengths bands only a few Ångströms wide could be in acceptable focus at any one time even if means existed for determining where that focus actually *was*.

The resourceful people at the DAO, who of course started with a telescope of twice the aperture and with reflecting optics (albeit depending on coatings of silver whose reflectivity falls off badly in the ultraviolet), experimented with a quartz-prism spectrograph and with plane gratings used in conjunction with a camera with a quartz-fluorite lens. Some of the resulting spectra are illustrated in their paper[20]. But — as they lamented — even when one can see the spectrum of the B star in the ultraviolet it turns out that it does not help, because there are no measurable lines there!

Not to be baulked, Wright & Lee[21] had recourse to the subtraction procedure — a heroic effort in pre-computer days — that they[62] had developed previously to try to unravel Capella, to rid the composite spectrum of the late-type contribution, which had of course been observed alone during the time of total eclipse. Although they thereby obtained results which do indeed look like B-type spectra, it appears that they still could not be measured for radial velocity with satisfactory accuracy. In a paper[63] given by McKellar and Wright at a binary-star conference held in 1956 on their own ground at the DAO, those authors, after presenting a table showing that different determinations of the mass ratio in 31 Cyg ranged from 1·2 to 4·7, concluded by saying, "To summarize, it is our belief that the determination of the mass ratio is not in a satisfactory state for any one of the ζ Aurigae stars." Wright & Lee published in *PASP* an earlier and somewhat shorter version[64] of their paper[21] on the B-type spectrum; McKellar *et al.*, too, presented a condensed version[65] of *their* paper[22], at a conference. It may be mentioned that the author, in collaboration with R. E. M. Griffin, published[66] some time ago a spectrum of 31 Cyg B (obtained far from eclipse by the subtraction procedure) covering $\lambda\lambda$ 3850–4050 Å; the interest then was not, however, in 31 Cyg at all, whose B star was merely serving as the nearest match that could be found among available spectra for the early-type component in the composite-spectrum system HD 190361.

The 1961/2 eclipse of 31 Cyg was not widely observed spectroscopically, as it occurred at a particularly bad time of year. All the same, however, at the DAO Wright & Odgers secured a good series of ingress spectra, which they described and illustrated in a paper[67] in the *JRAS Canada* in 1962. They show the remarkable changes, not by any means constituting a monotonic increase in strength, in the chromospheric *K* line and other lines in the run-up to the eclipse.

Hack obtained a series of spectra at Merate; her immediate conclusion[68] was only that the eclipse happened about three weeks later than was predicted by McKellar & Petrie[20]. Later, however, she published[69] in collaboration with Faraggiana a more extensive account, which included the observation that, no less than 58 days after the end of totality, a new strong set of chromospheric lines appeared, at a velocity offset with respect to the stellar lines of -15 to -30 km s^{-1}. The same authors subsequently published[70] a comparative study of the K-type members of the ζ Aur and 31 and 32 Cyg systems. They noted that, whereas in ζ Aur the chromospheric lines were normally shifted to the red before totality and to the violet afterwards, at least qualitatively consonant with rotation of the chromosphere, in the Cygnus stars the lines were nearly always violet-shifted.

Despite the unfavourable circumstances of the 1961/2 eclipse (including a full moon just at the time of ingress), efforts to observe it photometrically were made at a number of observatories. That had been agreed at the 1961 meeting of the IAU, at which Wright had been named as coördinator[71]. Herczeg & Schmidt[72], at Bonn, measured eclipse depths in *V*, *B*, and *U* to be $0^{\rm m}\cdot098$, $0^{\rm m}\cdot390$, and $1^{\rm m}\cdot646$, respectively. Kwee & van Genderen[73], at Leiden, found the magnitude and colours (V, $(B-V)$, and $(U-B)$) of the primary star, when it was observable alone during totality, to be $3^{\rm m}\cdot865$, $1^{\rm m}\cdot553$, and $1^{\rm m}\cdot540$; the eclipse depths in *V* and the colours were measured as $0^{\rm m}\cdot092$, $0^{\rm m}\cdot280$, and $1^{\rm m}\cdot084$, respectively, and by subtraction of the primary's light from the total the secondary's magnitude and colours were found as $6^{\rm m}\cdot449$, $-0^{\rm m}\cdot059$, and $-0^{\rm m}\cdot559$. O'Connell[74], observing from Castel Gandolfo, obtained $3^{\rm m}\cdot893$, $1^{\rm m}\cdot568$, and $1^{\rm m}\cdot681$ for the magnitude and colours of the primary alone and $3^{\rm m}\cdot780$, $1^{\rm m}\cdot262$, and $0^{\rm m}\cdot369$ outside eclipse. Lindblad & Pipping[75] observed from Stockholm and obtained *V* and $(B-V)$ as $3^{\rm m}\cdot93$ and $1^{\rm m}\cdot55$ for the primary and $3^{\rm m}\cdot82$ and $1^{\rm m}\cdot25$ for the system as a whole. Comparison of those results involves a little arithmetic owing to the different ways in which the various authors presented them, but when that is done it will be seen that the agreement is tolerable in *V*, not very good in $(B-V)$, and really bad in $(U-B)$, where the change between in and out of eclipse was measured at less than $1^{\rm m}\cdot1$ at Leiden but more than $1^{\rm m}\cdot3$ at Castel Gandolfo. Observing conditions may have been partly responsible, but other factors could include the highly unusual distribution of flux *within* the measured bands, which would make the measurements very sensitive to the exact wavelength–transmission functions of the instruments employed. It is also a fact (clearly shown in O'Connell's graph) that in the ultraviolet the star is significantly dimmed for many days outside the actual eclipse — so the baseline in *U* must be established far from eclipse.

In 1968 Wright & Huffman[76] published a fresh discussion of the 31 Cyg system. They included a new orbit, to supersede Miss Vinter Hansen's[19]. The velocities of the early-type star were still very uncertain, but the authors considered that a mass ratio of 1·5 was likely to be nearer the mark than larger values such as had been suggested previously. On that basis they found the masses to be 9·3 and 6·2 $M_{\odot}$, with surprisingly small uncertainties; the radii, from the eclipse data, came out at 133 and 4·05 $R_{\odot}$, and the absolute magnitude of the secondary was judged from what could be seen of the hydrogen and helium lines to be $-2^{\rm m}\cdot2$; from the observed depth in *V* light of the eclipse, very close to $0^{\rm m}\cdot1$, it followed that the primary had to be $-4^{\rm m}\cdot7$. Considerations of the effective temperatures and the (quite uncertain) bolometric corrections, however, indicated radii as large as 320 and 4·8 $R_{\odot}$, which the authors were less inclined to believe than those derived more directly from the eclipses. Some of the material in the papers[72–76] described in this paragraph and the previous one was reproduced in Wright's masterly review[59] of the ζ Aur stars in *Vistas*.

Seemingly perversely, the 1972 eclipse, which was total roughly throughout the months of May and June and was therefore much more favourable to observations, was relatively neglected in comparison with the 1962 one. Landis & Williamson[77], however, reported observations made with the former's own 8-inch reflector in Georgia, USA, and found eclipse depths of $0^{\rm m}\cdot10$, $0^{\rm m}\cdot36$, and $1^{\rm m}\cdot61$ in *V*, *B*, and *U*, respectively. Hayasaki *et al.*[78] published the results of a syndicate of three Japanese observers who obtained corresponding depths of $0^{\rm m}\cdot105$, $0^{\rm m}\cdot40$, and $1^{\rm m}\cdot66$. Lovell & Hall[79] used non-standard filters with fairly narrow bands, and found depths of $0^m \cdot 034$ at Hα and $1^m \cdot 80$ at λ 3530 Å; Guinan & McCook[80] found $0^m \cdot 028$ at Hα and $0^m \cdot 052$ at Hβ.

In reporting their observations of the eclipse in 1982 September–November, Schmidtke *et al.*[81] recommended specifying timings as the moments when the transmission of the light of the secondary was 50%; such times could be more accurately defined than the actual external and internal contacts, whose significance loses definition just as the supergiant star's limb does. By that criterion, they found the 1982 eclipse to last $64 \cdot 18 \pm 0 \cdot 18$ days in *U* and $63 \cdot 00 \pm 0 \cdot 18$ days in *B*. They considered that those durations were very similar to those observed in 1962. The period was becoming very accurately determined by the successive eclipse data, at $3784 \cdot 34 \pm 0 \cdot 12$ days. The same authors, as part of a larger consortium, reproduced the same data in a later paper[82], where the 3784-day period is repeatedly mis-stated in the text to be 3794 days.

By that epoch, satellite observatories, in particular *IUE*, enabled observations in the 'space ultraviolet' to be quite routinely available, and the ζ Aur systems naturally claimed a good deal of attention. The present writer is hardly qualified to explain or even to summarize the UV observations and results, which refer to high-energy processes in what are often highly rarefied media. It is difficult to accept, however, that *IUE* was responsible for the *discovery* of chromospheric lines[83], or that they could be principally relating to a region two or three supergiant radii above the limb of the supergiant only a week outside eclipse[83], since we *know* from the duration of totality that it takes 63 days for a relative motion of (at most) two supergiant radii. The suggested picture is that the B star illuminates a large shell of material that represents the supergiant's stellar wind (it was pointed out in Paper 180 of this series[84], in the case of ζ Aur, that a rate of mass loss that may look small when expressed as 10^{-8} $M_\odot$ a year is still an enormous rate, of the order of a million million tons per second). When the B star is directly visible, that material imposes absorption lines upon its spectrum in the far UV; when it is not, it still illuminates parts of the shell that are not hidden behind the K star itself and thereby gives rise to corresponding emission lines. Also, it has been proposed[85] that the supersonic motion of the B star in the K star's wind gives rise to a shock cone which is responsible for high-excitation emission lines and leaves a turbulent wake from which wind material may accrete onto the B star.

The UV spectrum of 31 Cyg was shown in an atlas ($\lambda\lambda$ 1270 – 3190 Å), with a list of line identifications, by Bauer & Stencel[86] in 1989. (As an aside, one notices that authors and readers were better served by journals then than now: the atlas is *actually there*, printed in the *ApJ Supplements* for us to see, whereas an analogous but better atlas[87] of VV Cep, made by *HST* rather than *IUE* and 'published' under the name of the same principal author in the same journal, is not actually there — we are referred to a remote computer if we want to see it.)

Eaton[88] attempted to determine the radial-velocity amplitude of the B star, from UV photospheric lines. He derived a value of $23 \cdot 2 \pm 1 \cdot 0$ km s^{-1} by fitting his 31 radial velocities (most of which are confined to one small range of phase) to an orbit with e and ω held fixed according to the values determined by Wright & Huffman[76] for the primary star. The value of q that follows is $1 \cdot 66$, the masses $11 \cdot 7$ and $7 \cdot 1$ $M_\odot$. He found the $v \sin i$ for the B star to be 70 – 80 km s^{-1}, and also remarked, "The K components of ζ Aur binaries rotate very slowly, if at all." Eaton & Bell[89] made a major investigation, mostly with *IUE* but with a few ground-based spectra covering less than 100 Å near the *K* line, of the 1993 eclipse. An interesting observation was the temporary appearance of strong red-shifted satellite components to all the chromospheric lines at one phase of egress.

A comprehensive and systematic series of profiles of the *K* line for two months before ingress and one month after egress at the eclipse of 2003, in spectra from which the primary has been subtracted, has been presented by R. E. M. Griffin[90]; on occasion the line is seen split into as many as four or five components, which can change significantly from one day to the next.

The *Simbad* bibliography reminds us that 31 Cyg has been mentioned in no fewer than four papers already in the series of which *this* paper is a member. Paper 104[91], on the composite-spectrum object 47 Cyg, reported that that star had been found to be a much stronger radio source than 31 Cyg (it is unusual for a star to be detectable at all in radio waves). Paper 127[92], on HD 188507, remarked on the large value of $a_1 \sin i$ of that star's orbit, saying that there were only four published orbits (of which that of 31 Cyg was one) with larger ones. Paper 175[93] referred to the large mass functions of 31 Cyg and a few other stars, in connection with the huge mass function ($1 \cdot 42\ M_{\odot}$) of BD +48°1048; and finally Paper 180[84] was on ζ Aur and naturally had occasion to draw parallels with 31 Cyg.

Radial velocities and orbits for 31 Cygni

The first radial-velocity measurements of 31 Cyg were made at Lick in 1899 with the (*Original*) *Mills Spectrograph*[94], a three-prism instrument on the 36-inch refractor, giving a reciprocal dispersion of 12·5 Å mm^{-1} at Hγ. A major discordance in velocities was already apparent in 1900 and was announced by Campbell[43]. The final catalogue[61] of Lick velocities contains 18 measurements, of which the last nine, obtained between 1904 and 1923, were obtained with the *New Mills Spectrograph* (described on p. ix of the catalogue and giving 10·9 Å mm^{-1} at λ 4500 Å). Four measurements made at Bonn with a three-prism spectrograph (15·2 Å mm^{-1} at Hγ) on the 12-inch refractor in 1905/6 were published by Küstner[95]. Six made at Yerkes in 1925 – 1928 were given by Frost, Barrett & Struve[96], who noted that the star had already been found by the Lick Observatory to be a spectroscopic binary, and "Orbit not known. The period is probably long, according to unpublished results by Struve and A. Pogo." Two measures, obtained in 1921 and 1922 with a single-prism instrument (29 Å mm^{-1} at Hγ) on the 72-inch reflector, were listed in a 1934 paper[12] from the DAO by Harper.

It was at that juncture that Christie[97] produced an orbit (one of 16 'provisional orbits' given in the same short paper), after adding only three unpublished observations (much later listed by Abt[98]) from his own observatory (Mount Wilson). One has to say that the *ensemble* of velocities available to Christie looks to be a rather unpromising lot; the Lick measurements span rather more than two complete revolutions of the orbit and could be said to indicate, no matter how approximately, a possible period, but they happen to be very much bunched in phase. They seem to become more positive each time they come round, a fact remarked upon by Christie, who 'corrected' them by −0·66 km s^{-1} per thousand days. Christie was apparently unaware of (or rejected, or simply did not care to use) either the Bonn measures, which we can now see would have been helpful, or the Yerkes ones, which are quite bad and would *not*, and he was clever enough to reject one Lick measure that is easier to see as 'wild' now than it could have been then. He did well to obtain an orbit that was in principle correct although of course the elements were far from accurate. The elements, in fact, were not computed at all, but were *judged* by comparing graphs of velocities plotted *modulo* the adopted period with a set of standard curves, which is why

Christie's 16 orbits, which he freely admitted in the title of his paper to be 'provisional', are mostly specified only to 0·1 in e and 10° in ω.

Quite soon afterwards Tremblot[52] published the first computed orbit, which seems to have been very generally overlooked subsequently. He did not subscribe to the idea of a progressively changing γ-velocity. He contributed 33 of his own measurements, which were obtained with two successive 4-prism Cassegrain spectrographs on the 0·81-m telescope at Forcalquier (as the Haute-Provence observatory was known at that time). The first instrument gave a reciprocal dispersion of 22, the second 16, Å mm^{-1} at the K line. His measurements were made in four successive summers, 1936 – 1939. The nominal year of publication of his paper was 1938, but it is clear from the inclusion of measurements made as late as 1939 August that its actual publication was substantially delayed; it was received in Cambridge on 1940 May 18, and it seems remarkable that it could have been published and distributed abroad at all at that time, just when France was being overrun by the German army. Although Tremblot listed his observations individually, he condensed them into seasonal means for the purposes of calculating the orbit and of plotting the corresponding graph. The observations fall in a very useful range of phase vis-à-vis the Lick data, and the resulting orbit actually has elements extraordinarily close to those that are concluded from the present paper. Tremblot thought that he could measure the Hε line in the B star on certain of his spectrograms, and suggested a mass ratio of 2·4.

There ensued the orbit[19] by Miss Vinter Hansen, who in principle was on the staff of the Copenhagen Observatory but had started on a world tour in 1939 and been marooned in the United States by the War; she found refuge at the Lick Observatory, and one of her activities there was to measure the 75 plates of 31 Cyg that had accumulated and to derive an orbit from them. She included in her measurements the plates that had already featured in the Campbell & Moore catalogue[61], all except for the 'wild' one, which she discreetly omitted from the list altogether, and one other plate (1904 August 1) which perhaps was not to be found. She did not include any of the published velocities in her calculation, only the 75 Lick ones that she had measured for herself, and to reduce the computational task she condensed them into 35 'normal places' for the purposes of the orbital solution. Her re-measurement of the early plates largely removes the apparent drift of the γ-velocity that Christie had noticed in the published[61] Lick velocities: in the new listing the nine *Original Mills* plates are attributed velocities that are on average 1·9 km s^{-1} higher than those in the catalogue. There is a very significant discrepancy between the eccentricity of the orbit found by Miss Vinter Hansen (0·131, with a 'probable error' of only 0·006) and the values (above 0·2) found in subsequent investigations. To check up on that point, the Lick velocities have been transcribed* and the orbit solved anew with all the 75 measurements utilized individually and equally weighted. The elements come out negligibly different from those found from the 'normal points', but their standard deviations are all about twice the 'probable errors' reported by Miss Vinter Hansen, whereas they ought to be expected to be very nearly 1·5 times as great. The r.m.s. residual of the Lick velocities from the orbit calculated from them alone is 0·93 km s^{-1}. From a plenary solution that includes the writer's observations their residuals are about 1·25 km s^{-1}. Since the eccentricity given by the Lick-only orbit differs from what seems likely to be the true value by some six times its

*It is noticed that one of the observations (JD 2426544·35, 1931 July 21·85) appears to have been made within a few minutes of local noon at Lick — no doubt a result of the confusion that is caused and so often leads to error by the use of Julian dates that are half a day out of step with the actual time.

computed standard deviation, it is clear that there are non-statistical errors in the Lick velocities; indeed, certain observing seasons are characterized by systematic runs of residuals, so it is not surprising that the velocities appear somewhat worse when viewed in a wider context. The systematic discrepancies are commented upon by Wright & Huffman[76] in their own subsequent re-determination of the orbit from DAO observations. Their Fig. 4 actually plots the Lick velocities together with the DAO velocity curve and illustrates the discrepancies very clearly*.

In the aftermath of the eclipses of 1951 and 1961/2 there was a welter of publications, not a few of which have been cited above, giving radial-velocity measurements of 31 Cyg. They were, however, directed in the main towards elucidation of the chromospheric effects, so they were mostly obtained at times when the stellar lines were, or may have been, blended with chromospheric components, and are thereby disqualified from contributing to a solution of the orbit. Observations were continued at all phases at the DAO, and in 1968 Wright & Huffman[76] presented them, together with 15 velocities contributed by Popper from Mount Wilson, in a fresh solution of the orbit. The elements that they found were repeated in his 1970 review by Wright[59]. Wright & Huffman were inclined to attribute the discrepancies between the Lick and DAO observations to random motions in the atmosphere of the K star. Although it is certainly a fact that highly luminous stars do exhibit such motions on a scale large enough to make noticeable changes to the radial velocities measured from the integrated light of the hemisphere that is observed, such motions in the case of 31 Cyg do not seem large enough to account for Lick–DAO discrepancies, which the present author would attribute mainly to seasonal systematic errors in some of the Lick velocities. The basis for that judgement is that although, as will become apparent below, there are runs of residuals in the author's own measurements, they are substantially smaller than those at issue between Lick and the DAO. Moreover, the orbital elements found by the author are in good accord with those obtained at the DAO, and, as Wright & Huffman's Fig. 4 makes clear, quite modest systematic errors in a minority of seasons would be enough to explain the difference of the Lick elements.

Three radial velocities were published from the Fick Observatory of the Iowa State University at Ames by Beavers & Eitter[99]; they were obtained with those authors' radial-velocity spectrometer[100] at the coudé focus of the 24-inch *Mather* reflector.

Just recently, Eaton & Williamson[101] have described the 2-m automatic spectroscopic telescope that they have put into operation in Arizona. It appears to have been in operation for about two years; there is a long list of bright stars that have been observed with it, mostly with a view to seeing whether their velocities exhibit measurable 'jitter' but also of course to demonstrate any orbital motion that there may be. They report as many as 191 measurements of 31 Cyg already, when it appears that the total duration of their observations can be only a small fraction of the orbital period. The star features in a table of new orbital elements; the numerical values of the elements are, however, enclosed in parentheses — indicating that they are taken from a different paper (in this case one that is listed as 'in preparation') — with the one exception of the γ-velocity, for which they give $-7 \cdot 65$ km s^{-1}. They quote a standard deviation of $0 \cdot 27$ km s^{-1}, per observation, from the fit to the orbit.

*In their commentary, however, one should read 'superior conjunction' where they write 'descending node', and in the next line 'higher eccentricity for the Victoria observations' where they refer to 'lower eccentricity'.

31 Cyg was placed on the Cambridge radial-velocity programme in 1988, after R. E. M. Griffin and the writer found what beautiful results could be obtained by the former's spectral-subtraction skills when applied to the isolation of the chromospheric spectrum[29] of ζ Aur. The intention was formed of investigating other such objects, and a corollary was that improved orbits should be obtained. The system has been observed round almost two complete cycles; the total number of radial-velocity measurements is 144, of which 15 were made with the original spectrometer at Cambridge, 39 with the Haute-Provence (OHP) *Coravel*, 89 with the relatively new Cambridge *Coravel*, and one with the spectrometer at the DAO 48-inch coudé. The data are listed in Table I. As usual, the OHP velocities have been adjusted by $+0{\cdot}8$ km s^{-1}; to obtain agreement in the mean, the Cambridge *Coravel* ones have been increased by $0{\cdot}3$ km s^{-1} from their 'as initially reduced' values. In solving the orbit, it has been found appropriate to weight the velocities obtained with the original spectrometer $^1/_6$ and those from OHP $0{\cdot}4$. The single DAO measure has been treated like the OHP ones; unit weight is attributed to the recent Cambridge measures.

An initial solution produced an orbit with a period of 3771 ± 4 days; the true period is accurately determined from the eclipses at $3784{\cdot}3$ days. The reason that the eclipse period is so much more accurate than the spectroscopic one is, of course, that whereas it takes the radial velocity five years to change slowly from one extreme value to the other, the magnitude changes go through their whole range in two days and thereby have a thousandfold higher timing sensitivity — so the spectroscopist need feel no embarrassment if he cares to profit from the photometrically determined period! Even so, at first sight it *is* embarrassing that the spectroscopic value is more than three standard deviations adrift. When one looks more closely, however, it is not entirely surprising, because there are small but quite long-term trends in the residuals from the orbit, as discussed below, and they could certainly impair the determination of the period. In the same way, they no doubt make all the elements somewhat less certain than they are made to appear by their formal standard deviations.

