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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2011 March 11 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

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

The President. Good afternoon, ladies and gentlemen. I have the sad duty today to announce the death of Professor V. Radhakrishnan, affectionately known as 'Rad', on 2011 March 3. He was elected an Associate of the Society in 1985, and he was the director of the Raman Research Institute between 1972 and 1994. Can we just stand and reflect for a moment, please? Thank you.

It's now time for me to introduce our first speaker, David Robinson, from Earth Sciences in Oxford. David was the winner of the 2010 Winton Capital Prize for Geophysics. His title is 'Super-shear earthquakes: what has happened and what could happen?'.

Dr. D. Robinson. Although frequently depicted in the media as being a point source, earthquakes have finite dimensions of width, length, and duration. An earthquake initiates at a point, the hypocentre, and a crack or rupture propagates away from that point at speed. The speed with which this rupture progresses is termed the 'rupture velocity'. Broadly speaking, solids propagate waves at one of two velocities, the longitudinal P-wave velocity being approximately $\sqrt{3}$ greater than the transverse S-wave velocity. Traditionally, ruptures have been thought to progress typically slightly below the local S-wave velocity; however, recent observations have thrown that into doubt.

Since the closing years of the 20th Century there have been several earthquakes that most authors agree exhibited a sustained portion of their rupture at so-called 'super-shear' or 'inter-sonic' velocities, having rupture velocities greater than the S-wave but less than the P-wave velocity. The mathematics of fracture mechanics has long been known to allow this: mode-III ruptures such as those that occur naturally in 'transform' or 'strike–slip' earthquakes have stable rupture velocities at either just below the S-wave velocity or between the S- and P-wave velocities. It was thought that the prohibited region of rupture velocities at the S-wave speed would prevent super-shear ruptures being reached.

Observations from the 2002 Denali (Alaska), 2001 Kunlun (Tibet), and 1999 Izmit and Düzce (Turkey) earthquakes challenged this view. The observations of recent earthquakes recorded with high-quality modern equipment breaking at super-shear velocities reignited debate about some historical earthquakes whose recordings have hinted at super-shear ruptures, such as the 1979 Imperial Valley (California) and 1906 San Francisco earthquakes. Many of these earthquakes share certain features which will now be discussed in turn.

Beginning with the most recent of these events, the 2002 November 3 Denali earthquake was a magnitude-7·8 earthquake that broke some 300 km of the Denali fault in Alaska. Despite its remote location, this earthquake had the potential to be devastating economically and environmentally, as the trans-Alaskan oil pipeline runs over the Denali fault. Fortunately, precautions taken during the construction of the pipeline were sufficient to prevent a catastrophe. The pipeline also provided seismologists with an opportunity, as a high-quality accelerometer had been sited close to where the pipeline crossed the Denali fault to measure ground accelerations during an earthquake. The recording made by this station during the earthquake is unusual and it was shown that the recording can only be fitted by the earthquake initiating at sub-shear velocities then transiting to super-shear speeds just before it reaches the station. More recently, other workers have re-examined teleseismic-array data using new methods developed in response to the 2004 Sumatra–Andaman Islands earthquake and tsunami and have confirmed this observation. The conclusion to draw is that it seems likely that the earthquake initiated at 'ordinary' sub-shear velocities before jumping to super-shear velocities. This jump occurred along a long, straight section of the fault.

A slightly older super-shear earthquake is the 2001 November 14 Kunlun event. This was also a magnitude-7·8 strike–slip earthquake that broke some 400 km of the Kunlun fault in remote Tibet. However, without the potential economic impact of the Denali earthquake, seismologists were slower to investigate. Also due to its remote location, the lack of local recordings did not offer the initial insight available in Denali. Despite this, as more seismologists began to take an interest in the earthquake, evidence began to mount for a super-shear rupture portion. In 2003 Michel Bouchon and Martin Vallée published their work on the earthquake using surface waves. Although the data used only allowed resolution at the level of 100 km, they were able to show that the average rupture velocity in three of their four 100-km-long earthquake segments was in excess of the shear-wave rupture velocity. Other workers confirmed and expanded on their results using higher-resolution data: Robinson *et al.* in 2006 used body-wave data and Walker and Shearer published their teleseismic-array analysis in 2009. These three separate studies by three distinct groups using three very different data sets mean that the Kunlun earthquake is now universally held as being the best example of super-shear rupture observed. Again, as with Denali, the models show rupture started at sub-shear velocities before jumping to super-shear speeds; and the super-shear rupture is observed on a long, straight portion of fault. Similar conclusions can be reached with older earthquakes, although the supporting data are less convincing.

Super-shear ruptures have the potential to cause very high accelerations close to the fault, as the S-waves emitted by the rupture front have the potential to 'stack up' in a mach cone behind a super-shear rupture, hence it is important to identify if faults near population centres have the potential for sustained super-shear rupture. Given that observed super-shear ruptures seem to require a straight section of fault to form and that complexity in the fault causes super-shear rupture to transition back to sub-shear ruptures or stop completely, we can search for faults that seem to have these properties. Thus we looked for long (> 100 km), straight (no more than 5° deviation over 100 km) faults worldwide. We identified 26 sections on 11 fault systems worldwide with close to 65 million people living within 50 km of these fault sections. Although some of them are well studied, such as the section of the San Andreas near San Francisco, others are less well studied, such as the Red River fault through Hanoi. We urge local authorities to investigate these faults urgently to ensure current building codes are sufficient.

The President. Any questions?

Dr. W. Tobin. In the earthquake in Christchurch two weeks ago, which was a strike–slip event, I understand that the ground accelerations reached up to 2·2*g*, which are much bigger than those you've shown on your slide. On the other hand, I think the fault is not very long. Is there any suggestion that super-shear velocities might have been involved?

Dr. Robinson. Well, that would be a possibility, but we don't have any models yet for that earthquake. As you mentioned, the ground accelerations recorded were exceptionally high, the highest I think that have ever been recorded. The question is whether that is just because we had a huge number of local stations — and we normally get velocities of 2*g* very close to the earthquake epicentre — or whether there is some other feature in there. I think that there is also a focussing effect — I am not an expert in New Zealand geology but there is a set of granite hills which can reflect into the sedimentary basin. So yes, it is a possibility: if you did get super-shear rupture then you would expect very high peak ground-accelerations with very short durations.

Professor Kathy Whaler. You mentioned these super-shear velocities between the P- and the S-wave speed — is there a formula that gives that velocity?

Dr. Robinson. There is a set of stable velocities out beyond the S-wave velocity, and the system will prefer whichever velocity is energetically the most favourable. The modelling of this becomes very complicated in three dimensions, and it has only really been done in two dimensions at the moment.

Mr. H. Regnart. As I understand it, the shear-wave velocity is always lower than the pressure-wave velocity. Does the super-shear velocity ever exceed the pressure-wave velocity?

Dr. Robinson. No; if you had a rupture which exceeded the P-wave velocity that would be an acausal rupture, because you couldn't get any information that an earthquake had happened to the rupture front: the P-wave is the fastest speed that information can be transmitted through a solid. But super-shear velocities only actually apply to so-called strike–slip earthquakes; in subduction-zone earthquakes, as for example in Japan, it's a different mode of sliding, so theoretically you would only get sub-shear rupture velocities.

Mr. M. Hepburn. Are there really no faults longer than 100 km around the Peruvian Altiplano, one of the great earthquake centres of the world?

Dr. Robinson. The point here is whether they're straight, or whether they're segmented, or so on. Quite often there are long fault systems, but they'll have so-called step-overs where the fault extends over a certain distance and then a step-over for a few kilometres. Where you have that complexity we think that it resets the earthquake rupture velocity. So it may rupture up to a super-shear rupture velocity for a short period of time, but then it will slow down at the complexity and then perhaps speed up again.

Mr. Hepburn. So although it looks straight on the map, the fault is not so long?

Dr. Robinson. Yes, you can have, say, 4–5 km of step-over, which is often enough actually to stop an earthquake dead; you perhaps don't see that detail on simple maps.

Professor Whaler. The current data you are using are from local single-component seismic stations, and I wonder whether there is extra information that you can get from three-component broadband data?

Dr. Robinson. The material I showed is from a variety of different sources, but I do use three-component broadband teleseismic data, usually S-wave data. But others use different data. I can show you examples of array data being used to track where the peak energy release is along the fault; this is a new technique, used since Sumatra, so it has only been used in the last 4–5 years, using data from local stations. An example is the Kunlun earthquake, which is really being looked at with a lot of different methods and techniques; most of those methods are now coming to the same conclusion, that there was a very fast portion of rupture.