The author considered whether to include some or all of the unusually rich lode of literature velocities in the solution of the orbit. The inclusion of the data right back to the 19th Century seemed to offer the likelihood of an improved determination of the period, but even *that* proved largely illusory. Obviously the addition of extra data, appropriately weighted, must in principle improve any determination of anything, but in practice that can only be true if the addition is homogeneous with the original basis. In the present instance, the addition of an enormous number of older data, at appropriate weighting (which turned out to be very small) to equalize the variances, made very little difference either to the orbital elements or to their standard errors. The decision was accordingly easily made — though in the opposite sense to the one usually made in this series of papers — not to utilize any of the published observations at all but to present a completely independent orbit on the basis of the author's own observations in Table I alone. The only use that has been made of extraneous information is that the period has been fixed at the known value of $3784{\cdot}3$ days. The data and the computed velocity curve are illustrated in Fig. 1, and the elements are as follows:

$P = 3784{\cdot}3$ days (fixed)	$(T)_2 = \text{MJD } 52345 \pm 9$
$\gamma = -6{\cdot}421 \pm 0{\cdot}034$ km s^{-1}	$a_1 \sin i = 709{\cdot}3 \pm 2{\cdot}3$ Gm
$K = 13{\cdot}94 \pm 0{\cdot}04$ km s^{-1}	$f(m) = 0{\cdot}995 \pm 0{\cdot}010\, M_\odot$
$e = 0{\cdot}2084 \pm 0{\cdot}0031$	
$\omega = 204{\cdot}5 \pm 1{\cdot}0$ degrees	R.m.s. residual (wt. 1) $= 0{\cdot}32$ km s^{-1}

TABLE I

Radial-velocity observations of 31 Cygni

Except as noted, the sources of the observations are as follows:
1988–1998 — Haute-Provence Coravel (weighted 0·4 in orbital solution);
1999–2008 — Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
1988 Nov. 6·87	47471·87	−5·9	0·712	+0·6
Dec. 28·72*	523·72	−7·6	·726	−0·1
1989 Jan. 12·74*	47538·74	−5·8	0·730	+2·0
Mar. 26·20	611·20	−8·8	·749	+0·5
Apr. 28·16	644·16	−9·8	·758	+0·2
May 31·08*	677·08	−9·6	·766	+1·1
June 26·00*	703·00	−10·3	·773	+0·9
Aug. 5·95*	743·95	−12·3	·784	−0·2
Sept. 6·93*	775·93	−13·4	·793	−0·5
Oct. 27·85	826·85	−14·0	·806	0·0
Nov. 25·73*	855·73	−14·0	·814	+0·6
1990 Jan. 30·73	47921·73	−16·0	0·831	+0·1
Apr. 5·17*	986·17	−18·6	·848	−1·1
May 27·10*	48038·10	−18·7	·862	−0·1
July 6·03*	078·03	−19·0	·872	+0·4
Oct. 6·89*	170·89	−21·9	·897	−0·8
Nov. 5·79*	200·79	−21·2	·905	+0·3
Dec. 19·71*	244·71	−22·4	·916	−0·3
1991 Jan. 27·73	48283·73	−22·9	0·927	−0·4
Feb. 4·25	291·25	−22·6	·929	0·0
June 14·06*	421·06	−22·2	·963	+0·8
July 21·04*	458·04	−23·1	·973	−0·3
Oct. 28·88	557·88	−21·9	·999	−0·1
Dec. 16·74	606·74	−20·7	1·012	+0·3
1992 Jan. 14·71	48635·71	−19·7	1·020	+0·7
Feb. 27·58†	679·58	−19·4	·031	0·0
Apr. 26·16	738·16	−17·8	·047	+0·1
June 21·05	794·05	−16·1	·062	+0·3
Aug. 13·96	847·96	−14·9	·076	−0·1
Dec. 20·74	976·74	−11·7	·110	−0·9
1993 Feb. 12·24	49030·24	−10·0	1·124	−0·8
19·23	037·23	−9·8	·126	−0·8
Mar. 18·21	064·21	−9·3	·133	−1·0
20·20	066·20	−9·2	·134	−1·0
23·14	069·14	−8·9	·134	−0·8
25·13	071·13	−9·2	·135	−1·1
July 8·14	176·14	−5·6	·163	−0·4
Sept. 11·99	241·99	−3·7	·180	0·0
Dec. 26·77	347·77	−0·3	·208	+1·1
1994 Jan. 9·73	49361·73	−1·1	1·212	0·0
Feb. 19·24	402·24	−0·2	·222	+0·2
May 3·16	475·16	+1·5	·242	+0·7
Aug. 2·97	566·97	+2·6	·266	+0·6
Dec. 10·73	696·73	+3·6	·300	+0·2

TABLE I (*continued*)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O–C) km s⁻¹*
1995 Jan. 2·75	49719·75	+3·9	1·306	+0·4
June 5·13	873·13	+4·2	·347	−0·3
Dec. 23·71	50074·71	+4·3	·400	−0·6
1996 Mar. 31·17	50173·17	+4·6	1·426	−0·2
Nov. 27·75‡	414·75	+3·5	·490	−0·4
Dec. 25·70	442·70	+3·6	·497	−0·1
1997 Jan. 24·72	50472·72	+3·6	1·505	+0·1
Mar. 1·25‡	508·25	+3·6	·515	+0·4
Apr. 1·20‡	539·20	+3·4	·523	+0·4
May 1·16‡	569·16	+3·4	·531	+0·6
June 17·08‡	616·08	+2·7	·543	+0·3
July 20·00	649·00	+2·2	·552	+0·1
Sept. 8·96	699·96	+1·2	·565	−0·4
Dec. 22·73	804·73	+0·3	·593	−0·1
1998 May 3·13	50936·13	−1·4	1·628	−0·1
July 8·06	51002·06	−2·4	·645	−0·2
1999 Dec. 28·69	51540·69	−12·0	1·787	+0·4
2000 Jan. 9·70	51552·70	−12·4	1·791	+0·3
Apr. 10·18	644·18	−15·0	·815	−0·3
May 20·12	684·12	−15·6	·825	0·0
June 18·11	713·11	−16·4	·833	−0·1
July 16·90	741·90	−17·5	·841	−0·6
Aug. 28·92	784·92	−17·9	·852	0·0
Sept. 20·88	807·88	−18·6	·858	−0·3
Oct. 13·83	830·83	−18·9	·864	−0·1
Nov. 13·77	861·77	−19·5	·872	−0·1
Dec. 3·70	881·70	−19·7	·878	+0·1
2001 Jan. 7·71	51916·71	−20·5	1·887	−0·1
Mar. 3·25	971·25	−22·2	·901	−0·9
May 12·14	52041·14	−22·4	·920	−0·2
June 8·11	068·11	−22·5	·927	0·0
July 4·12	094·12	−22·3	·934	+0·4
Aug. 13·97	134·97	−22·7	·944	+0·2
Sept. 25·88	177·88	−22·8	·956	+0·2
Oct. 18·87	200·87	−23·0	·962	0·0
Nov. 13·82	226·82	−22·5	·969	+0·4
22·79	235·79	−22·9	·971	0·0
Dec. 14·77	257·77	−22·3	·977	+0·4
2002 Jan. 19·73	52293·73	−22·4	1·986	0·0
Feb. 27·24	332·24	−22·0	·997	−0·1
Mar. 27·20	360·20	−21·5	2·004	0·0
Apr. 20·17	384·17	−21·3	·010	−0·2
May 16·14	410·14	−20·6	·017	0·0
June 23·10	448·10	−19·6	·027	+0·2
July 15·04	470·04	−18·7	·033	+0·6
Aug. 15·03	501·03	−18·4	·041	+0·1
Sept. 9·00	526·00	−17·5	·048	+0·3
Oct. 3·88	550·88	−17·3	·054	−0·1
Nov. 7·84	585·84	−16·5	·064	−0·4
9·69	617·69	−15·1	·072	+0·1

TABLE I (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)* *km s⁻¹*
2003 Jan. 7·73	52646·73	−14·9	2·080	−0·6
Feb. 21·25	691·25	−13·6	·091	−0·6
Mar. 19·18	717·18	−12·4	·098	−0·2
Apr. 7·19	736·19	−11·7	·103	−0·1
May 6·13	765·13	−10·8	·111	−0·1
29·09	788·09	−10·0	·117	0·0
June 11·10	801·10	−9·6	·120	0·0
28·10	818·10	−9·1	·125	0·0
July 16·04	836·04	−8·6	·130	0·0
Aug. 4·01	855·01	−7·5	·135	+0·6
Sept. 22·93	904·93	−6·4	·148	+0·3
Oct. 18·92	930·92	−5·7	·155	+0·3
Nov. 26·87	969·87	−4·8	·165	+0·2
2004 Jan. 9·74	53013·74	−3·6	2·177	+0·3
Apr. 23·16	118·16	−1·6	·204	+0·1
May 24·12	149·12	−0·9	·212	+0·2
June 15·11	171·11	−0·5	·218	+0·2
Aug. 8·08	225·08	+0·4	·233	+0·2
Sept. 2·00	250·00	+0·7	·239	+0·1
Oct. 18·91	296·91	+1·8	·251	+0·5
Nov. 26·76	335·76	+2·1	·262	+0·3
Dec. 16·69	355·69	+2·9	·267	+0·8
2005 Jan. 8·70	53378·70	+2·8	2·273	+0·4
May 15·14	505·14	+3·5	·307	−0·1
June 11·11	532·11	+3·4	·314	−0·4
July 17·08	568·08	+3·9	·323	−0·1
Aug. 15·06	597·06	+3·8	·331	−0·4
Sept. 12·98	625·98	+3·8	·338	−0·5
Oct. 25·90	668·90	+4·0	·350	−0·5
Nov. 18·82	692·82	+4·1	·356	−0·5
Dec. 11·69	715·69	+4·0	·362	−0·7
2006 Apr. 9·15	53834·15	+4·5	2·393	−0·4
May 11·11	866·11	+4·7	·402	−0·2
June 28·01	914·01	+4·8	·415	0·0
July 21·11	937·11	+5·0	·421	+0·2
Aug. 15·99	962·99	+5·0	·427	+0·2
Sept. 10·97	988·97	+5·0	·434	+0·3
Oct. 24·91	54032·91	+4·4	·446	−0·2
Nov. 25·75	064·75	+4·7	·454	+0·2
Dec. 17·69	086·69	+4·3	·460	−0·1
2007 Jan. 14·71	54114·71	+4·4	2·468	+0·1
May 2·13	222·13	+3·5	·496	−0·2
June 1·12	252·12	+3·3	·504	−0·2
July 8·12	289·12	+3·2	·514	−0·1
Aug. 3·05	315·05	+3·3	·521	+0·2
Sept. 8·02	351·02	+2·9	·530	+0·1
Oct. 4·95	377·95	+2·7	·537	+0·1
Nov. 16·79	420·79	+2·0	·548	−0·2
Dec. 12·78	446·78	+1·7	·555	−0·2
2008 Jan. 24·73	54489·73	+0·9	2·567	−0·6

*Observed with original Cambridge spectrometer; wt. 1/6.
†Observed with DAO 48-inch telescope; wt. 0·4.
‡Observed with Cambridge *Coravel*.

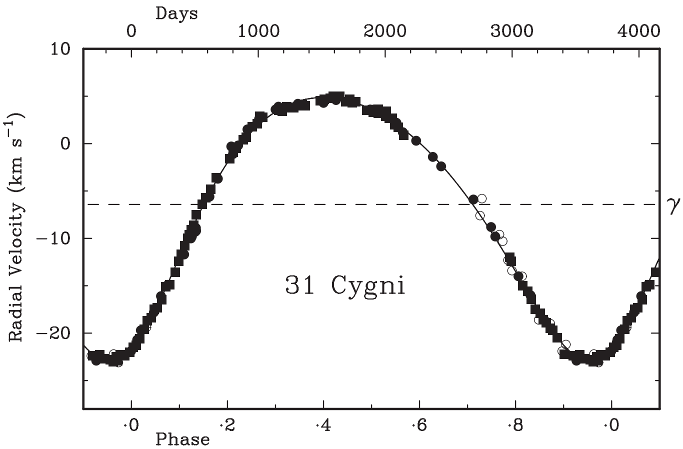

FIG. 1

The observed radial velocities of 31 Cygni plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. Open circles are used to plot observations made with the original spectrometer at Cambridge; filled circles represent the Haute-Provence *Coravel*, and filled squares refer to the Cambridge *Coravel*. There is one observation that was made at the DAO, but its symbol is completely hidden.

To enable comparison to be made with the previous orbits, Table II reproduces the elements given by Christie[97], Tremblot[52], Vinter Hansen[19], and Wright & Huffman[76], and those promised by Eaton & Williamson[101].

TABLE II

Previously published orbital elements for 31 Cygni

Except in the last column, the quoted uncertainties are 'probable errors'

Element	*Christie*[97]	*Tremblot*[52]	*Vinter Hansen*[19]	*Wright et al.*[76]	*Eaton & W'mson*[101]
P (days)	4000	3800	3802·8 ± 2·7	(3784·3)	(3784·34)
T (MJD)	14400	14420	22065 ± 28	37170 ± 28	52325·7
γ (km s^{-1})	var?	−7·4	−6·87	−7·73 ± 0·09	−7·65 ± 0·04
K (km s^{-1})	20·0	15·0	13·78 ± 0·08	13·98 ± 0·13	13·98
e	0·3	0·18	0·131 ± 0·006	0·222 ± 0·008	0·228
ω (degrees)	200	195	209·6 ± 2·6	201·1 ± 2·4	201·4
$a_1 \sin i$ (Gm)	—	771	720	709	—
$f(m)$ ($M_\odot$)	—	1·26	1·007	—	—

It is quite noticeable in Fig. 1 that the trend of the points does not follow very accurately a smooth velocity curve but tends to swerve from side to side on a time-scale of something like a tenth of the orbital period. Particularly vexing is the way in which the points miss the topmost part of the velocity curve.

The residuals shown in Table I are plotted directly against time in Fig. 2a. That diagram shows only the measurements made since the Cambridge *Coravel* was commissioned in late 1999. No one could well claim that the residuals appear randomly distributed — there is a clear periodicity of about 2½ years, not

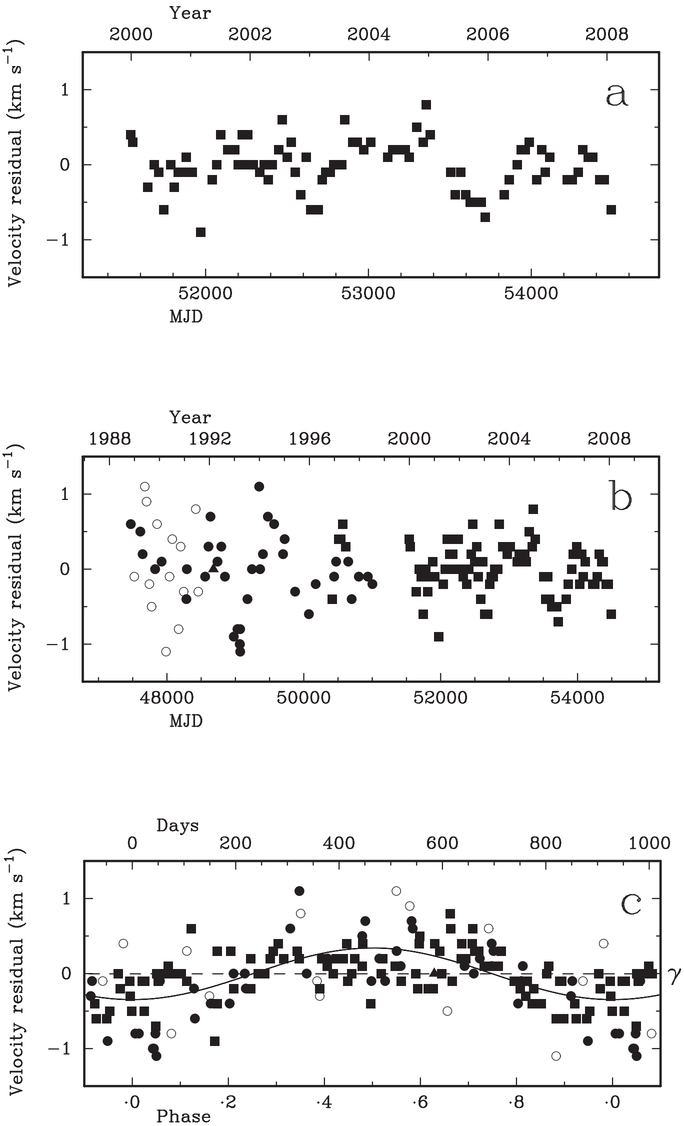

FIG. 2

Graphs showing the velocity residuals of 31 Cygni from the orbit deduced in this paper and illustrated in Fig. 1. Panel **a** plots only the measurements made with the Cambridge *Coravel*; they appear to show a repeating double-humped pattern with a periodicity of about 2¹/2 years. Panel **b** is an analogous plot but covers the whole duration of the author's observations; a variation with much the same time-scale appears to be recognizable throughout the interval covered. In panel **c** the same observations have been treated as if for an orbit; the plot tends to confirm that there is quite a significant repeating variation with a period of a little over 900 days. The different symbols carry the same meanings as in Fig. 1; the single DAO observation is represented by a filled triangle.

sinusoidal but having an apparent minor secondary dip somewhat after the mid-point between the principal minima. The most conspicuous minimum, around the year 2006·0, is the one that leads to what was characterized above as the 'vexing' swerve near the maximum velocity (ascending node) in Fig. 1.

Fig. 2b plots all of the residuals in Table I directly against time, increasing the time base by a factor of about $2^1/_2$ in comparison with Fig. 2a. In it, the Cambridge (post-2000) data appear rather squashed horizontally and it is less easy to see the character of the variation, but the fact of a quasi-periodic variation is traceable throughout the 20-year time span, with indeed a strong indication even of increased amplitude at one stage. The phasing of the variation holds up well enough throughout the duration of the observing campaign that it is plausible to run an 'orbital' solution on the residuals. The points have been fitted to a sine curve, but the other elements of the 'orbit' (period, amplitude, and phasing) were left as disposable parameters. (So was the zero-point, but that necessarily came very close to zero since the input data were already residuals.) The period was found to be 929 ± 9 days, an epoch of maximum velocity MJD 51348 ± 20, and the amplitude 0·34 ± 0·04 km s^{-1}. The solution is illustrated in Fig. 2c, which is plotted with the maximum at phase ·5, inverting the usual convention, because that way the saddle-backed nature of the velocity maximum is more easily seen than if that feature were divided between the two ends of the plot. It is clear not only that a systematic variation does exist, but also that a pure sine wave does not do justice to its character. To go into further detail, however, would (as so often happens) require more and better data: it needs someone to make measurements at a frequency of about once a week with one of the new generation of spectrometers that are accurate almost to the metre-per-second level, someone who is sufficiently young and/or influential to ensure that the measurements are maintained for 20 – 50 years or more. It would be desirable to obtain photometry, too, with the same frequency, since plausible mechanisms for producing velocity offsets mostly involve photometric consequences too. The present data have uncovered an interesting and unexpected problem and demonstrate its nature, but they were not designed to solve it and are not adequate to do so.

Discussion of the non-random nature of the residuals of the 31 Cyg radial velocities is postponed until the corresponding stage of the discussion of 32 Cyg is reached.

32 Cygni (*HR 7751, HD 192909/10*)

Representative mean magnitudes of 32 Cyg outside eclipse are $V = 3^m{\cdot}98$, $(B-V) = 1^m{\cdot}52$, $(U-B) = 1^m{\cdot}03$. They show that the star is slightly fainter than 31 Cyg in V and progressively more so at the shorter wavelengths, not only because the primary is slightly redder but mainly owing to the relatively smaller contribution from the early-type secondary. It is therefore, perhaps, somewhat surprising that the first classifications of the spectra put 32 Cyg a little earlier than 31: in the *Draper Catalogue*[33] 32 is called "H?", against 31's "K?", and Miss Maury[102], who did not recognize it as composite, put it between Classes XIV and XV, an intermediate class whose type star is κ Gem (modern type G8 III). Miss Maury added a note to its classification, but only to say, "In one of the photographs the line K appears weak. The plates are very poor." It is difficult to read any significance into the weakness of the K line; in an eclipse observation it ought to appear somewhat strengthened. The composite nature of the spectrum was noted in the *Henry Draper Catalogue*[3] by Miss Cannon, who numbered the component stars separately and listed them as nos. 192909, type K0, and 192910, A3.

32 Cyg is absent from the early lists from Mount Wilson of spectroscopic parallaxes and radial velocities, but in the 1920s it featured in the work at the DAO by Young & Harper[37], who made independent determinations of its absolute magnitude as $-0 \cdot 8$ and $-0 \cdot 6$, respectively, and classified it as late as K7, and in that at the Norman Lockyer Observatory by Rimmer[39], who gave its M_V as $-1 \cdot 7$. The Mount Wilson observers did include it in their last (1935) list[41], where they gave it as $-2^{m} \cdot 5$, cK5. It is that cK5 type that is listed for 32 Cyg by Hynek[42] in his catalogue of composite-spectrum objects, in which the system is numbered 91 and assigned to Class II ("Spectroscopic binary showing two spectra displaced relatively to each other"), although since no measurement of the spectrum of the hot star had been made the displacement must have been inferred from the variability of the radial velocity of the cool one. Exactly the same thing could, however, have been said about 31 Cyg too, although in that case the system was placed in Class I, as mentioned above.

Hynek made the following note concerning 32 Cyg: "A well-known long period spectroscopic binary whose composite spectrum is very similar to that of 31 Cyg … If HeI lines are present they are much weaker than in 31 Cyg. Probably the early star is correctly classed as A3 [its type in the *HD*]. In this case, however, the complete absence of an early K line is very striking and must mean that the lines of the early star are greatly broadened by axial rotation …". We now realize that the absence of a *K* line from the early-type star was largely because that star is faint in comparison with the supergiant, and is of much earlier type than Hynek supposed and accordingly its *K* line is intrinsically weak.

Among classifications made since the widespread adoption of the MK system, there is general agreement that the late-type component of 32 Cyg is a late-K supergiant whose luminosity class is Ib–II or thereabouts, but its type has been variously given between K3 and K5; the relative weakness of the hot star in comparison with that in 31 Cyg has resulted in large discrepancies between different classifications, all the way from B1 to B9 and even to undivided A. The first MK classification appears to be that of Bidelman[58], who listed it as K3 Ib–II + A or B, but later[103] he omitted the possibility of the hot star being of A type and gave the types as K3 Ib–II + B, and later still he[104] appeared to be putting his *imprimatur* to Markowitz' classification[105] of K3 II + B9:V. Slettebak[106] quoted Keenan as saying that the primary is of type K5 Ib–II. Other classifications are mentioned below in connection with investigations of the eclipses, but it is sometimes difficult to know whether authors are quoting types that they have seen in the literature or are giving newly considered ones of their own.

Although the hot star in 32 Cyg is so much more difficult to see than that in its neighbour 31 Cyg, the orbit of the late-type component of the former was determined long before that of the latter, no doubt because its period is much shorter. The orbit of 32 Cyg was initially determined in 1918 by Cannon[107], who found the period to be 1170 days on the basis of no fewer than 117 plates taken at Ottawa during the preceding four years, with the assistance of a few earlier ones for which velocities had been published by others. An idiosyncrasy of Cannon's paper is that it constantly (25 times!) refers to its subject as θ^2 Cygni. The paper was published before the relevant volume of the *Henry Draper Catalogue*[3], so the existence of an early-type contribution in the spectrum of 32 Cyg had not then been recognized; not only was it overlooked on the Ottawa plates, but one of those (that of 1915 April 14) was taken close to what we now know must have been a mid-eclipse epoch and evidently did not appear noticeably different from the rest.

There was then a lull in the interest in 32 Cyg until Harper[108] made a brief mention of it in a 1935 paper (where, no doubt following Cannon's example, he too called it θ^2 Cygni). He gave three DAO radial velocities of the star and said that they did not agree with Cannon's orbit; he remarked that the last of the three would give a residual of 17 km s^{-1} from the published velocity curve. It may be mentioned that his own three velocities agree almost perfectly (r.m.s. residual 0·6 km s^{-1}) with the orbit found in the present paper. The writer sees Harper's statement, "The spectrum is complex according to Harvard", as demonstrating clearly that Harper himself saw no evidence whatever in the DAO spectra of an early-type component even though he was well aware of its asserted existence.

Just as in the case of 31 Cyg, the decisive event in the observational history of 32 Cyg was the discovery by McLaughlin — in this case on 1949 November 1 — of an eclipse. The prompt communication[17] of that discovery by means of a *Harvard Announcement Card* said that spectrograms taken on November 1 and 4 "show that the early-type component of this well-known composite spectrum has disappeared. If the disappearance represents an eclipse, as seems probable, the period of 1170 days determined at Ottawa needs to be corrected to about 1140 days. ... The corresponding phase was missed in 1946, but a plate taken on 1943 July 31 also lacks the violet continuum of the early-type star." A shortly-ensuing *Card*[109] announced that spectrograms taken on 1949 November 14 had shown conspicuous atmospheric-eclipse effects similar to those seen in ζ Aur at fourth contact; the previous plate, on November 10, had shown the K-type spectrum alone, as first observed on November 1. Hard on its heels came a third *Card*[110] reporting that Hoffleit had examined a lot of Harvard objective-prism spectra "taken for the most part between December 1899 and June 1913" at the very low dispersion of 400 Å mm^{-1}, and had identified two dates — perversely enough well outside the interval that had just been cited, in 1890 and 1896 — when the system appeared to be in eclipse. They constrained the period to be within the range 1140·5 and 1141·0 days. The assessment must, however, have been illusory, because the period subsequently became established to be near 1147 days, leaving the 1890s dates several months adrift.

McLaughlin[111] soon wrote an account of the changes that he had seen in the spectrum of 32 Cyg from the time of its total eclipse until the chromospheric effects had completely subsided, which was not until the end of the calendar year, nearly two months later. His series of plates was taken with a two-prism spectrograph with a 12-inch camera giving reciprocal dispersions of 38 Å mm^{-1} at Hγ and 24 Å mm^{-1} at the *K* line. He reported that the normal spectrum showed prominently the strong lines of the K star right down to the limit of his plates at about λ 3700 Å, but the filling in of the metallic lines by the continuum of the early-type star was very marked in the ultraviolet and appreciable even at Hγ; even so, the Balmer lines of the hot star were not certainly recognizable against the clutter of K-type lines. The chromospheric effects associated with egress from eclipse and subsequently were broadly comparable with the analogous events in ζ Aur, the principal differences being that in 32 Cyg the hot star is relatively fainter and the events occurred on a more leisurely time-scale by a factor of about four. Judging by the relatively short maximum duration permitted by his observations for the total phase of the eclipse, McLaughlin was already able to suggest that, whereas the eclipse in ζ Aur might be nearly central, that in 32 Cyg is almost a grazing one.

It transpired that the Ottawa orbit[107] had long since been regarded as unsatisfactory by the radial-velocity enthusiasts at the DAO, who had had 32 Cyg on their observing programme since 1936; 68 plates had already been obtained before the discovery[17] of the eclipses by McLaughlin. In a 1952 paper on 32 Cyg, Wright[112] says that the plates had been "allowed to accumulate", which may have been a way of indicating that they had not actually been looked at in any detail. Some of the exposures were made so near to the times of previous eclipses in 1940, 1943, and 1946 that the associated effects were noticeable once attention had been drawn to them. A seeming inconsistency is that, whereas Harper[108] apparently saw no evidence on DAO plates that the spectrum of 32 Cyg was composite at all, Wright[112] said that "even the ultraviolet hydrogen lines in the secondary spectrum were too weak to measure with certainty". A way of reconciling the statements would be to suppose that Wright was putting the best possible face on a situation in which it was under-exposure in the relevant region that made all lines there too weak to measure. Both authors were referring to spectra taken with the Cassegrain prism spectrograph at the 72-inch reflector, but two different dispersions (about 11 and 30 Å mm^{-1} at Hγ) were used by Wright; Harper is completely silent about the nature of his spectra.

Once its eclipse had been discovered, the star was intensively observed at the DAO for the remainder of the calendar year, but bad weather prevented the observations from starting while the eclipse was still total. Four further plates were obtained in the following year (1950) before the whole set was measured and utilized by Wright[112] in a fresh solution of the orbit. The phase distribution of the radial-velocity data was unbalanced by the intensive post-egress observations, and it now seems risky in any case to have included measurements made near to eclipse. Wright appears to have considered omitting such measurements, but decided to accept them on the grounds that "the effect of the B star is relatively small in the region where measurements were made ($\lambda\lambda$4050–4530)", seemingly overlooking the fact that the measured lines might well be displaced from their true photospheric positions by blending chromospheric components. Wright also accepted, understandably enough, the orbital period which appeared to have been determined by Hoffleit[110] from eclipse timings far more accurately than he could hope to obtain it from the radial velocities, but which has subsequently proved to have been mistaken. Having committed himself to that period, he reduced the computational task of solving the orbit by condensing the 96 measured radial velocities into 11 'normal points', which involved the averaging of observations that were spread over five cycles and thus smeared the phase bins by up to 30 days or so, the adopted period being nearly seven days in error.

Wright next made an effort to measure the equivalent widths of the chromospheric *H* and *K* lines observed near eclipse — not a particularly easy or certain thing to do, since the relevant part of the spectrum is additionally bedevilled by emission, probably variable, from the K supergiant. He plotted the results for the *K* line against time from the supposed date of mid-eclipse; they formed what might now seem a surprisingly well-behaved, almost monotonically declining series. He was able to include values from plates taken near to the three pre-discovery eclipses (1940, 1943, and 1946), although the abscissae of the corresponding points must be in error by about seven days per cycle owing to the error in the adopted period.

Furthermore, Wright made an effort to characterize the B star from high-dispersion plates (4·5 Å mm^{-1} in the violet and 7 Å mm^{-1} in the red) already taken, before the discovery of the eclipse, with a different spectrograph that used a grating as the dispersing element. Not having any spectra of 32 Cyg itself in total eclipse, he used corresponding ones of γ Dra and ξ Cyg as surrogates for the primary, and by a laborious procedure of planimetry of successive short intervals of wavelength in 32 Cyg and the surrogate he was able to recover some approximation to the secondary spectrum. He considered that He I lines were present, and estimated the type as B8. In a final summary, he proposed that the primary is of type K5, with a radius of 195 $R_{\odot}$, an absolute magnitude of $-2^{m}\cdot 7$, and a mass of 21 $M_{\odot}$; the corresponding figures for the B8 star were $2\cdot 5$ $R_{\odot}$, $0^{m}\cdot 0$, and $7\cdot 6$ $M_{\odot}$. He estimated the duration of total eclipse to be 13 days, and derived an orbital inclination of 82°. Wright's work was evidently completed quite promptly, because an abstract[113] of his paper[112] appears among those that report the meeting of the American Astronomical Society held in Washington, DC, in 1951 June. The progressive diminution of the chromospheric *K* line after eclipse was already presented[114] at a meeting of the Astronomical Society of the Pacific at Salt Lake City in 1950 June.

A thorough discussion of the ensuing (1952/3) eclipse was presented by Wellmann and Weston from the David Dunlap Observatory (DDO), first as abstracts[115] in 1953 and then in a paper[116] in 1957 by the former alone. (Wellmann, on sabbatical leave from the Hamburg Observatory, spent five months at the DDO in 1953, which was evidently when he did the principal work, but he did not complete the paper until he made another extended visit to Toronto in 1955/6.) They used spectra not only from the DDO and Hamburg but also from Michigan. Both Wellmann and Weston concluded, from the accurately determined date of mid-eclipse (1952 December 27·3), that the true period must be close to 1148 days and not the 1141 days that had been proposed[110]. On the basis that the primary star was of luminosity class I, with $M_V \sim -6^{m}$, and a $\Delta m_V \sim 4^{m}$ by extrapolation from the Hγ region, as well as from a consideration of how far the Balmer series could be traced towards its head in the spectrum of the secondary star, Wellmann deduced that the latter must be of type about B2 (moderated to B3 in the final paper[116]). The radii of the stars were put as great as 353 and $3\cdot 9$ $R_{\odot}$, and the orbital inclination had to be about $72^{\circ}\cdot 5$. The duration of the eclipse could be determined with little uncertainty at 13 days. Weston recognized, from the irregularities in the development of the atmospheric-eclipse effects, that the K star's chromosphere was clumpy and included or consisted of gaseous clouds and streams. He also noted, in an extraordinary exhibition of clairvoyance, that in view of the grazing nature of the eclipse, "if the K star is even slightly variable in size, then the durations of the total and partial phases must be variable, and at some conjunctions totality may even fail to occur."

Provision of good photometric coverage of eclipses in 32 Cyg seems to have been delayed. That becomes less surprising when it is realized that the orbital period is just 51 days more than the integer number of three years, so eclipses come successively that much later in the calendar year, in a repeating 22-year cycle embracing seven eclipses. It happened that the discovery event in 1949 was the last one before the eclipses progressed into the season of the year when the weather was worst and the star, even for observatories where it was nominally circumpolar, was low in the sky.

Wood (who is unaccountably identified as "Bradshaw F" by *Simbad*) & Lewis[117] set out to observe the 1952 event photometrically but largely missed it, and considered that they had discovered instead that there were large capricious variations (not seen again since, however) of as much as $0^{m}\cdot 2$ in the yellow and $0^{m}\cdot 3$ in the blue. Chandra & Pande[118] presented in this *Magazine* a short but well-planned series of *BV* observations, obtained at the Uttar Pradesh State Observatory, of the 1959 event. They found eclipse depths of only $0^{m}\cdot 025$ in *V* and $0^{m}\cdot 12$ in *B*, and deduced values of V and $(B-V)$ of $4^m \cdot 14$ and $1^m \cdot 64$ for the primary and $8^m \cdot 24$ and $-0^m \cdot 26$ for the secondary. From that colour index, even without mentioning interstellar reddening, they suggested a spectral type as early as B1 V. Although Chandra & Pande did not comment on it, the graphs of their photometry are by no means compatible with a 13-day duration of totality. It is far from clear that there is any total phase (presumably to be identified with an interval of constancy at minimum light) at all, and the greatest length that it could possibly be would be four days.

Herczeg & Schmidt[119] observed the 1962 eclipse and found depths of $0^m \cdot 027$ in V, $0^m \cdot 126$ in B, and $0^m \cdot 633$ in U, thus closely corroborating the V and B results of Chandra & Pande. They too derived colour indices for the component stars, but got results appreciably different from those of Chandra & Pande in the case of the secondary: the $(B-V)$ colours were $1^m \cdot 63$ and $-0^m \cdot 07$, while the $(U-B)$ ones were $1^m \cdot 51$ and $-0^m \cdot 51$. They then allowed for modest interstellar reddening of $0^m \cdot 13$ in $(B-V)$ and $0^m \cdot 07$ in $(U-B)$, and from the resulting colour indices for the B star they proposed its type to be B4 V. Faraggiana *et al.*[120] observed the 1962 eclipse spectroscopically. They considered that it was total for 13 days and that the partial phases lasted four days; the chromospheric K line was visible for 23 days on either side of totality, and the chromospheric lines were blue-shifted by 10 – 20 km s^{-1} with respect to the photospheric spectrum both before and after the eclipse. An effort to recover the spectrum of the hot star by the subtraction process led to a suggested type of B5 IV or V.

Osterbrock[121], in a short paragraph in his Director's report of the Washburn Observatory, said that Doherty considered that the 1965 eclipse of 32 Cyg may not have been total; in an abstract[122], however, which is all that Doherty himself appears to have published about his work, that issue is not mentioned. Johansen, Rudkjøbing & Gyldenkerne[123] published a substantial amount of photometry of 32 Cyg, not only in *UBV* but also in a number of narrow bands, and they plotted the results of their own photometry and that of others (of whose sources they presented a useful and comprehensive listing) in 1959, 1962, and 1965. That the eclipse is caused in part by extinction by the supergiant's atmosphere, as well as geometrically, is well brought out by the manner in which the photometric minima widen towards short wavelengths. The authors doubt whether the 1962 eclipse reached totality, and do not see totality as being longer than four days at any of the eclipses.

Wright & Hesse[124], evidently with the coöperation of many other members of the DAO staff, made an intensive study of the 1965 eclipse of 32 Cyg, which occurred in late July, a time when the star is right at opposition and the Victoria weather is generally good. Their paper includes a splendid full-page montage of 42 spectra showing the vicinity of the H and K lines and documenting the development and disappearance of the chromospheric spectrum over the whole interval June–September. Intensity tracings of the K-line region from 33 of the spectra illustrate in detail the development of two chromospheric lines and particularly the extraordinary variations and multi-furcation of the K line itself, subjects that are of course discussed in the text of the paper. Its authors concluded that totality at the 1965 eclipse lasted at least 12 days and possibly 15. There is a troubling inconsistency between the conviction of spectroscopists such as Faraggiana *et al.*[120] and Wright & Hesse[124] that the eclipse is total for many days and the doubt of photometrists as to whether it even reaches totality at all, even at the same eclipse; and it seems certain that there are real differences between different eclipses.

In his 1970 review[59] in *Vistas* of the ζ Aur stars, Wright included a summary of the 1965 observations at the DAO. He considered that the K supergiant in 32 Cyg was slightly larger, later, and more luminous than that in 31. He gave the radii of the K and B stars as 395 and 6·2 $R_{\odot}$, respectively — both of which are surprisingly large values. It may be noted, however, that in the case of 31 Cyg the radius of the supergiant, estimated spectroscopically in the same way as the figure just quoted for 32, was more than double the same quantity estimated from the eclipse duration.

Saitō & Sato[125] published a substantial amount of convincing photometry from both the 1968 and 1971 eclipses. The observations of the two eclipses are plotted on the same graphs and appear to be in good agreement; they show no total phase, and indeed the authors say that the light-curves deduced from, and drawn through, them require that even at minimum light the B star is only half-eclipsed. The light-curves take into account the dimming caused by the chromosphere, which causes the light of the B star to be reduced to one quarter at mid-eclipse even though half the area of its disc is still uncovered. The duration of the (continuously partial) eclipse is put at 17 days. The eclipse depths at mid-eclipse are given as $0^{m}\cdot04$ in V, $0^{m}\cdot15$ in B, and $0^{m}\cdot88$ in U — disconcertingly greater than the depths found by Chandra & Pande[118] and by Herczeg & Schmidt[119] at earlier eclipses, especially since the eclipse is still apparently partial at the depths found by Saitō & Sato. The last-named authors found the Δm_V between the components to be $3^{m}\cdot3$.

Galatola[126] made an effort somewhat analogous to that of Saitō & Sato, but started from published photometry rather than from any of his own. He too modelled the eclipse by treating the eclipsing body as an opaque disc surrounded by a foggy atmosphere whose opacity decreases outwards according to an empirically specified law. He could obtain a solution by arbitrarily deeming that the eclipse was marginally total, *i.e.*, that at mid-eclipse the disc of the B star is internally tangent to that of the K star, but that solution was not unique. The difference in the model light-curves between a total eclipse and one that does not reach totality is too small to be distinguished by the photometry. Even the characterization of the supergiant star as an opaque disc, with the implication that it has a sharp limb at which the optical depth for a tangential ray changes discontinuously from a modest value to effectively infinite, begins to seem a little naïve in the face of photometric evidence that the disc really needs to be assigned a wavelength-dependent radius. Those problems are generally admitted by the best of the authors who have tried to tackle them, and still remain.

Doherty, McNall & Holm[127] obtained photometry of the 1971 eclipse of 32 Cyg in the 'space ultraviolet' region from *OAO–2*; they found that the eclipse appeared total for six or seven days after the supposed date of mid-eclipse at λ 1916 Å. They ascribed excess emission (not to be understood as coming from the supergiant component) at λ 2460 Å to recombination of Mg and Si ions in the chromosphere, but Stencel *et al.*[128] subsequently attributed it to a plethora of Fe II emission lines in the relevant band of wavelength.

Saijo & Saitō[129], in a fresh discussion of eclipse photometry and the wavelength dependence of opacity in the atmosphere of the K star, concluded that the spectral types of the components are K5 Ib and B8 V, and gave their radii as 250 and only 2·0 $R_{\odot}$. That would imply a B star that was substantially less luminous than the types, around B4 V, that had usually been found previously; those authors themselves attributed to it an absolute magnitude as faint as $+0^{m}\cdot1$, and put the K star also at the rather modest value of $-2^{m}\cdot3$, which, however, agrees exactly with the one listed by O. C. Wilson[130] from his *K*-line-width method. Indeed, there is close agreement with Wright's[112] 1952 findings, but in the intervening period a conviction — shared even by Wright in his 1970 review[59] — had arisen that the luminosities of the stars were substantially higher and the spectral type of the B star correspondingly earlier. The discordances were starkly illustrated in his 1960 review by Wilson[131], whose Table 5 juxtaposes the conclusions of Wright[112] and Wellmann[116]. Much later, in a spectroscopic analysis of a large number of GK giants, McWilliam[132] included 32 Cyg and obtained for it an absolute magnitude of $-2^{m}\cdot 11$, thus allying himself with the 'modest-luminosity' faction after approaching the problem from a completely different angle. Although it might be argued that his result is not to be trusted because the contribution of the B star to the spectrum was ignored, the fact is that his observations were made in the red, at about λ 6600 Å, where we know from the tiny depth of the eclipse that the B star contributes no more than about 2% of the flux.

Guinan & McCook[133] obtained photometry of 32 Cyg at Hα and Hβ wavelengths at times not confined to the vicinity of eclipse but extending all round the cycle, albeit with substantial seasonal gaps. Appreciable systematic variations (several hundredths of a magnitude) were seen, and were understood in terms of a model in which the supergiant star was tidally distorted. To achieve a fit, however, the authors felt free to regard the eccentricity and longitude of periastron of the orbit almost as disposable parameters. They elected to change the values of 0·30 and 218° initially adopted from Wright's orbit[59] to 0·28 and 185°; in both cases the changes were, as will become clear from the new and more accurate spectroscopic elements below, of the wrong sign, and in the case of the longitude were of quite unacceptable magnitude. It may be concluded, therefore, that the out-of-eclipse photometric variations — if indeed they are reproduced from cycle to cycle — have not been fully modelled by tidal distortion.

In 32 Cyg, as in the other ζ Aur stars, we know from the multiple chromospheric *K* lines (as well as from arguments, not recited here, concerning the number densities of ions and electrons in the chromospheres and stellar winds, which are orders of magnitude too large to be accepted as characterizing the complete volume) that the stellar atmospheres are extremely 'clumpy'. An observation made by or for Schröder[134] with *IUE* at the 1981 eclipse went further by demonstrating a secondary obscuration in a photometric band (as distinct from just an absorption line) at $\lambda < 2000$ Å, several days after the end of the total phase of the eclipse at that wavelength.

Recent years seem not to have added a great deal to our — or at least to the writer's — understanding of 32 Cyg, but there has been some renewed interest in its spectral classification. Parsons & Ake[135] reviewed the composite spectra that had been investigated with *IUE*; they made a rather uncertain classification of the hot star as B5·5:, and by partitioning the flux throughout the *IUE* and optical ranges they assessed the components as 'bK5 + B5', where the 'b' was intended to mean 'bright giant', *i.e.*, something between the Class I and Class III stars for which they actually had standards. Ginestet, Carquillat & Jaschek[136] classified the cool components of a lot of composite objects from spectra taken in the near infrared, where there would be little (in the case of 32 Cyg almost *no*) interference by the hot companions, and found the type to be K3 Ib-, where the hyphen is intended to mean that the luminosity is on the low side for Class Ib. Ginestet & Carquillat[137] also made an effort to classify the hot components of the systems by subtraction of the late-type spectra or analogues thereof from spectra taken in the violet, after the manner developed by the Griffins[27] — indeed, the work was performed with software supplied by R. E. M. Griffin. The result for 32 Cyg was B4·5:, and a note says, "Between B4 and B5 (poor subtraction). Strong Ca II emission from the primary." In a discussion of the composite system HR 2030, the Griffins[138] referred to 32 Cyg as being, with ζ Aur, the only *other* composite objects of which they were aware that exhibit emission lines of Si I at $\lambda\lambda$ 3905·5 and 4102·9 Å at certain phases of the orbit; the lines are produced as a result of the intense irradiation of the late-type star by the ultraviolet flux of the B star.

Radial velocities and orbits for 32 Cygni

The radial velocity of 32 Cygni was first measured at Lick, in 1905. By the following year, when four plates had been taken, it had already shown a large change; that fact was promptly announced — in triplicate, as was the convention at that time — by Campbell & Moore[139], who credited the discovery of the variation to Mr. Burns. The Lick observers seem to have been by no means enthused over a change that amounted to 30 km s^{-1} in just over a year, and even at the conclusion of their great survey[61] more than 20 years later they had taken only four further plates of the star. Meanwhile, Küstner[140] had obtained three plates, which gave mutually discordant velocities, with the Bonn 12-inch refractor. Moreover, Cannon[107] had published an orbit for the star on the basis of 117 spectra taken at the Dominion Observatory at Ottawa.

Cannon's paper has a number of errors and omissions, some of which we can rectify, and other features which not all of his readers are likely to understand. The constant reference to the subject of the paper as θ^2 Cyg has already been mentioned. The paper begins by saying that the star was under observation [at Ottawa] during the years 1914, 1915, 1916, and 1917, during which time 117 plates were taken. It does not describe the plates at all or say with what they were taken; the reader is left to suppose that they came from the one-prism spectrograph, which gave a reciprocal dispersion of about 33 Å mm^{-1} at Hγ, on the 15-inch refractor. The actual number of plates listed for the years concerned is 112 and not 117, but Cannon mentions that five plates taken in 1908 helped in the determination of the period. He does in fact list five early plates, bringing the *total* number to 117; according to the Julian dates listed for them, however, the five early plates were all taken in 1910 October and not in 1908.

A feature that the paper has in common with many of the Ottawa papers on spectroscopic orbits is that it includes an initial table giving a list of the lines measured — a list of wavelengths. The present writer is dimly aware that, in the days when radial velocities were measured line-by-line on photographic spectrograms, it was important to adopt satisfactory effective wavelengths for the lines that were measured, and that those wavelengths were not necessarily the same for different instruments having different dispersions. Also, in some cases (where lines were blended and the relative contributions of the components to the blend varied with spectral type) the effective wavelength of the blend varied with type, and it is believed that such lines were shunned by measurers rather than being assigned type-dependent wavelengths. Ottawa orbit papers, however, often started with just a table of wavelengths, without explanation and without the assignment of the lines to their particular elements of origin. Moreover, the wavelengths listed are unique to the paper giving them — that is to say, they differ, seemingly chaotically, from one paper to another. The differences are not small, often amounting to some tenths of an Ångström (in the violet, 1 Å corresponds to about 70 km s^{-1}), although the wavelengths themselves are listed to three decimal places.

In Cannon's list of 18 lines, the one that is furthest from any normally accepted value is the one given as 4404·042, which is without any ambiguity intended to relate to a strong line of Fe I whose wavelength is really near 4404·8 Å and is utilized in other papers in the same volume of the *Ottawa Publications* with wavelengths varying over the relatively modest range from 4404·780 to 4404·956 Å. The value 4404·042 is not a misprint, at least not in the ordinary sense of that word, since it is repeated 16 times in the tables of plate measurements that constitute most of the paper.

Cannon appears first to have fixed upon an orbital period; his continuous observing campaign only just covered one cycle, and he said in the paper that he used the five early measures to help with the period. He did not say whether he also took into account the published Lick and Bonn measurements, but the fact that he tabulated them anew may indicate that he *did*. He decided on a period of 1170 days, which is a fair enough approximation to the true value as we now know it, of 1147 days. He then solved the orbit by forming the 117 observations into 12 'normal points', upon which he performed a least-squares solution. He evidently did not like the manner in which the 12 points scattered around a Keplerian curve, because he proposed that, superimposed upon the motion in the elliptical orbit, there was a secondary variation corresponding to a circular motion in a period exactly one-third as long. It is not at all clear how he proceeded (his 23-page paper includes a total of only 30 lines of text, so explanations are very limited), but it appears that he may have plotted his normal points before he did any calculations, decided that the fit would be improved by the addition of the arbitrarily specified secondary variation, *and then applied 'corrections' corresponding to that variation to the normal points before performing the solution*. The evidence that he actually tampered with the velocities of the normal points lies in his assertion that the least-squares adjustment resulted in "a reduction of the value of Σpvv from 75 to 60" as well as in the residuals attributed to the normal points. It seems likely that 'Σpvv' means the sum of squares of the residuals, and that the p refers to the individual weights (ranging from 1 to 4) attributed to the normal points; those residuals and weights are specified in the table of those points. Comparison of the table with the plotted velocity curves shows that the residuals correspond to the discrepancies of the normal points not from the Keplerian curve but from the see-saw curve that is compounded from the actual velocity curve plus the triple-frequency one. The discrepancies from the Keplerian curve itself are much greater, amounting in several cases to more than 5 km s^{-1}, so in each case such a point would contribute to Σpvv an amount at least comparable with that whole sum. It is scarcely surprising that Cannon could get a relatively good fit to the points if he added a second variation — he was then fitting eight disposable variables (recall that he fixed the period beforehand) to 12 normal points, leaving only four degrees of freedom. A three-page summary of Cannon's paper, including all of the same text apart from omission of reference to the list of observations and minor editorial style changes (one of which was to change θ^2 to θ_2!), was published in the *Astrophysical Journal*[141].

In 1928 the publication of the Lick catalogue[61], already mentioned, brought four more velocities, and Harper[108] provided three more. Tremblot[52] then published a much improved orbit; he utilized the Lick[61], Bonn[140], and DAO[108] velocities and subscribed 29 of his own measurements, made at Haute-Provence ('Forcalquier') in 1936–39 with the same equipment as that described in the section on 31 Cyg above. He did not use the Ottawa measurements or subscribe to Cannon's idea that there was any variation apart from that in the obvious orbit;

he noted (in French, of course — this is a translation), that "the residuals ... are too small to support the hypothesis, put forward by Cannon, of significant perturbations in the motion of the cK5 star." A footnote to that remark adds, "His portrayal of them can be regarded only as a very rough diagram." As in the case of 31 Cyg, Tremblot obtained orbital elements remarkably close to those that are found in this present paper.

After a considerable interval there ensued the publication in 1952 of a new orbit by Wright[112], who had had 32 Cyg on his observing programme for many years and was galvanized into measuring the plates and writing up the work by the discovery in 1949 of the eclipses. A paragraph in the narrative above describes Wright's orbit and touches upon some possible shortcomings. After making a solution of the Victoria measurements, Wright resolved to see whether he could improve that of the early Ottawa ones, whose loan he solicited. He selected the best 52 of those plates and re-measured them; he obtained a solution that was probably better than Cannon's original one[107] from the whole set of Ottawa plates, but still was more ragged than the DAO solution although it was not significantly in conflict with it. Wright (like Tremblot[52], whom, however, he does not mention) did not feel a need to superimpose any additional variation upon the Keplerian orbit; he did, however, sail *very* close to the wind in performing the solution, because he condensed his 52 velocities into only eight normal points, and having fixed the period he was fitting five disposable parameters and leaving himself with only three degrees of freedom.

Wright also tabulated 13 post-1928 Lick plates for which radial velocities had been sent to him privately by Herbig for possible use in the derivation of the orbit (actually only ten of the entries have velocities assigned to them). He cited misgivings concerning their zero-point in comparison with the DAO measures as dissuading him from utilizing them. In doing so, however, he seemed to be saying that what dissuaded him was that, to judge from their residuals when plotted on the Victoria velocity curve, they did not conform to the departure between the zero-points usually to be found between Victoria and Lick, so his 'excuse' does not cover the *published* Lick velocities, which equally he failed to utilize. We can see in retrospect that Wright would have done well to put more faith in radial velocities and less in the supposed identification of 19th-Century eclipses. He actually remarked himself on the bad systematic positive offset from his velocity curve of five measurements taken near the time of the 1940 eclipse (he identified them as plates nos. 37–41 in his list, but they are actually nos. 35–39) in comparison with ones taken at the same phase in 1949. It happens that the time of eclipse is also the phase when the velocity is changing most rapidly and so has the maximum sensitivity to phase. Much of the discrepancy identifed by Wright stems from the imposition of the wrong period on the data. Actual experiment with his data set (all the plates being given equal weight) shows that, solved with the period left free, it gives $P = 1147{\cdot}2 \pm 2{\cdot}6$ days, and the offset of the offending 1940 observations is greatly reduced; inclusion of the published Lick velocities (even with no more than equal weighting with the DAO measures, although they deserve more), greatly increases the time base and almost halves the uncertainty in the period, which becomes $1146{\cdot}9 \pm 1{\cdot}4$ days.

In his 1970 review[59] of the ζ Aur stars, Wright gave slightly revised orbital elements for 32 Cyg, obtained from the same measured velocities as he had used[112] in 1952 but with the orbital period fixed at the much better value of 1147·8 days and with the solution performed by computer on the individual measurements instead of by 'hand' on 'normal points'.

Since that time there have been few further developments on the radial-velocity front until very recently. Beavers & Eitter[99] published just one measurement from Ames, and de Medeiros *et al.*[142] referred to three measurements, all obtained within an interval of 43 days, that were listed only as an undated mean. In 2007 Eaton & Williamson[101] included 32 Cyg among a lot of stars whose radial velocities had been measured very frequently with their automated telescope over a relatively short space of time, in a survey for short-term 'jitter'; they mentioned that they had made as many as 292 measurements of 32 Cyg, and they gave orbital elements derived from them, apart from a period (copied from Wright[59]) that is imposed. The r.m.s. deviation per point from the orbit is listed as 0·29 km s^{-1}.

As far as the present writer is concerned, the observational history of 32 Cyg has continued the same uncanny parallelism with 31 Cyg as it has had all along and as the stars themselves have exhibited. The observations started in 1988 and consist of 15 made with the original spectrometer, 35 obtained at Haute-Provence, and 93 taken with the current *Coravel* at Cambridge. They are all set out in Table III. As in the case of 31 Cyg, the OHP velocities have been increased by 0·8 km s^{-1} from the values given by the initial reductions, and the Cambridge *Coravel* ones have been increased by 0·3 km s^{-1}. Again as for 31 Cyg, and for the same reasons, the decision was taken not to include any of the published radial velocities in the solution of the orbit, but to rely entirely on those newly presented here. The same weightings for the different data sources have been adopted: 1/6 for the original spectrometer, 0·4 for OHP, and unity for the Cambridge *Coravel*.

The one thing that has been done differently here is that the period has been allowed as a free parameter. The seeming indefiniteness of the eclipses of 32 Cyg has prevented the period being established with great precision; the impression is that it may still be uncertain by a few tenths of a day, which is equally the case with the period obtained independently from the radial velocities, so there seems to be little merit in imposing the photometric period even if we could be sure what the best value for it might be. The most recent proposal that has come to the writer's attention is that of Schröder[134], who favoured a value of 1147·15 ± 0·1 days.

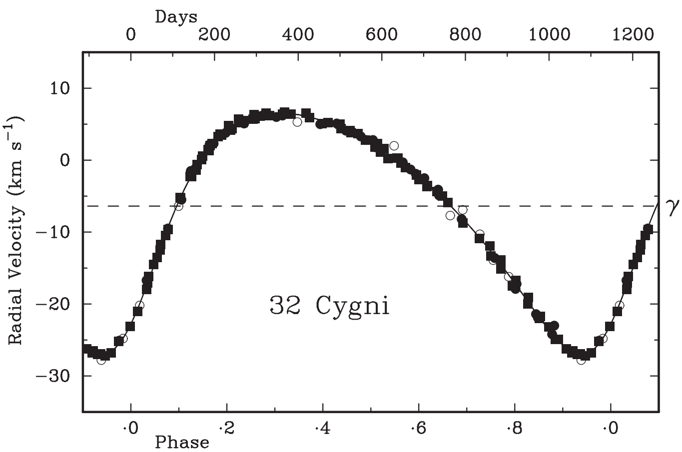

FIG. 3

The observed radial velocities of 32 Cygni plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The symbols are the same as in Fig. 1.

Table III

Radial-velocity observations of 32 Cygni

Except as noted, the sources of the observations are as follows:
1988–1998 — Haute-Provence Coravel (weighted 0·4 in orbital solution);
1999–2008 — Cambridge Coravel

Date (UT)	MJD	Velocity km s⁻¹	Phase	(O–C) km s⁻¹
1988 Nov. 6·87	47471·87	+2·8	0·490	+0·1
Dec. 19·79*	514·79	+1·5	·528	+0·3
1989 Jan. 12·74*	47538·74	+2·0	0·548	+1·7
Mar. 26·20	611·20	−2·5	·612	+0·5
Apr. 28·16	644·16	−4·1	·640	+0·6
May 27·09*	673·09	−7·7	·666	−1·4
June 26·01*	703·01	−6·9	·692	+1·2
Aug. 5·97*	743·97	−10·3	·727	+0·5
Sept. 6·94*	775·94	−13·9	·755	−0·8
Oct. 13·87*	812·87	−16·2	·787	−0·3
Nov. 1·80	831·80	−17·2	·804	+0·3
1990 Jan. 30·73	47921·73	−23·0	0·882	+1·4
Apr. 5·17*	986·17	−27·8	·938	−0·8
May 27·11*	48038·11	−24·8	·984	0·0
July 6·05*	078·05	−20·2	1·018	−0·2
Oct. 6·90*	170·90	−6·4	·099	−0·6
Nov. 5·79*	200·79	−2·1	·125	+0·1
Dec. 19·71*	244·71	+2·2	·164	+0·5
1991 Jan. 27·73	48283·73	+3·8	1·198	−0·2
Feb. 4·25	291·25	+4·3	·204	0·0
June 14·06*	421·06	+6·6	·317	+0·1
July 18·04*	455·04	+5·3	·347	−1·0
Oct. 28·88*	557·88	+4·7	·437	+0·2
Dec. 16·75	606·75	+3·3	·479	+0·2
1992 Jan. 14·72	48635·72	+2·8	1·504	+0·6
Apr. 26·16	738·16	−1·9	·594	+0·1
June 21·05	794·05	−4·9	·642	−0·1
Aug. 13·97	847·97	−8·2	·689	−0·2
Dec. 20·75	976·75	−17·9	·802	−0·6
1993 Feb. 12·22	49030·22	−21·9	1·848	−0·3
Mar. 18·21	064·21	−24·2	·878	−0·1
July 8·14	176·14	−25·1	·975	+0·4
Sept. 11·99	241·99	−16·7	2·033	+0·8
26·78	347·78	−1·5	·125	+0·8
1994 Jan. 9·73	49361·73	−0·9	2·137	0·0
Feb. 19·24	402·24	+2·3	·172	0·0
May 3·15	475·15	+5·1	·236	−0·5
Aug. 2·98	566·98	+6·2	·316	−0·3
Dec. 10·73	696·73	+5·1	·429	+0·4
1995 Jan. 1·75	49718·75	+4·1	2·448	−0·1
June 5·13	873·13	−1·3	·583	+0·1
Dec. 23·72	50074·72	−13·6	·758	−0·2
1996 Mar. 31·17	50173·17	−21·4	2·844	−0·2
Nov. 27·75†	414·75	−13·5	3·055	−0·1
Dec. 23·70	440·70	−9·5	·077	−0·1

TABLE III (*continued*)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O–C) km s⁻¹*
1997 Jan. 24·72	50472·72	−5·5	3·105	−0·6
Mar. 1·25†	508·25	−1·4	·136	−0·4
Apr. 1·20†	539·20	+1·3	·163	−0·3
May 1·16†	569·16	+3·5	·189	0·0
June 17·08†	616·08	+5·2	·230	−0·2
July 20·01	649·01	+5·8	·259	−0·3
Sept. 8·96	699·96	+6·0	·303	−0·5
Dec. 22·73	804·73	+5·0	·395	−0·6
1998 May 3·14	50936·14	+1·8	3·509	−0·2
July 8·06	51002·06	−0·3	·567	+0·3
1999 Dec. 29·69	51541·69	−16·2	4·037	+0·5
2000 Jan. 10·71	51553·71	−14·5	4·047	+0·3
Apr. 10·18	644·18	−2·3	·126	−0·1
May 20·13	684·13	+1·5	·161	+0·1
June 18·11	713·11	+3·6	·186	+0·3
July 16·90	741·90	+4·4	·211	−0·2
Aug. 12·01	768·01	+5·2	·234	−0·3
Sept. 20·89	807·89	+6·1	·269	−0·2
Oct. 13·85	830·85	+6·2	·289	−0·3
Nov. 13·78	861·78	+6·4	·316	−0·1
Dec. 4·69	882·69	+6·4	·334	−0·1
2001 Jan. 9·71	51918·71	+6·5	4·365	+0·4
May 13·14	52042·14	+3·7	·473	+0·3
June 29·11	089·11	+2·3	·514	+0·5
July 15·13	105·13	+1·6	·528	+0·4
Aug. 15·01	136·01	+0·3	·555	+0·3
16·86	137·86	+0·3	·556	+0·4
Sept. 29·86	181·86	−2·1	·595	−0·1
Oct. 25·79	207·79	−3·7	·617	−0·4
Dec. 1·76	244·76	−5·0	·650	+0·3
14·78	257·78	−5·9	·661	+0·1
2002 Jan. 19·73	52293·73	−8·7	4·692	−0·5
Feb. 27·25	332·25	−10·9	·726	−0·2
Mar. 27·20	360·20	−13·3	·750	−0·6
Apr. 20·17	384·17	−15·1	·771	−0·6
May 17·12	411·12	−17·5	·794	−0·9
June 23·11	448·11	−20·0	·827	−0·4
July 21·02	476·02	−22·0	·851	−0·2
Aug. 12·97	498·97	−23·2	·871	+0·4
28·93	514·93	−24·9	·885	−0·2
Sept. 23·92	540·92	−26·2	·908	−0·1
Oct. 6·88	553·88	−26·8	·919	−0·2
19·93	566·93	−27·0	·930	−0·1
Nov. 7·85	585·85	−27·2	·947	−0·3
21·87	599·87	−26·8	·959	−0·2
Dec. 9·70	617·70	−25·2	·975	+0·4
2003 Jan. 5·72	52644·72	−23·1	4·998	−0·1
23·74	662·74	−21·0	5·014	−0·3
Feb. 15·24	685·24	−18·0	·033	−0·6
Mar. 19·18	717·18	−12·5	·061	−0·3
Apr. 1·05	730·05	−10·5	·072	−0·3
7·18	736·18	−9·6	·078	−0·3

TABLE III (*concluded*)

Date (UT)		*MJD*	*Velocity* $km\ s^{-1}$	*Phase*	*(O–C)* $km\ s^{-1}$
2003 May	6·13	52765·13	−5·2	5·103	+0·1
	29·09	788·09	−2·3	·123	+0·2
June	15·10	805·10	−0·6	·138	+0·2
	28·11	818·11	+0·6	·149	+0·2
July	16·05	836·05	+2·0	·165	+0·2
Aug.	4·00	855·00	+3·3	·181	+0·3
	30·00	881·00	+4·8	·204	+0·5
Sept.	22·93	904·93	+5·7	·225	+0·5
Oct.	28·92	940·92	+6·3	·256	+0·2
Nov.	26·87	969·87	+6·5	·281	+0·1
2004 Jan.	9·75	53013·75	+6·7	5·320	+0·2
Apr.	23·16	118·16	+5·2	·411	0·0
May	24·12	149·12	+4·4	·438	−0·1
June	17·11	173·11	+3·8	·459	0·0
Aug·	13·02	230·02	+1·8	·508	−0·2
Sept.	13·97	261·97	+0·2	·536	−0·7
Oct.	25·86	303·86	−1·0	·572	−0·1
Nov.	26·77	335·77	−2·7	·600	−0·4
Dec.	16·69	355·69	−3·5	·618	−0·2
2005 Jan.	8·71	53378·71	−4·4	5·638	+0·1
May	15·14	505·14	−11·9	·748	+0·6
June	11·11	532·11	−13·9	·771	+0·6
July	17·08	568·08	−16·7	·803	+0·7
Aug.	15·06	597·06	−19·1	·828	+0·6
Sept.	12·99	625·99	−21·7	·853	+0·3
Oct.	25·90	668·90	−24·9	·891	+0·2
Nov.	29·85	703·85	−26·5	·921	+0·2
Dec.	17·79	721·79	−26·9	·937	+0·1
2006 Apr.	9·15	53834·15	−17·1	6·035	0·0
May	11·12	866·12	−11·7	·062	+0·3
June	28·01	914·01	−5·3	·104	−0·2
July	21·12	937·12	−2·1	·124	+0·3
Aug.	15·99	962·99	+0·1	·147	0·0
Sept.	10·98	988·98	+2·3	·170	+0·2
Oct.	24·92	54032·92	+4·2	·208	−0·3
Nov.	25·75	064·75	+5·5	·236	−0·1
Dec.	17·69	086·69	+5·7	·255	−0·3
2007 Jan.	14·72	54114·72	+6·2	6·279	−0·2
May	2.14	222·14	+5·9	·373	−0·1
June	1·11	252·11	+5·1	·399	−0·4
July	13·05	294·05	+5·0	·435	+0·5
Aug.	6·99	318·99	+4·1	·457	+0·2
Sept.	12·98	355·98	+2·8	·489	0·0
Oct.	17·92	390·92	+1·1	·520	−0·5
Dec.	5·85	439·85	−0·4	·562	0·0
2008 Jan.	24·74	54489·74	−2·6	6·606	+0·1

*Observed with original Cambridge spectrometer; wt. 1/6.
†Observed with Cambridge *Coravel*.

That period was selected to accommodate the timing of the eclipse that was then of interest to that of the one that led Wright[59] to favour 1147·8 days, but it has been noticeable in previous instances that timing precisions are illusory where eclipses of 32 Cyg are concerned, so the suggested uncertainty of the Schröder period may be optimistic. Six cycles of the orbit have been witnessed during the radial-velocity campaign, so the standard errors found here for the orbital elements, including the period, should be tolerably realistic. That is probably not true in the case of 31 Cyg, which has been seen round only two cycles and whose apparent orbit may still be appreciably disturbed by the ~ 2½-year 'swerves'. Fig. 3 shows the velocity curve and the observations upon which it depends; the orbital elements are:

$P = 1147{\cdot}51 \pm 0{\cdot}31$ days	$(T)_5 = \mathrm{MJD}\ 52646{\cdot}9 \pm 1{\cdot}4$
$\gamma = -6{\cdot}389 \pm 0{\cdot}032$ km s^{-1}	$a_1 \sin i = 252{\cdot}0 \pm 0{\cdot}8$ Gm
$K = 16{\cdot}77 \pm 0{\cdot}05$ km s^{-1}	$f(m) = 0{\cdot}486 \pm 0{\cdot}005\ M_\odot$
$e = 0{\cdot}3041 \pm 0{\cdot}0027$	
$\omega = 221{\cdot}4 \pm 0{\cdot}5$ degrees	R.m.s. residual (wt. 1) = 0·32 km s^{-1}

TABLE IV

Previously published orbital elements for 32 Cygni

Except in the last column, the quoted uncertainties are 'probable errors'

Element	*Cannon*[107]	*Tremblot*[52]	*Wright*[112]	*Wright*[59]	*Eaton & W'mson*[101]
P (days)	1170	1146	1140·8 ± 0·1	(1147·8)	(1147·8)
T (MJD)	20700 ± 19	15930	33127 ± 9	33142 ± 8	33139·48 ± 0·26
γ (km s^{-1})	−14·35 ± 0·65	−7·0	−6·12 ± 0·17	−5·69 ± 0·16	−7·35 ± 0·02
K (km s^{-1})	16·64 ± 0·93	16·6	16·65 ± 0·32	16·95 ± 0·28	16·77 ± 0·03
e	0·182 ± 0·053	0·35	0·274 ± 0·015	0·301 ± 0·013	0·3169 ± 0·0014
ω (degrees)	281 ± 5	220	216·6 ± 3·3	218·0 ± 3·0	223·6 ± 0·3
$a_1 \sin i$ (Gm)	263	245	–	255	–
$f(m)$ ($M_\odot$)	0·53	0·45	–	–	–

Once again, a table (Table IV) is offered here, to enable a comparison to be made between the currently determined elements and those given by others. It is apparent that the agreement with certain of the elements mentioned in the recent paper by Eaton & Williamson[101] is worse than it ought to be according to the quoted standard errors, but in the absence of any information as to how those elements were obtained it is not possible to comment except on the values of γ-velocity and T. The standard errors of the γ-velocities are to be seen only as internal errors; the discrepancy of 0·96 km s^{-1} is slightly less than the difference between the admitted offsets of −0·35 and +0·8 km s^{-1} of the respective series of velocities from what are taken as possibly more authentic zero-points. It seems extraordinary that an epoch of periastron in the year 1949 would be quoted as resulting from a very recent set of observations, and the standard error given for it is certainly illusory because it implies (among other things) that the period must be known almost to a hundredth of a day.

Runs of velocity residuals in both 31 and 32 Cygni

Reference was made, in the discussion of the orbital elements and velocity residuals of 31 Cyg, to the fact that the residuals seem to run in a systematic fashion with a quasi-periodicity of about 2½ years. Much the same thing is true at least some of the time in the case of 32 Cyg, although in the case of that star the periodicity is a little shorter. In fact that behaviour was noticed first of all in 32 Cyg, where it was not so readily visible directly in the velocity curve but was conspicuous in the runs of residuals, particularly after the more continuous and systematic Cambridge measurements had been going for a while. For some years an orbital solution for 32 Cyg, performed with the programme intended for a single-lined triple system, in which (as, for example, in HR 4454[143]) the velocity varies in two different periods, gave a most convincing graph of the second period and noticeably improved the graph of the principal one. The second period was just over two years, the orbital period just over three. A 2-year orbital companion in addition to the 3-year one was obviously dynamically impossible. Recently the triple orbital solution has not given such convincing results, and it has become clear that the apparent 2-year period in 32 Cyg is not a permanent feature. It was the periodicity that seemed at one time to exist in the velocity residuals of 32 Cyg that led to the investigation of those of 31 Cyg as well, to see whether they too could be understood as periodic.

The residuals from the orbital solution for 32 Cyg are plotted directly against time in Fig. 4a, which is exactly analogous to Fig. 2a for 31 Cyg; like Fig. 2a, it is restricted to the interval of time since the Cambridge *Coravel* came into regular use. Clearly there *are* systematic runs of residuals. The observations during the first and last years shown in Fig. 4a, however, do not seem to match the apparent periodicity seen in the intervening years; moreover, when the earlier velocities are plotted too (Fig. 4b, the analogue of Fig. 2b), it does not seem possible to trace in them the variation that appears as well-marked maxima about the middles of the years 2001, 2003, and 2005. One can possibly see maxima at about 1992·0 and 1994·0 and an isolated very high point at 1990·0, but there are minima scarcely more than 500 days apart on either side of the 1994 maximum and there is certainly no hint of a maximum in 1996. In short, the appearance of periodicity seen in the early years of the present century is not traceable in the previous one and seems likely to have been lost again already now. An 'orbit', whose period turns out to be 780 ± 26 days, can be computed from the recent Cambridge data alone, but it does not carry as much conviction as the one that is illustrated for 31 Cyg in Fig. 2c may be thought to do. By arbitrarily restricting the calculation to the time interval MJD 51750 – 53850, which embraces some $2^1/_2$ quite well-marked cycles, and which involves discarding 23 of the 88 data points, a tolerably convincing 'orbit', illustrated in Fig. 4c, is obtained. Its period remains practically the same, 775 ± 27 days; its amplitude is 0·32 ± 0·05 km s^{-1}. The discarded data are plotted as open symbols, and it can be seen that they do not support the 'orbit' at all, so it might be concluded that whatever it was that was responsible for the apparent periodicity in 2001 – 2006 did not operate, or at least did not hold phase, outside that interval.

The question clearly needs to be asked as to whether the apparent variations seen in Figs. 2a and 4a really represent things that are happening in or to the stars, or whether the explanation is to be sought much nearer to Cambridge. The most obvious possibility of error arises from a seasonal dependence of the mean hour angle of observation. The stars are nominally circumpolar in Cambridge and can be observed almost throughout the year, and since the *Coravel* spectrometer became available on the home site they have been scheduled for observation every month (and were in fact observed in every month of the year in 2002). Although the possibility of an hour-angle effect cannot be dismissed out of hand, it is demonstrably not the explanation of the quasi-periodic variations seen in Figs. 2a and

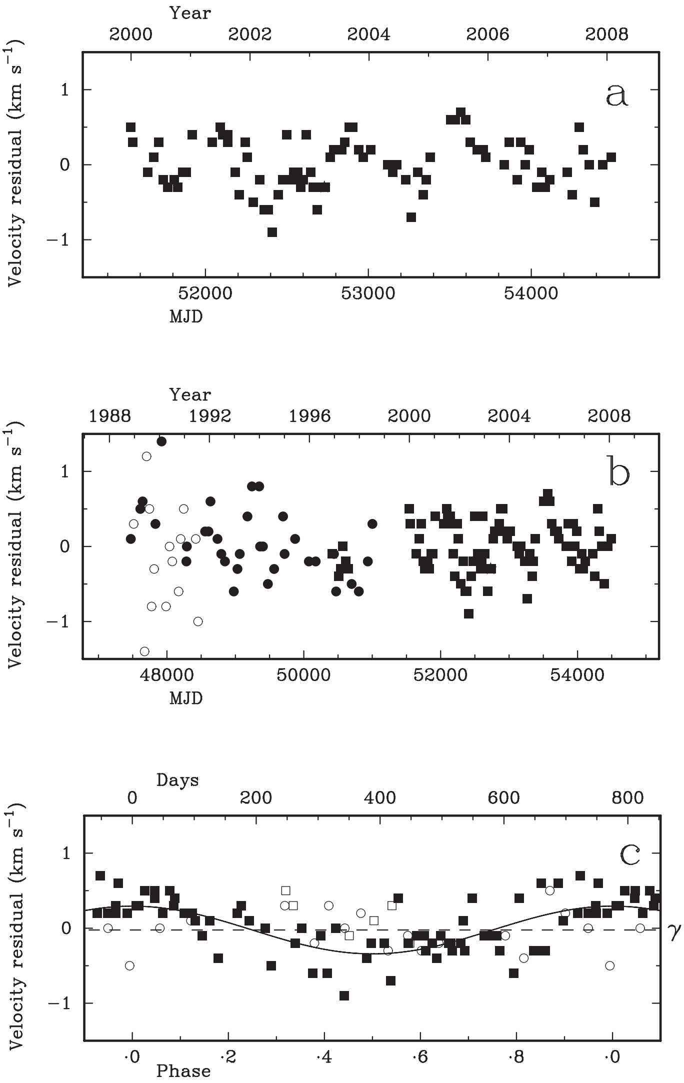

FIG. 4

Velocity residuals of 32 Cygni. As in Fig. 2 for 31 Cygni, panel **a** plots only the last 8 years' data, obtained since the Cambridge *Coravel* came into routine operation; for most of the time there is an obvious variation, but there are indications that it does not necessarily hold phase at the ends of the time interval. Panel **b** shows the whole span of the author's data and is thought to invite the conclusion that the periodicity seen in the residuals in 2001 – 2006 is not maintained throughout. Panel **c** is the result of running an 'orbit solution' on most of the data shown in panel **a**, but the first six and the last 17 of the 88 observations have been excluded from the solution and are represented here by open squares and open circles, respectively. While the solution does show that a distinct variation existed, the omitted points plotted with the open symbols show no inclination whatever to follow the same trend, reinforcing the impression gained from panel **b** that the periodicity in the variation was only temporary.

4a, because those variations do not show the strict annual periodicity that would be caused by such an effect. Indeed, if their periods were exactly two years, the phasing would actually be reversed from one year to the next. Then again, it could be considered to be somewhat disconcerting that quite similar periodicities seem to be shown by the two completely independent stars. The variations, however, are out of phase with one another, whereas *any* explanation in terms of instrumental or any other type of locally induced error would necessarily affect the two stars similarly. Thus it seems very likely that the variations seen in Figs. 2a and 4a do arise at least mainly in 31 and 32 Cyg, respectively. The non-randomness (more particularly in the case of 31 Cyg (Fig. 2b)), of the pre-2000 velocities, which were observed with an entirely different instrument, could be seen as providing additional reassurance that the effect being documented is not mainly an instrumental one.

An obvious *prima facie* explanation of the variations is that they are related to the axial rotations of the stars: that plage-like regions or prominences such as have been seen so conspicuously in chromospheric spectra, or to put it in the most non-commital terms 'azimuthal inequalities' of one sort or another, are responsible for the runs of residuals. The equatorial rotational speed of a star like 31 Cyg, whose radius has been widely regarded as being in the region of $150-200\ R_{\odot}$ (the uncertainty arising principally from that of the mass ratio and thus the transverse velocity of the B star), in the period of the observed variation, would be $8-11$ km s^{-1}. Cambridge radial-velocity traces actually give a rotational velocity of about 9 km s^{-1} for 31 Cyg, but they assume that the various types of turbulent line-broadening are no greater than they are in normal giants, which is not likely to be true in the case of such a luminous star. The OHP *Coravel* reductions utilize a specific luminosity-related calibration of line widths, and on the basis that 31 Cyg is of luminosity class II they give the rotational velocity as 6·8 km s^{-1}. Such a value would represent the rotation in 929 days of a star whose radius is 125 $R_{\odot}$, a value that is not impossible for the supergiant in 31 Cyg but would require the eclipse to be practically central and the mass ratio to be only about 1·1.

There are other published rotational velocities to be found for 31 Cyg. Pan & Tan[144] gave a value of 11 km s^{-1}, which they judged from the widths of just two lines in a spectrum taken at the Kitt Peak coudé feed; they identified the star that they observed as V695 Cyg, which is indeed the variable-star designation[145] of 31 Cyg, but they listed its spectral type as K4 V, which is far from reassuring. There are in the literature some estimates that tend to be lower. Thus Ahmad[83] claimed that the rotational velocity is only $1 \pm 1\cdot5$ km s^{-1}, and (as noted previously) Eaton[88] went so far as to make the generalization that "the K components of ζ Aur binaries rotate very slowly, if at all." Such authors, however, were comparing the apparent velocity of the *chromosphere* before and after eclipse, so their results do not necessarily pertain to the stellar photosphere which emits the light that we see and absorbs in the lines that we measure. Of course, if we were prepared to postulate multiple azimuthal irregularities we could manage with rotational velocities reduced by the multiplicity factor — but the regularity of the variation whose explanation is sought, at least in 31 Cyg, makes that idea a rather artificial one.

In any consideration of the possibility of rotation being responsible for the radial-velocity residuals, there is in the case of 32 Cyg the additional tantalizing coincidence that the pseudo-synchronous[146] rotational period at that star's orbital eccentricity is shorter than the orbital period by a factor of about 1·57, making it about 730 days or just two years, and plausibly the same as the temporary periodicity just noted. On the basis that the radius of the K supergiant is once again in the range 150 – 200 $R_\odot$, the equatorial rotational velocity would have to be 10 – 14 km s^{-1}. Cambridge radial-velocity traces, which have been admitted above to be reduced on slightly naïve assumptions, always give rotational velocities close to 9 or 10 km s^{-1}; the inclination of the rotational axis, if assumed to be the same as that of the orbit, is so high that the sin *i* factor can be taken as unity. The OHP traces, reduced in the case of supergiants in a perhaps more realistic manner, give a mean $v \sin i$ of 7·6 km s^{-1}.

One way of improving somewhat the plausibility of a rotational explanation of the velocity residuals, for both stars, would be to adopt less extravagant radii for them. The radii of 150 – 200 $R_\odot$, from which we have deduced $v \sin i$ values of 8 – 11 and 10 – 14 km s^{-1} for the respective stars, are numbers frequently canvassed in the literature, but we could consider for ourselves such information as we know about 31 Cyg. (Owing to the grazing nature of the eclipse, we are not in possession of the data to perform an analogous calculation for 32 Cyg.) The orbital elements show that the transverse velocity of the K star in that system at the time of eclipse is just over 15 km s^{-1}, so in the 63 days between the half-intensity points of the eclipse it moves 63 × 86400 × 15 km, ~ 82 Gm or 118 $R_\odot$. The eclipse chord, which sets a lower limit to the diameter ($2R_\star$) of the supergiant, is therefore $118(1 + q)\ R_\odot$, where q is the mass ratio between that star and its B-type companion, so we have the inequality $R_\star \geqslant 59(1+q)\ R_\odot$. The mass ratio must presumably be appreciably greater than 1 to have allowed the primary star to reach its present stage of evolution while its companion is still a B star, but perhaps it does not need to be more than, say, 1·1, in which case — and provided that the eclipse occurs near the equator — the minimum radius of the supergiant is 124 $R_\odot$. As noted in the last paragraph but two, at that radius the rotational velocity corresponding to the period of the radial-velocity residuals would be in full agreement with the rotation determined at OHP. It could be recalled from earlier in this paper that Wright & Huffman[76] obtained a stellar radius as relatively small as 133 $R_\odot$ when, so far from trying to minimize it, they might have liked it to agree better with the much larger radius that they estimated spectroscopically.

In any case, all that can really be done here is to present the systematic runs of residuals as facts of observation, wherein the writer sees himself as fulfilling his own rôle. If the explanation diffidently suggested here, in terms of azimuthal irregularities in the surfaces of rotating stars, proves not to be tenable, then the coast is clear for interpretational pundits to have a field day fulfilling *theirs*.

31 and 32 Cygni — an actual double star?

Stickland[147] is on record as referring to " … the twin systems of 31 and 32 Cygni (a strange coincidence in position and numeration) … both lie at about 400 pc …" — but he did not go so far as to suggest that their mutual proximity in the sky might *not* be merely a coincidence but could arise because the two systems do constitute a real physical double star, albeit one that we can hardly expect still to be gravitationally bound but to be more in the nature of a common-proper-motion pair. That idea has certainly not been widely canvassed previously, but the present writer is not absolutely the first to propose it: it was put forward almost as a matter of fact by Tremblot[52] in his seemingly forgotten paper published in (nominally) 1938. He says — here we have translated from the French — "The stars 31 and 32 Cygni, whose association in space can scarcely be doubted on account of their apparent proximity and the very similar values found for the radial velocities of their centres of mass (1), also exhibit other curious analogies." The footnote to which the superscript 1 refers says, "The proper motion and the parallax are almost imperceptible; it would be useful, by using a spectrograph of higher dispersion, to look for the presence in one case of stationary calcium lines." The purpose of the reference to stationary [interstellar] lines is not clear to the present writer, but the matter of proper motions and parallaxes of the stars is of obvious interest and some progress has been made on it since Tremblot's time.

The values selected by *Simbad* to represent the radial velocities and proper motions of 31 and 32 Cyg conceal the almost identical similarities in those critical properties. For their radial velocities it gives values of $-6 \cdot 9 \pm 0 \cdot 9$ and $-14 \cdot 4 \pm 0 \cdot 9$ km s^{-1}, respectively, so any suggestion of real association between the stars appears to fall at the first hurdle. It is clear that those values are being quoted from the *Radial Velocity Catalogue*[148], which is now more than 50 years old and was itself still quoting the γ-velocity of 32 Cyg from Cannon's 1918 orbit[107]. Cannon did not give any standard errors for the elements that he published, but the compiler of the *Radial Velocity Catalogue* assigned to his γ-velocity (as well as to that of the one given for 31 Cyg) the 'quality' *a*, the highest quality. He did that, not on the basis of any realistic assessment, but merely on the statistical grounds that it stemmed from more than ten plates. Quality *a* is listed in the *Catalogue* as having an 'average probable error' of $0 \cdot 5$ km s^{-1} and a 'maximum' one of $0 \cdot 9$ km s^{-1}, so it looks as if *Simbad* thought that it was conservatively adopting the figure for the maximum uncertainty but did not realize that the figure was not intended to be a standard error but a 'probable' one. Be that as it may, the impression is given that the radial velocities of 31 and 32 Cyg differ by many times their joint uncertainty.

That is not actually true: we have seen, above, that the centres of mass of 31 and 32 Cyg have radial velocities of $-6 \cdot 421 \pm 0 \cdot 034$ and $-6 \cdot 389 \pm 0 \cdot 032$ km s^{-1}, respectively, giving a ΔV of just $0 \cdot 032 \pm 0 \cdot 047$ km s^{-1}. That is smaller by a factor of more than 200 than the one indicated by *Simbad*! It has to be admitted that, if it is a coincidence, it is an amazingly close one. Carping critics might try to object that it is nevertheless just a fluke, or worse still a fudge, on the grounds that the underlying observations are not nearly as accurate as that and indeed they have received various 'corrections', in part empirical, that are ever so much larger than the resulting coincidence. As a comment that would be in part correct, but as a criticism it does not hold water. It is a fact that two of the three major sources of velocities have been the subject of corrections that are many times larger than the difference that is finally found between the γ-velocities of the two stars. Those corrections, however, have acted similarly in both cases — remarkably similarly, because not only are the corrections themselves the same for both stars but the numbers of observations from the various sources concerned are almost the same in the two cases. In the case of 31 Cyg the numbers of 'original Cambridge', OHP *Coravel,* and Cambridge *Coravel* observations are 15, 39, and 89, respectively, and in the case of 32 Cyg they are 15, 35, and 93, so even a large and arbitrary change in the correction applied to any of the sources could have only minimal differential effect on the result.

Then, *Simbad* gives the parallaxes of the two stars as $-0'' \cdot 007 \pm 0'' \cdot 007$ and $0'' \cdot 00294 \pm 0'' \cdot 00060$. The first value is copied from the old *Parallax Catalogue*[149] of 1952, and most people would think that it had been supplanted by the *Hipparcos* value of $0'' \cdot 00241 \pm 0'' \cdot 00057$, which we see agrees with the 32 Cyg value to within the standard error of either. The proper motions, too, are listed by *Simbad* from different sources which may not be truly comparable. In their case, *Hipparcos* may not be the best authority, owing to the possibility of confusion between the proper and photocentric motions. 32 Cyg just about completed a revolution during the time that *Hipparcos* was operating, but 31 Cyg accomplished only a minor part of its orbit then. In both cases the satellite's observations were interpreted in terms of orbital motion (most of the orbital elements being imposed from Wright's orbits[59]), but it is hard to believe that the proper motion of 31 Cyg could have been freed reliably from the effects of the orbit. More promising may be the proper motions published in *Tycho 2*[150], wherein the *Hipparcos* measurements were combined with astrometry in many catalogues going back a century, thus smoothing out the motions of the photocentres. Not wishing to lay himself open to a charge of hiding relevant data that suspicious readers might suppose failed to support his case, the writer provides here in Table V the proper motions, resolved according to RA and declination, from both *Hipparcos* and from *Tycho 2*. It is seen that the *Hipparcos* motions are formally more precise than the *Tycho 2* ones, and that all the results are entirely consonant with the hypothesis being promoted here, that 31 and 32 Cyg are indeed associated with one another.

TABLE V

Proper motions of 31 and 32 Cygni from Hipparcos and Tycho 2
Units are milliseconds of arc per annum

	Hipparcos		*Tycho 2*	
	μ_α	μ_δ	μ_α	μ_δ
31 Cyg	$+4.20 \pm 0.68$	$+1.87 \pm 0.60$	$+3.5 \pm 0.6$	$+3.3 \pm 0.7$
32 Cyg	$+3.88 \pm 0.78$	$+1.20 \pm 0.53$	$+1.8 \pm 1.4$	$+4.5 \pm 1.4$

As nearly as modern measurements can tell, therefore, the two stars are at similar distances (which are of the order of 400 pc, as noted in pre-*Hipparcos* days by Stickland[147]), and have extremely similar proper motions and radial velocities. It remains to point out that, at almost 1° apart in the sky, their minimum linear separation is about 6 or 7 pc, which is too great for them to be gravitationally bound together as a true binary star but is highly suggestive of a common origin. One has only to think of the Hyades cluster, about eight times nearer to us but with well-attested members covering an area of sky much more than eight degrees in *radius*, not to mention diameter, to appreciate that there is no reason to reject association at an analogous level between the objects that are the subject of this paper.

Acknowledgement

I am most grateful to an anonymous referee for an extremely shrewd and thorough report on this paper.

Note added in proof

In a paper that was published after this one went to press, Eaton, Henry & Odell (*ApJ*, **679**, 1490) have presented numerous radial-velocity measurements, made automatically by a 2-m telescope, of 31 and 32 Cyg. They have deduced orbits for both stars, although their data cover only about a third of a cycle of the former. Because their principal interest was to look for 'jitter' in the velocities rather than to determine the orbits, the frequency of their observations was much greater than that of the writer's; the precision also is somewhat higher. Their measurements entirely confirm, and by their exceptional abundance add detail to, the runs of residuals documented in the present paper; their orbital velocity curve for 31 Cyg mimics exactly the annoying 'swerve' of the data points seen above in Fig. 1 near the maximum of the curve. The congruity of the two entirely independent sets of results clearly validates the demonstration by both of them of a previously unrecognized behaviour of the stars concerned, at a radial-velocity amplitude that might otherwise have been considered by some to be dangerously close to the observational uncertainties of the underlying data in the present paper.

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NOTES FROM OBSERVATORIES

The Radial Velocity of HDE 276743

By R. F. Griffin
Cambridge Observatories

Radial velocities found in the literature for HDE 276743 show substantial discordances, which are not corroborated by new systematic measurements. The finger of suspicion points to two particular observations. It is thought plausible that they actually refer to HD 29957 and not to HDE 276743 at all.

HDE 276743 is a ninth-magnitude star in the eastern border of Perseus, about 3° preceding the famous eclipsing star ε Aurigae. *Tycho*[1] has given its V magnitude and $(B-V)$ colour index as $8^{m}\cdot 74$ and $1^{m}\cdot 40$, respectively. Three objective-prism observations were made of it by Boulon & Fehrenbach[2] and their collaborators at Haute-Provence in the 1950s; they classified the spectrum as K0 II and gave the radial velocity as -36 km s^{-1}, with quality 'A', which does not really mean what one might imagine it to mean: thirty-eight years later a note[3] published by the Centre de Données Stellaires (CDS) said that all the velocities given in the relevant paper[2] and field (Selected Area[4] 24) should be corrected by no less than $+17$ km s^{-1}, giving in the relevant case a revised mean value of -19 km s^{-1}. Much later, the object was observed by de Medeiros & Mayor with the Haute-Provence *Coravel*; in the published synopsis[5] of their results it is shown as having been observed four times and possessing a mean radial velocity of $-13\cdot 30$ km s^{-1}, with an r.m.s. deviation per observation of $7\cdot 97$ km s^{-1}, making it an obvious spectroscopic binary. Subsequently those observers made their individual radial velocities accessible through the CDS; meanwhile they had come across two additional measures, making six in all, which are set out again here as Table I.

Table I

'Published' radial velocities of HDE 276743

Date (UT)	*RV (km s^{-1})*
1986 Nov. 21·06	−8·6
1987 Oct. 1·06	−25·0
1989 Oct. 24·00	−9·1
1992 Aug. 26·15	−9·0
1997 Aug. 30·12	−27·1
1998 Oct. 15·14	−9·0

Promptly upon the provision of the de Medeiros & Mayor data to the CDS, the present writer seized upon certain stars, including HDE 276743, as spectroscopic binaries in need of orbit determination, and placed them on the Cambridge observing programme. It turned out, however, that he did not catch a fish on every hook! — *cf.* ref. 6. In particular, no significant change was seen in the radial velocity of the *HDE* star, which was rather surprising in view of the large amplitude suggested by the results of de Medeiros & Mayor. After a couple of

TABLE II

Cambridge radial velocities of HDE 276743

Date (UT)	*RV (km s^{-1})*	*Date (UT)*	*RV (km s^{-1})*	*Date (UT)*	*RV (km s^{-1})*
2002 Sept. 30·16	−8·0	2003 Dec. 28·96	−7·8	2006 Apr. 4·85	−7·9
Nov. 2·14	−7·9	2004 Apr. 13·84	−7·5	Oct. 27·11	−7·5
Dec. 11·13	−7·4	Sept. 6·18	−8·0	2007 Mar. 26·89	−8·0
2003 Jan. 7·09	−8·0	Dec. 20·94	−7·6	Oct. 20·14	−7·7
Mar. 14·90	−8·2	2005 Mar. 24·89	−7·7	2008 Mar. 31·86	−7·8
Sept. 24·17	−7·7	Sept. 29·17	−7·5		

years the frequency of observation of the star at Cambridge was reduced to two a year. The 17 new velocities are set out in Table II; they have a mean of −7·78 ± 0·06 km s^{-1} and an r.m.s. scatter of 0·23 km s^{-1}. They are plotted in Fig. 1 directly against time, along with those in Table I. When it is borne in mind that there is normally a zero-point discrepancy between Haute-Provence and Cambridge in the sense that the Cambridge observations tend to be nearly 1 km s^{-1} more positive, an overview of Tables I and II indicates that just two of the velocities in Table I are discordant with all the rest, which in the absence of the two outliers would be consonent with an entirely stable radial velocity. The dates of the two outliers differ by just under ten years. If their difference from the other measurements arises from motion in a binary orbit (which would have to be of very high eccentricity to allow all the other velocities to be practically the same as one another), a renewed discordance could be expected to occur after the

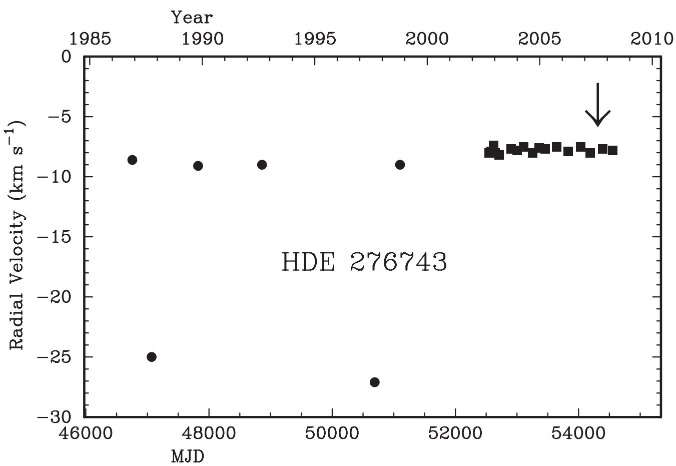

FIG. 1

Radial-velocity measurements of HDE 276743, plotted directly against time. The circles represent the measurements lodged at the CDS by de Medeiros & Mayor[5], while the squares are new data from the Cambridge *Coravel*. No adjustment has been made to either set of observations for the known discrepancy in their zero-points, which would account for the difference between the new measurements and all but two of the early ones. The arrow marks the time when a further interval equal to that between the two 'low' velocities had elapsed. In the absence of any evidence of a renewed variation then, or at all, the suggestion is made here that those velocities are attributable to a different star, for which HD 29957 is a strong candidate.

lapse of another similar interval. The writer neglected to take an observation very close to the exact date of duplication of the interval, which is indicated by the arrow in Fig. 1, but it seems quite clear that no such variation occurred then — or for that matter at any other time, at least within the duration of the writer's observing campaign.

About 15′ north-following HDE 276743 is HD 29957, a star listed by *Tycho* as being about one magnitude brighter visually but only about half a magnitude brighter in *B* because it is substantially redder even than the *HDE* star. Very little is known about it, but Boulon & Fehrenbach[2] observed it (no doubt on the same objective-prism plates as HDE 276743) and gave its type as K5 III and its radial velocity as -35 km s^{-1}, with their quality category 'B'. A recent Cambridge observation (2008 April 6·86) gave its velocity as $-26 \cdot 7$ km s^{-1}. The hypothesis is advanced here that the discordant observations attributed to HDE 276743 actually belong to HD 29957. At least at the epoch of the first discordance, the Haute-Provence telescope was operated by being set manually to the required coördinates, which were read from painted circles; the setting precision was not good enough to distinguish safely between stars 15′ apart, and it was the routine procedure for the observer to look through the finder eyepiece and to pull in the desired star onto the cross-wires after referring to a finding chart. A mistake could be made. By the time of the second discordance an improved encoder-based method of setting the telescope had been introduced, so a mistake should have been less likely. All the same, the difficulty of understanding the two discordant observations as genuine measurements of HDE 276743, together with the existence nearby of a star which could plausibly have been confused with it and which has about the right velocity, gives considerable substance to the speculation put forward here.

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CORRESPONDENCE

To the Editors of 'The Observatory'

Sherlock Holmes' Knowledge of Astronomy not even Elementary

Like Drs. Trimble and Davenhall, I have been puzzled as to how Holmes and Watson could have held a discussion about celestial mechanics, in *The Greek Interpreter*, when, in *A Study in Scarlet*, Holmes declares himself completely and deliberately ignorant of even the most basic concepts of the subject.

A further astronomical anomaly occurs in *The Musgrave Ritual*, where the Sun apparently sets in the East! "The setting Sun" is described, and illustrated by Sydney Paget, as "shining full on the … floor of the passage which Holmes has just entered" by two steps to the west.

Yours faithfully,
P. FELLGETT

Little Brighter,
St. Kew Highway,
Bodmin,
Cornwall, PL30 3DU

2008 April 23

[The Editors (and Dr. Trimble) are somewhat confused by the geography of Musgrave Mansion and are unsure of Professor Fellgett's assertion. They deem it best that this correspondence is now closed.]

REVIEWS

The Biographical Encyclopedia of Astronomers, edited by T. Hockey *et al.* (Springer, Heidelberg), 2007. Pp. 1341 (in two volumes), 28·5 × 22·5 cm. Price £307/\$499/€399 (hardbound; ISBN 978 0 387 31022 0).

I do not have any statistical basis for this impression but I do get the feeling that as astronomers ineluctably progress towards retirement (and beyond) their interest in the history of the subject grows, sometimes at the expense of their once-favourite areas of astronomical research. Could this be an attempt to see how their own work fits into the grand scheme of things or, perhaps less charitably, how it stands up in comparison with the endeavours of those who have gone before? Whatever the cause, I've certainly developed a thirst for 'astro-history' in the autumn (or at least late summer) of my years, and it was with some relish that I pounced upon this substantial work by Hockey and his many colleagues when it arrived for review.

In two volumes, (A–L) and (M–Z), we find biographical notes on more than 1500 figures with some connection to astronomy and who were born before 1918 (the youngest is Gérard de Vaucouleurs). In general, the articles begin with a short statement as to the main claim to fame of the character under discussion, followed by family and career details, and then a more substantial review of their work. The length of the piece generally relates to the magnitude of their contribution to the subject, and in most cases there is a short list of titles for further reading. The longer pieces have named authors from among the 410 contributors to the volumes, while a few very short biographies, more particularly of characters from antiquity, are anonymous. When the subjects have been known by more than one name, there is extensive cross-referencing.

While I must admit that I have not read these substantial tomes from cover to cover (at my speed of reading this review would not have appeared for several years), I have certainly browsed widely and studied in more detail most of the British entries to get some feel for the depth and accuracy of the coverage. And there are many gems to be unearthed; it was a pleasure to make the acquaintance of John Michell (1724–1793), who clearly predicted the existence of black holes ahead of the field; Maud Makemson (1891–1977), a versatile and dynamic lady I'd not met before; and the early polymath Robert Recorde (1510–1558) whose work was intrinsically interesting but with little real astronomical content.

And there we come to the first of my concerns about this encyclopedia. Of course the major figures in astronomy are present but there are quite a few whose contributions to the subject are ephemeral. John Milton (1608–1674) we can appreciate for his poetry, but I would question his scientific contribution; and why does George Thomson (1892–1975) appear, when his work is really 'laboratory' physics rather than astronomy? And the same arguments apply to many of the 'ancients' whose biographies are presented. Did they really contribute anything of unique or lasting value to astronomy or were they just speculating in a vague way on the nature of the world about them?

This leads one to ponder on the users of this work: is it for the casual reader (surely an expensive purchase) or was it aimed at serious researchers? If the latter, I wonder whether it might not have been better to dispense with the alphabetical split between the volumes and perhaps go for a temporal division: perhaps a volume running from the ancient world up to just pre-Copernicus, a 'dynamical' volume from Copernicus to the mid-19th Century, and then a modern volume. There are, for example, very many fine biographies of the Arab astronomers who doubtless kept the flame alive during the European 'Dark Ages' but whose work will be of but passing interest to researchers concerned with the development of astrophysics.

Then we must consider those who didn't make it into this compendium. For example, in more than a few of the papers from the heroic series in this *Magazine* by Roger Griffin, we encounter W. B. Rimmer, who worked at the Norman Lockyer Observatory on stellar luminosity; I'd like to have found out more about him (without having to wade through pages and pages of web-based material thrown up by Google!). Similarly for Mr. W. T. Lynne, who kept *The Observatory* supplied with a constant stream of letters and notes between 1878 and 1912. And we do find an entry for Norton, but it's not the one who gave us the *Star Atlas*. But of course it's a hard choice to make and does to some degree depend on the areas of expertise (and interest) of the commissioning editors and the available contributors.

The diverse collection of biographers naturally produces a somewhat uneven approach to the content and style of the entries, but most seem to be well researched and written. I particularly enjoyed Kollerstrom's article on Newton, which is refreshingly free of the usual idolatry! What is also uneven is the use of pictures. There are some fine monochrome illustrations of subjects from the mid-19th Century onwards (*e.g.*, of Mary Somerville) but some equally pointless engravings of ancient astronomers, like Ptolemy, in the representation of which one can have little faith. I'd have preferred to see good (*i.e.*, reliable) photos of *all* the modern astronomers, with authenticated likenesses (from paintings, *etc.*) where available for earlier practitioners.

And finally to the reliability. I have no evidence of major faults although one or two minor ones have crept in; for example, in the account of Robert d'E. Atkinson's work, we find him involved in the removal of the Royal Observatory from Greenwich to Herstmonceux Castle in *Surrey*! And a regular problem (which just happens to 'bug' me) is the use of the term 'Royal Greenwich Observatory' when what is meant is the Royal Observatory at Greenwich; the title RGO was not bestowed until that institution moved to Herstmonceux (in *Sussex*!). In the same vein, we note that J. G. Porter joined *Alfred* Lovell at Jodrell Bank, whereas a few pages later the same biographer tells us that J. P. M. Prentice joined Porter and *Bernard* Lovell to work on meteors. (We also find that Porter joined *Her* Majesty's Nautical Almanac Office in 1949.) I'm not quite sure what this tells us about the author, the proof reading, or the editorial supervision (although in this case the author was also a 'Senior Editor'!). However, the proof reading does seem to have been a problem since quite a large number of mis-spellings occur, especially with names, and we find in places an almost random use of upper and lower case letters to start names and titles. Another curiosity is the decision to spell out 'Saint' in full, whereas common usage would have pointed to 'St.', as in St. Andrews University, or St. John's College.

Nonetheless, it's a brave effort and one in which most astronomers today (especially the older ones!) will find plenty of interest, but surely only rather well-off libraries will have £300 to spend on such a luxury. — DAVID STICKLAND.

Thus Spoke Galileo: The Great Scientist's Ideas and Their Relevance to the Present Day, by A. Frova & M. Marenzana (Oxford University Press), 2006. Pp. 493, 22 × 14·5 cm. Price £19·99 (hardbound; ISBN 0 198 56625 5).

Astronomers love a good debate. And being rather disputational I am sure that present-day astronomers could argue long and hard as to who was the greatest astronomer who ever lived. For my money the answer is uncontroversial — Galileo Galilei (1564–1642).

Here we have that magical combination of a broad and over-abundant intellect and ability — observer, theorist, technician, lecturer, astronomer, mathematician, physicist, philosopher, gourmand, and musician — wedded to a prodigious curiosity, apposite experimentation, a hatred of dogma, a joy in logic, an exultation at success, an urge to push results to the limit, and a desire to publish clearly, prominently, and speedily. It is true that Galileo was very lucky to be in the right place at the right time, but his single-handed rout of Ptolemaic geocentricity and Aristotelian quintessence was masterful, and clearly something he relished greatly. His *Sidereus Nuncius* (1610) and *Dialogo sopra i due massimi sistemi del mondo, tolemaico e copernicano* (1632) must certainly make the top three of the best-ever astronomical books ever published.

The International Year of Astronomy, 2009, is going to be built around the achievements of Galileo, and there are few better ways of refreshing your memory as to what those achievements were than by reading *Thus Spoke Galileo*. Mariapiera Marenzana is an expert in Italian literature, humanity, and history and her husband Andrea Frova is a professor of physics at the University of Rome 'La Sapienza'. The book under review was first published in Italian in 1998 and has now been translated by Jim McManus. The result is a skilful combination of quotations from translations of Galileo's original publications, insightful contextualizations, mathematical explanations, and a detailed discussion of modern relevance.

The book is impressively comprehensive. Galileo's views on falling bodies, inertia, relativity, the pendulum, telescopes, lunar mountains and surface reflectance, Jovian satellites, planetary atmospheres, tides, trade winds, the Bible, faith, dogma, science and scientists, the stationary Sun, infinitesimal quantities, acceleration down slopes, the weight of air, the vacuum, sensory perception, heat, the speed of light, and comets are all dealt with in detail. The book concentrates on what Galileo says and the effect that these views had on his contemporaries, and not on what people today think about Galileo and how he was treated. It is a rewarding, illuminating, and refreshing return to primary sources and a superb summary of the work and thoughts of a genius. — DAVID W. HUGHES.

Un globe-trotter de la physique céleste — L'astronome Jules Janssen, by Fr. Launay (Ed. Vuibert & Obs. Paris, Paris), 2008. Pp. 282, 15·5 × 24 cm. Price €30 (about £24) (paperback ; ISBN 978 2 7117 7069 4 (Vuibert), and 978 2 901057 57 4 (Obs. Paris)).

If you read French and are interested in the history of European astronomy, get this volume for yourself or your library. Launay's style is fluid and fully accessible, even to non-specialists. The book, abundantly illustrated (all b/w), follows the life of Jules Janssen (1824–1907), who was, to put it briefly, the father of French astrophysics and the founder of Meudon Observatory. But this self-made man, not educated in one of the French *grandes écoles*, was also an inventor and a maker of spectroscopic and photographic instruments with two major scientific interests: the Sun and the Earth's atmosphere.

The context is the 19th Century when scientific expeditions became easier, as I am reminded daily by a venerable traveller now resting not far from my office: built by Utzschneider & Fraunhofer, that heliograph went, as an instrument of Gotha Observatory, to the Kerguelen Islands in 1874 to observe a Venus transit, before changing affiliation in 1877 to the newly founded Strasbourg Observatory (then German) and going to Argentina to observe the subsequent Venus transit in 1882. The same increase of mobility applied also to people. Before the 19th Century, a scientific expedition was the story of a lifetime: remember Le Gentil who left in 1760 for Pondicherry (India) to observe a transit of Venus and came back in 1771 only to discover that his possessions had been shared between his heirs as everybody thought he was dead.

One century later, means of transportation had dramatically improved and scientists who had the fidgets could move around much more easily and frequently. Janssen was definitely one of them: Peru, Japan, India, Siam, Caroline Island, United States, not to forget closer destinations such as Algeria and other Mediterranean countries, as well as ... the United Kingdom that Janssen visited frequently, nurturing friendship with a number of prominent scientists (Lockyer, *et al.*). Janssen actually received quite a number of British honours and was a member of several British learned societies. Towards the end of his life, Janssen himself in return founded a number of prizes and medals (French Academy of Sciences, Société Astronomique de France, *etc.*).

It would be too long to detail here all the facets of Janssen's activities and achievements, from studies of the magnetic equator to those of telluric absorption in the solar spectrum, from his first observations of the 587·49-nm helium line to his instrumental developments applied to solar eclipses and Venus transits, from his many official missions (including escaping besieged Paris by balloon) to his founding of observatories (Mont Blanc, Meudon), not to forget his farsightedness ("the photographic plate will be soon the real retina of the scientist"), *etc.* It is a shame that all but seven of his 6000 photographs of the Sun on 36-cm glass plates have been lost.

Launay's book is extremely well documented, in particular through a rich correspondence between Janssen and his wife Henriette. (An efficient postal service and the telegraph had also dramatically improved communications in the 19th Century.) Here is an outline of the book's table of contents: 'Childhood and education'; 'Spectral analysis and telluric lines'; 'Janssen and the flames of the

Sun: the 1868 key-eclipse'; 'The 1870 eclipse, the balloons and patriotic missions'; 'Janssen and the Sun in glory: the 1871 eclipse'; 'Janssen and the cinema: the 1874 transit of Venus'; 'The foundation of Paris Astrophysical Observatory located in Meudon'; 'Janssen, technician of photography'; 'From Caroline Island to Washington'; 'Janssen and Edison's phonograph'; 'The Mont Blanc Observatory epic'; 'The literary salons and the education problems'; 'Janssen and communication'; and 'Epilogue'.

A couple of appendices gather together key dates of Janssen's life and a substantial bibliography. An index of names concludes the book. Probably some readers would have benefitted from a general index — and this could be a suggestion for a possible second edition. But this reservation does not remove anything from the intrinsic interest of this formidable historical work. One would wish similar compilations be undertaken with the same care and the same luxury of details for all major characters of the past centuries.

In conclusion, this is a book as we like them: it reminds us that, if astronomy is a science, it is above all carried out by humans. Over the centuries, some of them have been flamboyant, remaining in history through their scientific achievements, but also through their daring undertakings. In the foreword of Launay's book, Jean-Claude Pecker paints a portrait of Janssen with a few brushstrokes: "physicist, inventor and constructor, mad of Sun and travelling, ... guided by his energy and his inquisitiveness". *Trahit sua quemque voluptas.* — A. HECK.

Stephen Hawking: A Biography, by K. Larsen (Prometheus Books, Amherst, NY), 2007. Pp. 215, 23 × 15·5 cm. Price $16·95 (about £8·50) (paperback; ISBN 978 1 59102 574 0).

In 1961 July, Stephen Hawking was a student on the six-week RGO summer course at Herstmonceux Castle. I was also there that year, and remember him arguing with the Astronomer Royal, getting the RGO to order the *Daily Worker* for the students' common-room, and playing croquet on the castle lawn (I even have a photograph of one of the post-lunch croquet games). Four years later I met him again, at DAMTP in Cambridge, and was horrified by the change in his physical condition. Since then our paths have diverged, and I don't suppose he'd know me now, but I have followed his career with interest and amazement. It was therefore a particular pleasure to have the opportunity to review this book, despite being in no position to vouch either for its scientific accuracy or for the details of his personal life.

The author, Kristine Larsen, is in a much better position to do that, since she was a graduate student in General Relativity in the 1980s, met Stephen at the 1986 Texas Symposium, and has clearly done a great deal of research into his life, as evidenced by the extensive references in every chapter. Her scientific background has allowed her to write a biography from the point of view of a practising cosmologist, and the resulting book is a well-judged mixture of a semi-popular account of Stephen's science, placed carefully in the context of what other people were doing at the time, and a sympathetic but honest account of his private life. There are occasional places where her American background is evident (as in the title "Royal College Observatory" for the RGO (p. 31), and some of the spelling) but I soon ceased to notice them in my reading of what rapidly became a compelling story. There have been many accounts of Stephen's life and work, both in the press and in more academic contexts, and professionals at least will be aware of his main scientific achievements, so a great deal of what is here is familiar — but Larsen's style has a directness and clarity that makes even the most familiar stories worth re-reading. And for this reader, at any rate, much of the material about his personal life is put sensitively into context and reveals the man as well as the scientist; for example, his motivation for writing *A Brief History of Time* was as much about wanting to share his work with the public as with the often-quoted need to secure a necessary extra source of income.

For the non-scientist, there is a set of well-written and helpful short appendices giving some of the necessary background physics and cosmology. There is also a glossary, a select bibliography, and an index — so this is a useful source book as well as being a good read. Those interested in the science will find an honest attempt to present the essence of the physics behind Stephen's many fruitful ideas, and will also become aware that there were mistakes along the way. Not all Stephen's ideas were immediately accepted, and some of them proved to be wrong, but that in no way detracts from the remarkable list of what he has accomplished. The new 'afterword' to this edition (the book was first published in hardback in 2005) makes it clear that he is still extremely active as he approaches a retirement that most people thought he would never reach. If you want an overview of his life and work to date, then I can strongly recommend this account. — ROBERT CONNON SMITH.

Gravitational Collapse and Spacetime Singularities, by P. S. Joshi (Cambridge University Press), 2007. Pp. 273, 25·5 × 18 cm. Price £65/$130 (hardbound; ISBN 978 0 521 87104 4).

Mathematics may be studied beneficially as a subject in its own right or viewed as the language of physics. In its latter rôle, which some might argue to be more important, it is simply a tool which must remain subservient to the physics. However, which rôle does it assume in this book? Although there is talk of seemingly physical situations concerning the collapse of stars, much of the material is highly mathematical and concerns singularities — those mathematical entities which, until recently, were taken to herald the breakdown of man-made theories. There is even extensive discussion of naked singularities, singularities that we can theoretically *see*! After moments of disbelief on reading of these, one wonders if the notion is akin to Michell's[1] dark body — a Newtonian body whose escape speed is greater than the speed of light? Also, worryingly, it seems to be implied that cosmic censorship is merely a device introduced artificially to exclude such naked singularities from what is simply a mathematical model.

The book is beautifully produced, as one might expect, but contains irritating grammatical errors, American spellings (this in a book published in Britain), and the author exhibits a tendency to indulge in overlong sentences. However, for those interested in an involved mathematical discourse on the present position regarding a theory of gravitational collapse, these irritations may not prove too distracting. For those interested more in an open-minded physical examination of the problem, the situation may not be so clear-cut. Early on, the author introduces the well-known Schwarzschild solution to Einstein's field equations. As is usually the case, the equation quoted is simply *not* Schwarzschild's solution, as is seen by examining equation (14) of Schwarzschild's original paper[2]. It might be remembered that the lack of physical significance for the so-called Schwarzschild singularity is something made clear many years ago by both Brillouin[3] and Einstein[4]. This is not a good beginning, and what follows builds on that foundation: plenty of elegant mathematics, but how much of it really relates to the actual physics of the problem?

Hence, intending purchasers should ponder their requirements carefully. Do they wish to purchase a book devoted to examining seriously a genuine physical question, or do they wish one devoted to a highly mathematical theory involving entities normally regarded as unphysical? If you wish the former, this book is possibly not for you. — JEREMY DUNNING-DAVIES.

References

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(2) K. Schwarzschild, *Sitzungberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin, Phys.-Math. Klasse*, 1916, pp. 189–196. (translation: S. Antoci & A. Loinger, arXiv:physics/9905030)
(3) M. Brillouin, *Le Journal de Physique et la Radium*, **23**, 43, 1923.
(4) A. Einstein, *Annals of Mathematics*, **40**, 922, 1939.

Cosmology, by S. Weinberg (Oxford University Press), 2008. Pp. 573, 25·5 × 18 cm. Price £45 (hardbound; ISBN 978 0 198 52682 7).

This review is in part the repayment of a long-standing debt. In the mid 1970s, I was an undergraduate who was rather keen to find out something about General Relativity, but facing the problem that my physics degree included no course on the subject. The first thing in the library to catch my eye was the colossal black bulk of Misner, Thorne & Wheeler. I spent some time perusing this fundamentalist tract and getting more and more depressed: it seemed I was expected to digest hundreds of pages of new mathematical language before being able to say the first thing about space–time curvature. No wonder the subject had a fearsome reputation, I thought. But before giving up, I picked up a slimmer blue volume entitled *Gravitation and Cosmology*, by Steven Weinberg. This was published in 1972, within a year of MTW — but what a contrast. In the space of a single page (p. 71, to be precise), Weinberg writes down Special Relativity, and then appeals to the equivalence principle to transform out of a freely-falling frame to a general one. The result immediately shows the need for a metric tensor, and derives the geodesic equation of motion involving the affine connection written in terms of derivatives of the metric. Better still, this analysis is couched in completely traditional physics language, and is immediately comprehensible: no insistence that you first learn about fibre bundles and all that. I was left dizzy by the power of the argument: starting with clear and deep physical principles, and then conjuring new physics equations almost out of thin air.

Now, Weinberg is a particle physicist by profession, and one might wonder if his view of General Relativity gained in clarity by coming to the subject from the outside. Evidence in favour of this argument comes from his three-volume set of texts on quantum fields, written during the 1990s. When these came out, I was excited by the idea of Weinberg supplying similarly simple but deep insights into his own field of particle physics. But I found these books much more formal and hard to absorb; perhaps this says more about the subject matter, or about me. So when I heard that Weinberg had returned to his 1972 subject and written a new text on cosmology, I was both excited and apprehensive — would the new book match up to the revelatory standard of the original?

The first thing to say is how much cosmology has changed in 36 years. The theory of inflation, plus the apparatus of structure formation through to the anisotropies in the cosmic microwave background, were all entirely absent in 1972. Unsurprisingly, therefore, this text is well more than twice the length devoted to the cosmology parts in 1972 (Weinberg has not revised his treatment of core General Relativity, and assumes that this will be found elsewhere if needed).

Of the ten chapters, the first three expound the basic expanding hot Big Bang in a way that would largely have made sense in 1972. The remainder cover inflation, fluctuation growth, gravitational lensing, and the CMB. These topics can be quite technical at the full professional level, and this is certainly Weinberg's approach. My impression is that he has started with the research literature, and then set off to re-derive the main results from scratch, making sure that every step is fully understood along the way. Some of this is not for the faint of heart: at one point even Weinberg admits that, "... the equations for the scalar modes are still fearsomely complicated". This is therefore not a book for the cosmological novice, but rather for someone who has absorbed the main ideas elsewhere, and who now wants to get deeper into the details. This is fine, since the other thing that has changed since 1972 is the number of cosmology textbooks. Readers who want to get a good overview of cosmological perturbations could well start out with Dodelson's fine treatment of the subject; this would then leave them in a position to appreciate Weinberg's more in-depth approach.

The other point to make is that this is a selective approach to the subject. It concentrates on doing certain specific calculations very well, and does not make much space for, *e.g.*, the latest *WMAP* results. Perhaps the most surprising issue of balance is that relatively little is said about that most fashionable of cosmological topics: dark energy. This is especially odd because Weinberg's greatest contribution to cosmology was probably his 1989 review on vacuum energy — and most especially his prediction in that paper of a non-zero cosmological constant based on anthropic reasoning. It would have been nice to see how Weinberg now views that prediction, and his feelings about the multiverse. For now, we have to make do with a textbook on just the one Universe, but still a book with many fine features, which will be invaluable for those teaching the subject at the most advanced level. — JOHN PEACOCK.

Mathematical Aspects of Natural Dynamos, edited by E. Dormy & A. M. Soward (Taylor & Francis, London), 2007. Pp. 482, 26 × 18·5 cm. Price £76·99 (hardbound; ISBN 1 584 88954 3).

Dynamo theory is a broad and very active area of research in which significant progress has been made in recent years, as already described in a number of books and review articles. Indeed, this volume is the thirteenth in the series, *The Fluid Mechanics of Astrophysics and Geophysics*, but by no means the first to examine the subject. Happily though, the present work is distinguished by its aim of filling a noticeable gap in the market — the lack of a modern comprehensive introductory text to the field, linking mathematical background with observations and the latest numerical simulations and laboratory experiments.

The book is divided into two parts, of roughly equal length, to which various authors have contributed sections or chapters. The first and more mathematical part deals with the theoretical foundations of dynamo theory while the second covers natural dynamos and their modelling, with chapters on the geodynamo, planetary dynamos, stellar dynamos, and galactic dynamos as well as results from laboratory experiments. While the target readership consists mainly of graduate students and researchers new to the field, the introductory style and wide range of subject areas will make it of interest to anyone in the broader astrophysical community interested in learning more about the subject. A word of warning, however: while the book jacket claims that no prior knowledge of magnetohydrodynamics (MHD) is assumed of the reader, the introduction to MHD provided here is weak and those unfamiliar with the subject should look elsewhere before tackling the later material.

The editors should be credited for producing a book with a logical development, coherent style, and consistent notation that is unusual for a multi-author work. Although somewhat let down by a poor index, which might limit its usefulness as an every-day reference, this volume is to be recommended to anyone with an interest in dynamo theory. — ANTONIA WILMOT-SMITH.

Particle and Astroparticle Physics, by U. Sarkar (Taylor & Francis, London), 2008. Pp. 524, 24·5 × 16 cm. Price £48·99 (hardbound; ISBN 1 584 88931 4).

The book is directed at a broad readership. It has a simple recollection of basic formalisms and the latest ideas and developments of particle physics beyond the Standard Model, such as supersymmetry and grand unification. It also covers wide-ranging topics such as extra dimensions, bosonic and superstring theory, and low-scale quantum gravity. The last section is dedicated to cosmology and astroparticle physics. Overall the aim of this book is to cover as many topics as possible with a view to understanding problems pertaining to particles and astroparticle phenomena.

There are obvious advantages and disadvantages of covering all the issues. Advantages are that the book provides glimpses of everything which can be covered by an aspiring postgraduate student in high-energy physics. The biggest drawback is that the book lacks depth in many important chapters. The particle-physics chapters are very well written but topics such as strings and cosmology have been somewhat ignored. The author has used lots of hand-waving arguments in the cosmology section, and missing out the derivation of the seed perturbations for the observed cosmic microwave background radiation is an important fault. Similarly, black-hole entropy and information loss should have been treated more fully. For instance, the author discusses at length leptogenesis, but there is not much on electroweak baryogenesis, Affleck–Dine baryogenesis, spontaneous baryogenesis, and other ways of getting matter–anti-matter asymmetry. Some issues on supersymmetric cosmology have been neglected, and gravitino and moduli problems and their ramifications have not been discussed to the extent a graduate student would probably like. The author might also have included ultra-high-energy cosmic rays and magnetogenesis, together with the physics of phase transitions.

I hope in any revised version the author will keep all these important topics in mind, and I am sure the book will improve over time. In spite of these criticisms I still think the book provides valuable information to postgraduate students and new researchers in the field. — A. MAZUMDAR.

Statistical Challenges in Modern Astronomy IV (ASP Conference Series, Vol. 371), edited by G. J. Babu & E. D. Feigelson (Astronomical Society of the Pacific, San Francisco), 2007. Pp. 448, 23·5 × 15·5 cm. Price $77 (about £38) (hardbound; ISBN 978 1 58381 240 2).

This volume reports events of the fourth conference on statistical challenges in modern astronomy (SCMA), which took place in 2006 June on the campus of Penn State University. Held every 5 years, SCMA has seen a dramatic surge in interest in the burgeoning field of astrostatistics in recent years. Sophisticated methods are being developed to deal with present and future statistical challenges arising from the exponential growth in the size and complexity of astronomical data. This volume is particularly interesting for its cross-disciplinary aspects, bringing together many excellent contributions from experts in three key areas: statistics, astronomy, and particle physics. The editors have achieved the difficult goal of producing a collection of works that are unified by their underlying attention to statistical aspects, without obscuring the physics of the problem at hand.

The themes covered can be broadly structured in three main topics: statistical issues in dealing with extremely large data sets (including microwave-background maps and galaxy surveys), small-N problems (for example, photon-number counts in the presence of a background), and detection of periodic signals (relevant for gravitational-wave searches and extrasolar-planet detection). One recurring problem is to quantify in a correct way the 'significance' of a signal. This is discussed from several angles, for example, in connection with gamma-ray counts, exoplanet discovery, or detection of periodicity in pulsar searches. Several very different statistical techniques are proposed in the different contexts, ranging from Bayesian model selection (Ford & Gregory) to extreme-value theory (Bickel, Kleijn & Rice). The resulting impression is that successful statistical analysis remains very much a craft, requiring a skilful merging of technical ability with genuine physical insight. As such, this volume offers many excellent examples of good statistical modelling. In keeping with the practice of the statistical community, papers are followed by a discussion that illuminates the subject from a different perspective. Many of these rejoinders provide a critical commentary and highlight outstanding issues.

Part 7 rounds off the volume by offering three different summaries: by a particle physicist (Lyons), by an astronomer (Lahav), and by a statistician (Berger). It transpires that astrostatistics is a complex field with many open methodological questions, many of which have never been faced by statisticians before. In particular, astroparticle physics presents the challenges of both astronomy and particle physics, and is a particularly suitable terrain where the growing collaboration among experts from both fields is likely to bear fruitful results.

This is a book that will appeal to astronomers, astroparticle physicists, and cosmologists who are looking for new and creative perspectives on the subject of data analysis and model inference. It is warmly recommended to anybody who believes that statistical analysis can (and should) be much more than χ^2 fitting. — ROBERTO TROTTA.

Convection in Astrophysics (IAU Symposium No. 239), edited by F. Kupka, I. W. Roxburgh & K. Lam Chan (Cambridge University Press), 2007. Pp. 524, 25·5 × 18 cm. Price £62/$117 (hardbound; ISBN 978 0 521 86349 0).

Convection is a phenomenon familiar to anyone who has boiled an egg; its power as a transport process is apparent to anyone who has watched a thunderhead. However, a quantitative description remains a challenge for engineers, oceanologists, meteorologists, and astronomers. Thirty years ago, local-mixing-length theory (MLT) was nearly as good as it got for stellar structure, though valiant efforts were made to develop non-local theories and other variants.

Convection in Astrophysics demonstrates that the subject has progressed enormously. Sophisticated treatments are available to deal with a huge variety of convective environments in stars — though MLT remains the standby for stellar structure. It is no surprise that the cores of massive stars, the envelopes of brown dwarfs, differential rotation in a magnetic Sun, and the ionizing layers in the atmospheres of white dwarfs offer environments ripe for exploration with a battery of new techniques and modern computational tools.

The proceedings of this IAU Symposium do justice to a subject which may be coming of age. This thoughtfully-prepared volume offers extended pedagogical reviews as well as tantalizing glimpses of contemporary research. It is wide-ranging in subject and varied in pitch. The inclusion of discussion adds useful texture. *Convection in Astrophysics* should provide a seminal reference for a generation of astronomers. — SIMON JEFFERY.

The Universe in X-Rays, edited by J. E. Trümper & G. Hasinger (Springer, Heidelberg), 2008. Pp. 499, 23·5 × 15·5 cm. Price £69 (hardbound; ISBN 978 3 540 34411 7).

At first glance, this looks like the proceedings of a conference, but it is not. Rather, Trümper & Hasinger have persuaded 24 colleagues to write a total of 26 self-contained chapters on various aspects of X-ray astronomy from proportional counters to gamma-ray bursters. They have done some of the writing themselves, and the focus of the volume is, as you might expect from the editors' own very important contributions to X-ray astronomy, largely instrumental and observational. Thus there are many mentions of *ROSAT*, *ASCA*, *RXTE*, *BeppoSAX*, *Chandra*, *XMM–Newton*, *INTEGRAL*, *Swift*, and *Suzaku*, and relatively few of, for instance, synchrotron, inverse Compton, or jet collimation (none of these are index entries, though charge exchange and comptonization make the cut). Each chapter has separate references, and 'house style' has required that they be cited as (1) to (88) or more, though most of the authors have also alphabetized from Abramowicz to Zwicky, or thereabouts, so that the *ApJ, A&A, MNRAS, AJ, etc.*, system of citing as (Night & Day 2004) would have been possible. Most chapters have at least a few historical remarks at the beginning, though I was told by an expert just a few days ago that the 1966 report of X-rays from M87, described in Chapter 23 as the first detection of a cluster source, was probably not true. If you are at one of the (diminishing number) of institutions whose libraries have standing orders for this series, you may well find yourself snitching figures, tables, and numbers for lectures, homework problems, and such. If not, you may have to do without. — VIRGINIA TRIMBLE.

Cosmic Frontiers (ASP Conference Series, Vol. 379), edited by N. Metcalfe & T. Shanks (Astronomical Society of the Pacific, San Francisco), 2007. Pp. 364, 23·5 × 15·5 cm. Price $77 (about £38) (hardbound; ISBN 978 1 58381 320 1).

Cosmic Frontiers, like many other conference proceedings in the ASP conference series, constitutes a collection of many outstanding extra-galactic studies done within the last few years and presented during a very nice summer conference in Durham (UK). The high level of the presentations and subsequent papers in the proceedings is largely due to a careful selection of renowned and acknowledged researchers in the fields covered by the meeting itself.

The conference focussed mainly on the first half of the history of the Universe, with the book following the same natural time flow, starting obviously with the (then) latest cosmic microwave background results from *WMAP*. The preface of the book gives a rather accurate summary of the work presented during the conference and in the proceedings, and is well worth reading.

However, like all conference proceedings, there is a natural bias in their content, in the sense that they do not necessarily reflect the whole picture of the field being addressed. This is an unavoidable fact, once taken into account that only a limited number of contributions can be considered in a week-long conference, with no parallel sessions. Certainly there is no attempt to match the in-depth work that a proper review can give. Nevertheless many of the papers presented reflect rather well the 'current' status of the observed Universe. In the conference, particular emphasis was given to the broad subject of galaxy formation and evolution, including separate sessions, and hence chapters, on AGN, feedback, and environment, which take up more than half of the whole proceedings. Let us not forget the many results presented from on-going imaging and/or spectroscopic surveys, like, *e.g.*, UKIDSS, RCS, SDSS, 2SLAQ, *VIMOS* on the *VLT*, STAGES, Combo-17, *etc.* These are all precursors of future larger and deeper surveys, the main topic of the EU funded SISCO network, of which 'Cosmic Frontiers' was the concluding conference.

Finally, like all conference proceedings, this one also contains the very much unavoidable official conference photograph, taken outside the 14th-Century Great Hall of Durham Castle. What better way to remember who was there and how we all looked then! — PEDER NORBERG.

Why Galaxies Care About AGB Stars: Their Importance as Actors and Probes (ASP Conference Series, Vol. 378), edited by K. Kerschbaum, C. Charbonnel & R. F. Wing (Astronomical Society of the Pacific, San Francisco), 2007. Pp. 506, 23·5 × 15·5 cm. Price \$77 (about £38) (hardbound; ISBN 978 1 58381 318 8).

What is interesting about asymptotic-giant-branch (AGB) stars and why are they important? Why study them? What can they tell us about galaxies? If you are curious about the answers to any of those questions then these proceedings of an international conference, held in Austria, will be of interest. The conference was held at the University of Vienna in 2006 August and was used by the AGB community to show the relevance of its work to other fields of astronomy. This resulted in the bringing together of about 140 scientists from a range of disciplines, in particular the evolved-stars and the galactic-evolution fields of research, to identify the important problems which need attention.

All stars with an initial mass $\leqslant 8\ M_\odot$ will go through the AGB phase of stellar evolution. AGB stars are bright and lose mass at a considerable rate, producing the majority of dust in galaxies, therefore playing an important part in the production and ejection of new materials. This makes them important drivers of galactic chemical evolution. They can also be used to trace the structure and extent of star-formation activity of a galaxy. An understanding of AGB stars is therefore essential to the understanding of galaxies — to their structure, chemistry, and evolution.

These proceedings show the latest (up to 2006) steps in AGB-star research. Although AGB stars dominate, there are papers on their descendants, post-AGB stars and planetary nebulae, and on super-AGB stars ($7-11\ M_\odot$; see the RAS Specialist Discussion Meeting on 2008 February 8 for more on these) and also on galactic evolution and stellar populations. The editors have divided the contributions (50 oral papers and 55 poster papers) into three sections, each starting with the invited talks, followed by the questions raised at the meeting and then the poster presentations. This volume does not include an introduction to the structure, characteristics, and evolution of AGB stars — an introductory talk was included at the conference but is not repeated here. However, an introduction to the subject of AGB stars can be found in the book *Asymptotic Giant Branch Stars*[1].

The papers describe the exciting questions still to be answered and suggest instrumentation to tackle them. Preliminary results are presented from IR survey programmes in progress at the time, such as that using *Spitzer*, observing nearby AGB stars, and with *AKARI* providing a sample of AGB stars in the LMC. The first section of the proceedings, entitled 'Nucleosynthesis, mixing, abundances', starts with an interesting summary, by John Lattanzio, of some of the progress and problems with AGB-star models. The second section of the proceedings focusses on mass loss from AGB stars and is the largest section, featuring 38 poster papers, many of these on individual objects. The final group of papers addresses AGB stars and galactic evolution. Monica Tosi begins this section with a review featuring some examples of why galaxies, and therefore galaxy modellers, care about AGB stars and some of the problems which need addressing. Other contributions cover star-formation histories of galaxies probed by AGB stars, population-synthesis models, stellar-population studies in our Galaxy (and beyond), and papers on related objects. With such a range of topics I was disappointed not to see a conference summary or a detailed subject or object index. However an author index is included.

This was an interesting conference that provided a good review of recent research in these areas. It will be of interest to anyone entering or working in these fields, providing useful background, showing the techniques used and the advances in AGB research. — Kim Clube.

Reference

(1) H. J. Habing & H. Olofsson, *Asymptotic Giant Branch Stars* (Springer, Heidelberg), 2004.

From Stars to Galaxies: Building the Pieces to Build up the Universe (ASP Conference Series, Vol. 374), edited by A. Vallenari, R. Tantalo, L. Portinari & A. Moretti (Astronomical Society of the Pacific, San Francisco), 2007. Pp. 512, 23·5 × 15·5 cm. Price $77 (about £38) (hardbound; ISBN 978 1 58381 309 6).

Cesare Chiosi is well known for his contributions to the study of stellar evolution and the chemical evolution of galaxies. This volume is the proceedings of a conference in his honour held in Venice in 2006 October. As might be expected from Chiosi's interests, the papers cover a wide area of astronomy — from star formation through to stellar explosion, stellar abundances, the structure of the Galaxy, and on into other galaxies and their stellar populations. As is perhaps inevitable with conference volumes, there are quite a lot of rather insubstantial one- or two-page articles, of a type which may not survive the era of electronic publishing without more backup, and quite a lot of 'work in progress but no real results yet'. But there are several excellent review highlights. I'd particularly pick out Alan Dressler's well-argued (but not necessarily proven!) contention that the mass of a galaxy is really its overriding property — determining its position in the Hubble sequence and the history of its star formation — and he marginalizes the rôle of mergers in galaxy evolution. A rather broader and agnostic view is taken by Guy Worthey, commenting "it sometimes seems as if we know less about galaxies than we did a decade ago" — perhaps it is just that we are now in a position to recognize the complications. The arcane problems of the interpretation of element abundance ratios in stars get their fair share of discussion. It is also clear from several papers that the study of stellar populations, both through integrated spectra and through study of deep colour–magnitude diagrams, is coming of age. It is at last beginning to yield reasonably reliable results on ages and compositions of sub-populations — and (perhaps most importantly) some realistic recognition of the uncertainties involved. The volume is reasonably well produced, with a number of informal and enjoyable 'spot-the-astronomer-you-know' pictures — the kind you eagerly scrutinize to see who is ageing best. Overall, some good reviews, but probably another volume for the library rather than the personal shelf. — MIKE EDMUNDS.

Cosmological Enigmas: Pulsars, Quasars and Other Deep-Space Questions, by M. Kidger (Johns Hopkins University Press, Baltimore), 2007. Pp. 225, 24·5 × 18·5 cm. Price £20 (hardbound; ISBN 0 801 88460 8).

It is always a pleasure to read a book that is written by someone who has such a passion for the subject. As well as being a practising astronomer at ESA, Kidger has a wonderful ability to tell stories and weave history and current practice into the ten chapters of this excellent book. The book is a sequel to *Astronomical Enigmas: Life on Mars, the Star of Bethlehem and Other Milky Way Mysteries*. In *Cosmological Enigmas*, each chapter is a self-contained review of a number of questions, which include: 'How are stars born?', 'How far is it to the stars?', 'How old is the Universe?', 'Is there anybody out there?', and 'How do we know that there was a Big Bang?' In doing so they cover objects such as pulsars and quasars, which are highlighted in the book's subtitle.

This very well-written volume seems to be aimed at the beginner, but most likely would suit someone who already has some basic background knowledge in astronomy and cosmology. It might also suit the professional astronomer who needs a quick overview of fields outside their normal area of expertise. What is particularly pleasing, as well as being very useful, is the extensive set of end notes (covering 24 pages) which amplify some of the salient points within the text, many of which cite academic research papers. In addition there is, at the end of each chapter, a suggested reading list, structured into sub-sections — 'Popular books', 'More advanced reading', 'Science fiction', and 'Internet sites' — with detailed descriptors for each entry, many of which carry the author's personal comments on how readable and accessible they are. What really works is the author's ability to weave into the text his own work (on binary black holes, such as OJ287), which gives the reader an insight into that inner sanctum of astronomical research.

The overall impression is of a book that is worthy of inclusion in any bookcase. On a final note, it has a superbly designed and eye-catching dust jacket! — JOHN GRIFFITHS.

An Introduction to the Physics of Interstellar Dust, by E. Krügel (Taylor & Francis, London), 2008. Pp. 387, 24·5 × 16 cm. Price £39·99 (hardbound; ISBN 1 584 88707 9).

This text is an abbreviated and modified version of *The Physics of Interstellar Dust* by the same author. His objective was to produce a book which is more accessible to students; and, at £40 for a copy, it is certainly much more affordable than the original. At almost 400 pages in length, it remains a substantial book; but those who do purchase personal copies will be rewarded with a well-written and balanced text, which covers in detail most of the properties and the consequences of cosmic dust. Whilst much of the discussion is orientated towards the interstellar medium, as the title suggests, there is some consideration given to dust in the Solar System; and 'dust' is interpreted broadly, to include polycyclic aromatic hydrocarbons.

Our knowledge of the dust in the cosmos has advanced remarkably in recent years, largely but not exclusively thanks to spectroscopic observations in the infrared. Dust particles not only absorb, scatter, and polarize radiation but participate also in the chemistry and the dynamics of the interstellar medium. We have now a fairly good knowledge of the composition of interstellar grains and their ice mantles, and we recognize that dust particles will grow, in some conditions, and be eroded, in others. On the other hand, we have more limited knowledge of the structure of grains; and their structure is certainly much more complex than the idealized models of spherical grains might lead us to believe.

The comprehensive treatment of the properties of dust particles that is undertaken in this text involves many different areas of the physical sciences: solid-state physics, classical electrodynamics, classical mechanics, optics, thermodynamics, and the theory of chemical bonding. Much of the book is devoted to considering the background physics. Although a lot of this material can be found elsewhere, it is valuable to have it brought together, in a coherent and uniform manner, in a single text. I suspect that many students, commencing their studies for a higher degree, will find the material forbidding; but physics is a demanding subject, and if its demands are not met there are no rewards in the form of progress in the discipline. The author bites this bullet and sets about the task of covering the requisite material in a commendably workmanlike manner.

This book should find a space on the shelves of every university and research-institute library. It deserves inclusion in the personal collections of many astrophysicists. The use of cgs units is a recognition of the realities of the astrophysics research literature but may prove to be an initial obstacle to students brought up with SI units. Also, if authors choose to list physical constants in their books, they should ensure that they are quoted correctly: the Bohr radius given in Appendix B3 is ten times its correct value. — DAVID FLOWER.

The National Virtual Observatory: Tools and Techniques for Astronomical Research (ASP Conference Series, Vol. 382), edited by M. J. Graham, M. J. Fitzpatrick & T. A. McGlynn (Astronomical Society of the Pacific, San Francisco), 2007. Pp. 713 + CD ROM, 23·5 × 15·5 cm. Price $77 (about £38) (hardbound; ISBN 978 1 58381 327 0).

The Virtual Observatory (VO) movement aims to develop the computational infrastructure within which astronomical research will be conducted worldwide, and, by achieving a global federation of astronomical-data resources, to make possible for everyone the kind of data-intensive, multiwavelength analyses that have been too taxing hitherto for all but the most dedicated and IT-savvy. This book presents a useful summary of the progress that has been made towards achieving those ambitious goals during the first five years of VO development, and is a worthwhile read/skim for anyone not familiar with the VO.

The book is based on material delivered at summer schools run in 2004, 2005, and 2006 by the US National Virtual Observatory (NVO) project. These gave researchers and software developers a week-long introduction to the VO and then required them to spend a couple of days applying their new-found knowledge to publish a dataset or tool to the VO, or to undertake a scientific analysis using existing VO-accessible tools and data. The short reports on these mini-projects are some of best chapters in the book, showing how it is possible to start doing real science with the VO with relatively limited preparation. Another strength is the explanation of the basic technologies — XML, SQL, and web services — which underpin the VO, and which must be understood, at some level, by those wishing to exploit its full potential. Such people will also benefit from the software included on the CD accompanying the book, the many (Java, Python, and C#) code snippets included in the text, and the detailed 'How to…' chapters; these will have a limited shelf-life, given the speed of VO development, but, for the moment, probably comprise the best practical introduction to the VO.

My only significant criticism of the book is its US-centric view of the VO. This is, perhaps, inevitable, given its origins, and its misattributions can be excused, but it is instructive to think how different a book would have been produced by the team behind AstroGrid, the UK's VO project. In terms of funded effort, NVO and AstroGrid dominate the VO world, and they have adopted very different approaches: NVO has developed a plethora of user tools, whereas AstroGrid has concentrated on developing a comprehensive VO infrastructure. By focussing on the former at the expense of the latter, the book risks failing to convince readers that the grand VO vision is attainable in its desire to prove to them that useful things can be done with the VO already. — BOB MANN.

Rejuvenating the Sun and Avoiding Other Global Catastrophes, by M. Beech (Springer, Heidelberg), 2008. Pp. 225, 23·5 × 15·5 cm. Price £19·50/$29·95/€24·95 (paperback; ISBN 978 0 387 68128 3).

If you have ever mused over what will become of Mother Earth when the Sun evolves away from the main sequence, here is a book to carry you further into the realms of futuristic science than you have probably hitherto visited. Beech's thesis on averting, or at least delaying for a useful interval, the eventual evolution of the Sun stems from classical stellar-evolution theory, which he describes qualitatively at some length. The introduction of enhanced mixing, pressure, or mass loss into a solar model delays or avoids the turnoff towards the red-giant branch, and all that asteroengineering has to do is to make it work. The day of doom for our civilization can at least be postponed.

The book discusses the probabilities and devastation levels of known celestial-based disasters from supernovae to large meteorites. Much space is given to explaining the fundamental relationships that control a star's peregrination through the H–R diagram, and to ranking the efficiencies of postulated rejuvenation schemes intelligently, but the major question of how to set about all this is never asked. With each ensuing chapter I anticipated a discussion on the feasibility and performance of various techniques that were hinted at, but I reached the end of the book disappointed. Just how those modifications will be effected is conveniently ducked by assuming that future asteroengineers will have mastered the technologies needed. I was disappointed too that the also-rans in the title (the 'other' global catastrophes, which must surely include cleaning up our terrestrial atmosphere) only get minimal coverage. Even dissuading NEOs from heading our way is dismissed rather peremptorily as a well-known technique. And it does seem a little rash to propose homogenizing the solar interior by capturing and shooting into it a small black hole, when by definition such a thing can be neither seen nor located, and where a near approach to the Sun will incinerate what the black hole has left of its transporter.

The author makes commendable efforts to present a mathematical subject in a non-mathematical way, though he may in fact slightly irritate both camps; equations are present, though most are written within the text, which reduces their clarity, and — worse — they are often split between lines, which impairs their sense. Since this is not a textbook, it might have been more readable if relevant equations and workings were in an annexe for verification by those curious.

However, two omissions from Beech's theoretical 'solutions' made me question the usefulness of such a book at all. First, there is no mention of the obvious challenges which any civilization — however technologically competent — must address in such a programme. It is all very well to discuss models that could bring about the desired changes to an ageing Sun's structure and properties, but an overarching condition is surely that any engineered change have no adverse effect upon the Earth *while in progress*. The consequences of the Maunder Minimum led to widespread harvest failures even though the modifications that caused it were small. The need for total control over changing the solar output adds a new dimension of complexity that simply cannot be dismissed. Portia-like, we may remove material from the Sun but we may not influence the ozone equilibrium, the ionosphere, or a day's length. The other omission was the moral question: should a civilization's decision to tamper with its only life-support machine be democratic or autocratic? Whose designs win the technology contracts, and who is in control, or to blame, if things do not go as planned?

Beech takes a lot for granted. He postulates that we can confidently leave the worry of the know-how to our descendants, who will have all the messy technological facts worked out by the time they need to *do* something. He assumes that what we understand today in terms of stellar structure and evolution are correct in detail (though we cannot even forecast terrestrial weather correctly); the metal content and inhomogeneity of the Sun and the heights of its various theoretical 'zones' are crucial to his arguments but their coarse parametrization and uncertainties are nowhere questioned. Further, in arguing the possibility that other, older civilizations have already asteroengineered their parent star, 'terraformed' their home planet to modify its orbit, or migrated, he attributes to them the flatland knowledge which is peculiar to our own thought processes; to other advanced civilizations, mathematics and astrophysics (if indeed present) may be quite different patterns of thinking and reasoning.

The production contains several typos and mis-chosen words, and occasional printing errors. But Beech writes well, with a slight whimsical humour (though he uses parentheses too often), and by pushing all the nasties well under the carpet he offers a comfortable read. Be wary, though, of anyone who wants to discuss the subject with you. You may be unaware of his or her provenance. — ELIZABETH GRIFFIN.

Ice, Rock, and Beauty: A Visual Tour of the New Solar System, by D. Brodie (Springer, Heidelberg), 2008. Pp. 152, 28·5 × 21·5 cm. Price £19·50/$39·95/€32·95 (hardbound; ISBN 978 0 387 73102 5).

This book is a collection of images of the smaller bodies in the Solar System. The greater part is taken up by more than sixty images, some in black and white, some in colour, true or false, and a few graphics. Nearly all the images are from space and are credited to NASA. Subjects range from meteors to Titan, including a wide range of ice and rock, as befits the title. An enormous number of similar images have been taken in the last decade, but none with artistic beauty in mind. Some of the beautiful images presented here are the results of happy accidents. One such is Phobos, shown in black and white, with the crater Stickney in deep shadow, emphasising its size and depth. The artist comes into his own in composing false-colour images. Techniques of colour manipulation are well explained, using Europa as an example. Europa also figures in one of the most beautiful images where the double ridges are displayed as a golden filigree woven into the smooth icy-blue plains.

Each image is on the right-hand side of an opening; some are whole page but many are smaller, the smallest occupying roughly a quarter of a page, with the rest left blank. The left-hand page holds an essay on the image and two factual boxes, one containing details of the object imaged, the other details of the image itself and a schematic diagram of the Solar System indicating where the image on the right fits into the big picture. The prose style is vivid and enthusiastic, well matching the images. It is difficult to discern in what order, if any, the pictures are placed. The three pictures of impact craters on the Earth are on pages 25 (Manicouagan), 45 (Aorounga), and 85 (Chicxulub).

The picture pages are preceded by ten, separately paginated, pages. The same pictures appear as in the latter part of the book, but much smaller. They are grouped by logical themes accompanied by further expanded explanatory text. However, there is no cross-referencing to the latter part of the book and the only way to trace a theme between the two paginations is *via* the index.

There is only a small overlap between the target readership for this book and the readership of *The Observatory*. The pictures are beautiful but the facts, while accurate, are not encyclopaedic and most will already be familiar to many readers. — DEREK JONES.

The Rough Guide to the Universe, 2nd Edition, by John Scalzi (Rough Guides, London), 2008. Pp. 401, 19·5 × 13 cm. Price £10·99/$16·99 (paperback; ISBN 978 1 84353 800 4).

For a handy-sized, no-nonsense introduction to astronomy, *The Rough Guide to the Universe* is hard to beat. In this one-stop shop you get a comprehensive rundown of all the basic factual material you would expect, together with an excellent section of star charts. In this second edition, John Scalzi has brought the original 2003 version up to date with, for example, references to the *Cassini–Huygens* mission to Saturn and Titan, and to dwarf planets.

Part 1, called 'The Universe', is the main text section and despite its title is geared strongly towards the Sun and planets. The Solar System receives about three times more coverage than the rest of the Universe put together. For beginners' books at this level, though, an emphasis on the Solar System is not unusual and reflects what interests most newcomers to astronomy. Budding astrophysicists and rocket scientists obviously need to move on, perhaps using the 'Resources' section at the back to identify further reading and recommended web sites.

Scalzi's style is direct and engaging. He has the knack of keeping it simple without compromising the accuracy, and he mostly gets it right. Somehow, though, when he trawled the 2003 edition to identify where updates were needed, one paragraph in the Moon chapter obviously slipped through the net. It seems astonishing to read now in a newly published book an assertion that the USA has shown no interest in sending humans back to the Moon! How things can change in five years.

Part 2, the 'Star Charts', takes up nearly 40 per cent of the book and is well done. The clear white-on-blue maps are easy to read and organized by constellation, some smaller adjacent constellations being combined on a single page. Opposite each map there is information about the origin of the constellation(s) featured, associated legends, and notable sights for amateur observers.

Anyone taking up astronomy as a hobby or interest, whether actively from the back garden or just from an armchair, and whether young or old, will get two for the price of one from *The Rough Guide to the Universe*: an entertaining read and a handy reference book. — Jacqueline Mitton.

Tourists in Space: A Practical Guide, by E. Seedhouse (Springer, Heidelberg), 2008. Pp. 360, 23·5 × 17 cm. Price £19·50/$34·95/€26·95 (paperback; ISBN 978 0 387 74643 2).

The first space tourists were the handful of people who each paid $20M to be trained as astronauts and visit the *International Space Station*. This book sets out to give readers an account of the range of possibilities likely to become available in the next few years — and what they would be letting themselves in for. The author considers two classes of excursion: sub-orbital trips to the edge of space (100 km), giving spaceflight participants, as the punters are officially designated, about 5 minutes' experience of microgravity, and orbital trips, when they will fly in low-Earth orbit for some days. This effectively divides the bulk of the book into two sections owing to the very different physiological demands on the participants and training requirements.

First, we have a survey of nine companies offering sub-orbital flights, costing around $200K, in a variety of craft based on a range of mission architectures. This is a fast-moving area: there are critical test flights planned for 2008–09 and useful web sites are provided for readers to follow developments. We are then given details of the medical requirements and a generic 4-day training programme including spacecraft systems and basic space physiology: effects of reduced pressure, microgravity, and high-*g* toleration. This is a good account, but I found the detail in the tables, down to timings of meals, coffee breaks, 'meet and greet', checking into hotel, *etc.*, got in the way of the story. It was frustrating to be told of "Hypobaric-chamber orientation" (Practical Module HP1) but not of the actual experiences in the chamber (modules HP2 and HP3, after coffee), or during the centrifuge runs (modules GP1 and GP2).

Orbital flight is significantly more expensive, but the space industry has the incentive (the NASA COTS programme) of developing orbital transport services to the *ISS* required after the retirement of the Shuttle. The likely providers of orbital space excursions (at around $5M a ticket) are reviewed, and the author gives an extended account of the medical requirements and rationale behind them. Then follows the training programme. Whereas the five paying visitors to the *ISS* each required 900 hours' training, the author believes this will not be marketable to punters, and sets out a 248-hour programme. It would be interesting to know its provenance and status, *e.g.*, was it devised in collaboration with the FAA or one or more of the space companies? There is a lot of information here, but some of the tables from external sources require to be tied better to the text or to have fuller captions, *e.g.*, what is column "ITD" in Table 6·6? I could not find the table in the reference cited. Both *rem* and *sieverts* are quoted in tables in the section on radiation and space weather, but we are not told how they are related, and there is a misprint in the definition of the *rem*. This section is rather unclear. Better are the sections on high-*g* tolerance and high-altitude physiology, but Fig 6·10 needs explanation of the arrows superimposed on what looks like a Stohl curve. Survival training, to prepare for emergency landing ("contingency de-orbit") in the arctic, desert, sea, or the tropics, is given 27 pages. Finally, we are introduced to the vehicle and its systems.

The author writes with a good balance of enthusiasm and caution. He does not minimise the risks ("... any assessment uncertainty is highly substantial."), placing them in the context of other adventurous activities such as climbing Mt. Everest, where the fatality rates are currently comparable. Altogether, he has provided a useful tourist guide (*Space on a Million Dollars a Day*?) for potential and armchair travellers. — PEREDUR WILLIAMS.

Philip's Guide to Weather Forecasting, by Storm Dunlop (Philip's, London), 2008. Pp. 176, 20 × 12·5 cm. Price £9·99 (paperback; ISBN 978 0 540 09026 6).

This book's title, an accurate description of what it is about, also creates a problem for the book itself so far as the average reader is concerned. Who, nowadays, actually needs to work out their own weather forecasts? Weather forecasts are one of the few items which are available in abundance and at virtually no charge, *via* television or radio or newspapers, or on-line, and mostly to a much higher level of accuracy than can be achieved by even a gifted and proficient amateur.

The book itself is attractively presented with many colourful illustrations, some of them memorably beautiful cloud or landscape shots and others being well-thought-out and helpful diagrams, and the text is thorough and comprehensive, packing a great deal of information, in rather small print, into its 176 pages; but therein lies the rub. The text is not exactly light or beguiling reading; it is all well and clearly expressed, but most readers will feel a need to make notes as they go along, with revisions and summaries from time to time, in order to get the best from its perusal. And since professionally produced weather forecasts are available twenty-four hours a day and at no cost (other than sometimes being rather depressing), the question inevitably enters the reader's head, "Is the conscientious intake and assimilation of this wide range of wind-and-cloud-related information worth the effort?".

This is more a criticism of the subject than of the book. The aptly named author is a meteorological expert who comes across as both knowledgeable and enthusiastic about all aspects of weather forecasting. His remark at the beginning of his chapter about clouds is illuminating — he says, "For some reason, people tend to think that recognising clouds is difficult, but it need not be", explaining that there are only "ten types (genera), 14 species and nine varieties, as well as six supplementary features". He goes on to describe the various cloud forms thoroughly and well, with some strikingly good photographic illustrations, but he does not, I think, make proper allowance for the reader's increasing breathlessness in keeping up with him.

Essentially this is a book for reference rather than assimilation in one or even three sessions. On this basis I found the chapters on the origins of weather, weather systems, and local variations satisfyingly informative, and I certainly now know much more about clouds than I did before, though species recognition remains more difficult for me than the author thinks it should be. As noted above, the book is attractively presented so as to be a pleasure to look at; it has a useful glossary, and a 'Further Information' section which provides the reader with a booklist and (not without a hint of unconscious irony) a list of on-line addresses from which weather forecasts can be obtained.

In terms of both the information and the illustrations that it contains, this book gives good value for money, and I look forward to returning to it as the seasons and the weather change. — COLIN COOKE.

Weather, by Storm Dunlop (Cassell, London), 2007. Pp. 288, 27·5 × 21 cm. Price £16·99 (hardbound; ISBN 978 1 844 03601 1).

This is a splendid book of an unusual character: although it is all pictures, it contrives at the same time to be highly informative. Every page is largely filled by a picture, but typically there is quite an extensive and enlightening caption, so the reader gets painlessly educated while enjoying the pictures. The author's parents evidently took an initiative concerning his future interests when they christened him Storm, and there are certainly some impressive pictures of some of his namesakes, but the subject matter is by no means limited to them. Clouds, fogs and hazes, frost, ice and snow, atmospheric optical phenomena, and aurorae are all well covered. Then the reader is shown how the various elements of the weather affect the Earth's surface and can create problems for the people who are trying to live there, and *vice versa*. Full advantage is taken of satellite pictures to illustrate aspects of weather and its interaction with land and sea, and the matters of climate and of climate change are by no means overlooked. The book concludes with a glossary of meteorological terms and a good index; those are the only pages, apart from one page at the beginning with a foreword by Bonington and one at the end listing picture credits, that are not mainly covered by pictures. Normally alert to mistakes, the reviewer noticed only one, where it is said that Sun pillars may extend 20 or more arc-minutes above and below the Sun (p. 170), where degrees were probably the units intended. And, normally more apt to condemn than eulogize, the reviewer is pleased to advise you that this book, while perhaps of somewhat oblique relevance to astronomy, is first-rate, and it comes at what is for nowadays a remarkably modest price, too. Buy it for your friends and children for Christmas — and you will not be able to resist the temptation to have a look at it yourself before parting with it! — R. F. GRIFFIN.

Photographing Weather, by S. Dunlop (Photographers' Institute Press, Lewes), 2007. Pp. 144, 21·5 × 15·5 cm. Price £14·99 (semi-hardbound; ISBN 978 1 86108 449 1).

For many of us, the weather is simply a question of whether it is sunny outside or not. A photograph taken outdoors just happens to have some sky in it by way of an unplanned backdrop. Sadly, most people do not take the time to look up and observe the beauty of cloud patterns, the colours of a double rainbow, the vitality of a lightning display, or in more extreme cases, the awesome power of a tornado or a tropical storm. For many years, I have harboured the desire to observe some of these phenomena at close quarters. Consequently, I thought this book might provide some useful insight on capturing a permanent record of such events.

I have to confess my first reaction to the book was one of disappointment. It aspires to being a coffee-table book full of beautiful pictures. In reality, it is bounded by its pocket-sized format. The pictures are good but lose a lot of their impact in this small format. To be fair, the author does describe it as a "conveniently sized field guide" but you cannot quite shake off the feeling that the book was designed to meet a particular price point in the market.

There is a reasonable amount of useful information here, particularly in the first two chapters, which cover the suitability of different types of photographic equipment and the basic techniques necessary for capturing a wide variety of weather phenomena as well as aurorae and noctilucent clouds. Chapters 3 & 4 address the need to try and understand the forces that drive the weather you are trying to observe and the cloud patterns which are the most obvious result of those weather patterns. Chapter 5 deals with the variety of colours and optical phenomena you might come across in your observations of the weather, and the penultimate chapter covers what might be described as the 'tricks of the trade' used in weather photography. Unfortunately, the final chapter, on digital manipulation of images, seemed to be a bit of an afterthought.

Although there is quite a lot of useful information here, I feel that a good opportunity has been missed with this book. In all honesty, I cannot recommend that you go out and spend £14·99 on this publication. — STEVE BELL.

Observing the Sun with Coronado Telescopes, by P. Pugh (Springer, Heidelberg), 2007. Pp. 352, 23·5 × 15·5 cm. Price £24·50/$39·95/€32·95 (paperback; ISBN 978 0 387 68126 9).

When the opportunity arose to review this book, I was delighted to oblige. Although it has been many years since I last indulged my interest in astronomy from a purely amateur perspective, I can still remember making solar projections when I was in my teens and thinking that the possibility of observing the Sun in Hα was a long way away. The opportunity to observe a prominence with the naked eye only presented itself during all-too-infrequent total eclipses of the Sun. Although it has long been an interest of mine to make regular observations of these phenomena, Hα observations have been something that only the well-heeled amateur could afford to do until relatively recently. In the last ten years or so, the cost of observing the Sun telescopically in Hα has dropped to the level where it can be described as affordable. Even now, you could spend anything between £400 and £10 000 pounds on such dedicated telescopes. It is therefore most welcome to find a book that provides useful information on choosing and using such equipment, especially if you are seriously considering buying such a specialist telescope.

Philip Pugh's book comes from the *Patrick Moore's Practical Astronomy Series*. It describes the ownership experience of what is perhaps the most popular range of dedicated Hα telescopes and etalons currently available. Coronado is probably best known for the PST, the Hα Personal Solar Telescope, and the most affordable way to observe the Sun in Hα. At the blue end of the visual spectrum, the more recent calcium *K*-line version of the PST is also described in this book. I did note with some interest that I am probably too old to get the best of out the calcium *K*-line PST visually. My ageing eyes are losing some of their blue sensitivity and it seems I would have to use a camera or CCD as the detector to get the full benefit of this device. Now I come to think of it, policemen are getting younger these days.

I think it is worth describing the contents of the book. Chapter 1 provides some introductory material on the Sun, observing in 'white light', and what Hα and calcium-*K* observations will reveal. Chapter 2 is dedicated to the Hα Personal Solar Telescope and covers its construction, accessories, use, and a discussion of its capabilities and limitations. Chapter 3 looks at the MaxScope 90, the top end of the Coronado range, and Chapter 4 looks at the range of equipment available between the PST and MaxScope 90. The latter two chapters follow the same general format as the PST chapter. Imaging using these telescopes is discussed in Chapter 5 and instruments from other manufacturers are discussed in Chapters 6 & 7. Offerings from ChromixSun and Daystar in Chapter 6 are approached from the point of view of experience with this equipment whereas those in Chapter 7 such as the SolarScope range of instruments are classed as "untried". There is also a useful summary chapter as well as a glossary of terms used in the book.

This book provides a wealth of useful information on choosing the right telescope, what accessories work best with those instruments, the use of cameras and CCD detectors, and the post-processing of their images. There are copious illustrations of both the instrumentation and the results that you might obtain given time and patience. I have no hesitation in recommending this book. For potential buyers of such telescopes this book is well worth £25. The advice it offers may save you from making a costly mistake and will open the door to a new way of observing our nearest star. Philip Pugh and his co-authors are to be congratulated on a very useful guide. — STEVE BELL.

Guide to Observing Deep-Sky Objects, by J. A. Farinacci (Springer, Heidelberg), 2007. Pp. 208, 23·5 × 15·5 cm. Price £19·50/$29·95/€24·95 (paperback; ISBN 978 0 387 72850 6).

Guide to Observing Deep Sky Objects, one of *Patrick Moore's Practical Astronomy Series*, consists of a textbook accompanied by a CD-ROM. There are full instructions on how to load and use the software in the 'Introduction' of the textbook.

The main body of the book is an alphabetic list of all 88 constellations. Each gets a double-page spread with a stick figure of the constellation showing the brightest stars with their Bayer letters, plus a selection of deep-sky objects depicted with standard symbols. Unfortunately these maps are rather small, and for the most busy constellations, such as Sagittarius, good eyesight is needed to read them, especially under dim red light. The maps have an angular-size calibration line, but no constellation boundaries, or equatorial coordinates. Accompanying the map are tables of the stars and selected deep-sky objects (generally identified by their M or NGC designation), with right ascension, declination, and apparent visual magnitude. Because the map does not have equatorial coordinates, finding a listed object can be tedious for anyone who doesn't already know their way around the sky. A graph shows when the constellation is visible in the sky from mid-northern latitudes. Farinacci doesn't tell us exactly what his fictitious observing latitude is, but it seems to be about 40° N. A more serious difficulty is that this is his observing site for *all* constellations, so those graphs are of no use whatsoever for the 18 constellations that never rise above the horizon at 40° N. So, regrettably this text is not so useful for southern-hemisphere observers.

The software allows a user to specify their location and time zone and then, in a separate file, to specify local time. The user is then prompted to indicate what class of deep-sky object they wish to view (globular cluster, nebula, galaxy, *etc.*) and a limiting magnitude, and the program comes up with a DOS window containing a list of visible objects, and an associated string of numbers which includes RA, declination, apparent visual magnitude, and size. On my computer, running Windows XP Professional, the DOS window could not be persuaded to format properly, neither could it be edited or printed. There is a very brief description of objects. For example, the galaxy NGC4856 is described as 'oval, easy-to-see'. How useful this is I leave you to decide.

It is not clear to me for whom this book is intended. For anyone who navigates their way around the sky by finderscope and star-hopping, the maps in the book are not really suitable. True, the coordinates can be used by anyone with a 'GOTO' telescope to locate the objects, but *Patrick Moore's Practical Astronomy Series* already includes Jess Gilmore's *Practical Astronomer's Deep Sky Companion*, which is far better in all respects than this volume. As for the accompanying software, it is no match for the several good planetarium programs currently available. In short, I would not choose this book, or software, to guide my deep-sky observing. — ALAN LONGSTAFF.

THESIS ABSTRACTS

AN OBSERVATIONAL STUDY OF POST-ASYMPTOTIC-GIANT-BRANCH STARS

By Timur Şahin

In this thesis, we present LTE model-atmosphere analyses of a group of early B-type post-asymptotic-giant-branch (pAGB) stars. With initial masses $\leqslant 9\,M_\odot$, post-AGB stars form an important group of evolved stars and provide a unique opportunity to study stellar evolution almost on a human time-scale. Post-AGB stars have spectral types ranging from K to B and luminosities between 10^3 and $10^4\ L_\odot$. These objects ended their asymptotic-giant-branch (AGB) evolution phase with a period of strong mass loss ($10^{-7} - 10^{-4}\ M_\odot\ \mathrm{yr}^{-1}$) and have been evolving from cooler to hotter temperatures at almost constant luminosity on a timescale of $\sim 10^4$ yr. B-type pAGB stars span a wide range in effective temperature (10 000 – 30 000 K). Their expected surface gravities (log g) and effective temperatures (T_{eff}) coincide with those of B stars evolving from the main sequence. Therefore systematic observational analyses are required to distinguish these two groups. Furthermore, post-AGB stars may be divided into four distinct groups based on their chemical composition. In this thesis, groups I and II represent post-AGB stars which are very metal deficient with C/O $\approx$ 1, and metal poor with C/O $<$ 1, when compared with the Sun, respectively. The question is whether hot pAGB stars belong to any of those four groups. Three further objectives included: (*i*) to discover whether post-AGB stars have helium-normal or helium-rich photospheres; (*ii*) the detection and measurement of *s*-process element abundances (*e.g.*, Sr, Y, Ba, Hf); and (*iii*) to determine whether they show any anomaly in phosphorus abundance such as that seen in the extreme helium (EHe) stars.

High-resolution échelle spectra of several post-AGB stars were obtained at the *AAT* in 1999 and 2005 in order to study chemical composition, rotation velocities, and other fundamental properties. Échelle spectra present many difficulties for data reduction, including the problems of order rectification and merging. To address these problems we developed an échelle-spectrum-reduction package, known as TIGER. These spectra were analyzed using model atmospheres and synthetic spectra computed with the Armagh LTE stellar-atmospheres software. The semi-automated spectral-fitting package SFIT was used to measure the stellar surface parameters and composition.

The results show that the programme stars have T_{eff} in the range 15 000 – 25 000 K and log g in the range 2·5 – 3·0. In addition to being metal-poor stars, they show mostly C/O $<$ 1. Several of our programme stars, namely HD 119608, LSS 4331, LSS 5112, and LB 3116 confirm this. The majority of hot post-AGB stars can be identified with the group II, metal-poor and C-deficient post-AGB stars. The model-atmosphere parameters, LTE element abundances, and estimated distance, obtained here support the idea that the programme stars are true post-AGB stars.

We detected helium enrichment in the post-AGB stars Hen 3–1428 and LSS 4331. We did not detect any evidence of *s*-process elements, primarily because of the high effective temperatures of our targets. Our results do not show overabundance in phosphorus for any hot pAGB stars. Since we used the same atomic data and methods, we conclude that the enhancement of phosphorus previously found in some EHe stars is real.

We studied stellar-wind signatures for the post-AGB star LSIV −12 111. Emission-line equivalent widths for Balmer lines show changes between two different epochs. Hen 3–1428 and LSIV −12 111 show blue-shifted absorption lines. A stellar wind is clearly present in both stars.

We compared the variability of a group of post-AGB and a group of EHe stars using archival photometry. We did not detect any variability in EHe stars. We detected variability in five post-AGB stars. Large photometric variations in HR 4049, HD 213985, and HD 52961 appear to be related to the binary period. — *The Queen's University of Belfast; accepted 2008 March.*

A Radio Study of the Starburst Galaxy M 82

By Danielle Fenech

Radio observations of the prototypical starburst galaxy, M 82, are presented. These provide the most sensitive, high-resolution observations of the discrete supernova remnants and H II regions in the central starburst to date. In addition, the first ever Global VLBI and *MERLIN* data combination has produced images of the detailed structure of the compact sources at 1·6 GHz.

MERLIN 5-GHz observations made over a period of 8 days have provided extremely sensitive images of the discrete sources within the M 82 starburst with an r.m.s. noise level of $\sim 17\ \mu$Jy beam^{-1}. In total 55 of the sources within M82 have been imaged with resolutions of 35–50 milli-arcseconds. This has enabled direct comparison with previous *MERLIN* 5-GHz observations from 1992. As a result, expansion velocities have been measured for ten supernova remnants with values ranging from 1500 to 10500 km s^{-1}.

Global VLBI observations at a frequency of 1·6 GHz were performed using sixteen antennae across Europe and America. The combination of these observations with *MERLIN* 1·6-GHz data has provided the first ever Global VLBI and *MERLIN* combined image of M 82 at this frequency. These data have been used to study the supernova remnants and H II regions using resolutions ranging from 20–130 milli-arcseconds.

Both the *MERLIN* 5-GHz and combined Global VLBI and *MERLIN* 1·6-GHz observations have been used to study the supernova-remnant population, including measurement of the supernova rate and the star-formation rate within M 82.

The 1·6-GHz Global VLBI data form the most recent epoch of observations used to study the evolution of the most compact sources within M 82. Comparison of this and previous epochs covering a 19-year timeline has enabled confirmation of the expansion of SNR $43{\cdot}31+59{\cdot}2$, with a velocity of $\sim$8000 km s^{-1}, and study of the source $41{\cdot}95+57{\cdot}5$, showing it to be unique of the sources observed within that galaxy. — *University of Manchester; accepted 2007 December.*

THE VARIATION OF CORONAL HOLES WITH SOLAR CYCLE

By Steven A. Chapman

Coronal holes are commonly known to be the source of the open magnetic flux and the high-speed solar wind. In EUV and X-ray emission lines, coronal holes are seen as the regions with reduced emission due to lower temperatures and electron density than the surrounding quiet Sun. Observing the evolution of coronal holes over the course of a solar cycle can lead to a greater understanding of the evolution of the solar dynamo and can advance our knowledge in forecasting space weather. Synoptic data from the *Normal Incidence Spectrometer* (*NIS*), which is part of the *Coronal Diagnostic Spectrometer* (*CDS*) onboard the *Solar and Heliospheric Observatory* (*SoHO*), have been used in this work to observe the evolution of coronal holes during cycle 23, from mid-1996 through to mid-2007 (solar minimum to solar minimum).

Towards the end of 1998, communication with *SoHO* was recovered after being temporarily lost for several months. On recovery of *SoHO*, it was noticed that the spectral lines had acquired wings, making the Gaussian profiles broader. That broadening now extends profiles beyond the boundaries of some spectral windows, preventing a direct measurement of the background of the spectral line. Here, I present a new method of obtaining an estimate of the background for both the *NIS* bands, as well as alternative values with which to describe the wings of the broadened Gaussian spectral profiles. The new method for the background level shows a considerable improvement over the background level determined by a fitting routine; I demonstrate that the resultant intensity could be over-estimated by up to 30%.

Previous studies which investigated the coronal-hole-area variation were either limited to or concentrated on the polar coronal holes, or else the models experienced large uncertainties when determining the area of coronal holes close to the solar limb. However, the isotropic nature of the radiation emitted from the low corona does allow for coronal-hole areas to be determined at any location on the solar disc, right up to the limb.

Imaging the coronal lines Mg IX and Mg X shows many, dark, low-emission regions that belong to filaments and their channels as well as coronal holes. It is not until the temperature has been derived from the ratio of Mg X/Mg IX that we are able to see coronal holes as the only regions cooler than the surrounding quiet Sun. Observing the typical coronal-hole temperature through the solar cycle shows that the temperature of the holes varies in phase with the solar activity cycle, with values of $1 \cdot 17 \pm 0 \cdot 06$ MK and $1 \cdot 09 \pm 0 \cdot 02$ MK at solar maximum and near solar minimum, respectively.

Using this low-temperature property of coronal holes, an automated method has been developed to distinguish the coronal-hole areas using the large Mg X/Mg IX temperature-ratio datasets from 1998 May onwards. Prior to this, the Mg X line was not included in the synoptic study; therefore the coronal-hole regions were identified manually from Mg IX-intensity images. The results show a negative correlation between the synoptic coronal-hole area and solar activity; areas of $\sim 12\%$ of the total solar surface area are found around solar minimum, where the coronal holes are predominantly found at the polar regions, and this falls to $\sim 6\%$ around solar maximum.

The motion of the coronal holes over the solar cycle has also been observed using maps created from the Mg X/Mg IX temperature ratio. It is shown that, in the northern hemisphere, the old-cycle coronal hole leaves the pole in 1999 May and the new opposite-polarity polar coronal hole begins to re-establish at the pole from 2001 September. The southern-hemisphere polar hole vacates the pole in 2000 May and begins to reform in 2004 January. This shows that the northern-hemisphere polarity reversed first, followed by the southern hemisphere approximately two years later. An interesting behaviour was noticed at the poles when, shortly after a polar coronal hole established, the coronal hole was displaced from the pole. This occurred three times in both hemispheres until the polar coronal holes finally settled in 2005 June (North) and 2006 November (South). These displacements could be due to interactions between holes of opposite and of like polarity at mid-latitudes.

Combining the synoptic coronal-hole areas with the daily disc-averaged magnetic-field data, obtained by Kitt-Peak and *SOLIS* facilities, the variation of the open magnetic flux over cycle 23 is also presented. Shortly after the old-cycle northern polar coronal hole vacates the polar region, the open flux is shown to increase steadily up until ~2003, approximately two years after solar maximum. The open flux begins to return to solar-minimum levels in 2004 January, when both polar coronal holes are attempting to encompass the pole. These events signify that there is a strong connection between the evolution of open flux and the processes involved in reversing the polarity of the Sun. — *University of Central Lancashire; accepted 2007 November.*

Here and There

NOT TO OUR VICTORIAN READERS

… the newly discovered Comet Holmes, which flared up unaccountably in October [2007] … — *The Daily Telegraph*, 2007 December 1, December Night Sky. [See *The Observatory*, **15**, 441, 1892 for Holmes' announcement.]

CHROMATIC ABERRATION

… for a fixed angular diameter, a redder star is angularly larger than a bluer one. — *The Power of Optical/IR Interferometry* (Springer), 2005, p.34.

INTERESTING NEW ASTROPHYSICS

… a triple-lined spectroscopic binary. — *Ann. Rev. A & A*, **45**, 490, 2007.

Advice to Contributors

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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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The Managing Editor of 'THE OBSERVATORY'

16 Swan Close, Grove

Wantage, Oxon., OX12 0QE

Telephone +44 (0) 1235 767509

Email: manager@obsmag.org

URL: www.obsmag.org

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