Mr. M. F. Osmaston. Can I briefly supplement what you said about the Sendai earthquake? This margin of Japan is straight above where the plate is down-bending; and this down-bend process, actually, is a step-faulting process, it's not elastic. So I think that there are two sorts of earthquake: a step-faulting increment, which then offsets the interface and locks it; and then a stress build-up, over this fault, which shears through the obstruction. And this is what occurs in big earthquakes; in fact, when you study them you find a tendency to alternate between earthquakes that didn't produce a tsunami and those that did, because of the change of mechanism. And in Japan, what is extremely well known is that if the position of the down-bend is locked, there is about a 50-km-width strip which is highly seismic.

The President. We had better wrap it up there, so thank you, Miles [Osmaston], and thank you once again, David. [Applause.]

Our second speaker is Karen Masters from Portsmouth University, and she is going to answer the question: 'Do bars kill spiral galaxies?'.

Dr. Karen Masters. It has been known for a long time that when we look out into the Universe we find that the bright galaxies come in two main types: (*i*) smooth, spheroidal elliptical galaxies, and (*ii*) spiral galaxies like the one we live in. Probably the most important question in extragalactic astronomy is how these two types relate to each other. We now have an understanding that most of the evolution goes from blue/star-forming spirals to red and dead ellipticals, but the details of what processes are most important in this transformation remain unclear. After deciding that a galaxy is a spiral, perhaps the next most obvious feature it could have is a bar. Edwin Hubble, in his famous tuning-fork classification, split the spiral population into two by this feature. Early optical studies found that about 30% of spirals have bars, and this fraction increases to more like 60% if 'weaker' bars and oval distortions are included. In this talk I'm going to consider the importance of bars on spiral-galaxy evolution based on research using classifications provided by members of the public through the Galaxy Zoo website.

In the time of Hubble, and even up to the end of the last century, the numbers of galaxies that had been catalogued was such that it was possible for individual astronomers visually to classify all of them. However, the invention of the CCD and the advent of the digital era has made much larger surveys of galaxies possible. The largest of these efforts is the Sloan Digital Sky Survey (SDSS) which in its Main Galaxy Sample catalogues almost a million galaxies. Not even all the astronomers in the world could classify visually that number of galaxies. It was considerations like that which inspired the Galaxy Zoo project (www.galaxyzoo.org), launched in its original form in 2007 July. The idea behind

Galaxy Zoo was to enlist the help of members of the public through an internet site to classify large numbers of galaxies (to start with, just splitting them into spirals and ellipticals). Galaxy Zoo was much more of a success than its founders could have imagined — each of the one million SDSS galaxies was classified several times (by at least 20 people, with a median of 38 people). This allows a classification 'probability' to be constructed — including both information about the most likely classification and a level of confidence in that answer.

When we ask the volunteers why they spend time on Galaxy Zoo we find that almost half of the volunteers (40% in the latest survey) say that their main motivation to classify galaxies with Galaxy Zoo is that they are "excited to contribute to original scientific research". This answer gives the researchers involved in Galaxy Zoo a responsibility to produce scientific results — something which I have been happy to lead in recent years.

Galaxy Zoo was such a success, and the volunteers so eager for more challenges, that in 2009 February Galaxy Zoo 2 was launched. In this version of Galaxy Zoo, the brightest quarter of a million galaxies from SDSS were presented, and a series of much more detailed questions was asked, including a question about the presence of a bar. It was the classifications from this question which we used to produce the first scientific result from Galaxy Zoo 2 (*MNRAS*, **411**, 2026, 2011), which I'm going to talk about today. In this work we took a volume-limited sample of about 14 000 disc galaxies which had reliable answers to the 'bar question' (meaning that 10 or more users had answered that question). The main result we found was that red disc galaxies were much more likely to host bars than blue disc galaxies. Looking in more detail we found that if the disc-galaxy population were split into two based on the size of the central bulge (a spheroidal component in the centre of the disc) this also split the population into a sample with a low bar fraction which was mostly blue (those with small bulges) and a sample with a high bar fraction which was mostly red (those with large bulges). This work was the subject of an RAS press release by Robert Massey that provided the title for my talk today, and which received some media coverage (*e.g.*, *Nature* Research Highlights, *Astronomy Now*, *A&G*).

In work looking at the properties of the red spirals found by Galaxy Zoo (*MNRAS*, **405**, 783, 2010) we had seen that the reddest spirals (which are as red and dead as most elliptical galaxies) are about twice as likely to host a bar than the normal blue spiral population. This result seemed very intriguing so we were inspired to follow it up, and question if the bar itself could be responsible for turning spirals red — taking an early look at the Galaxy Zoo 2 bar classifications. Red spirals are an important subset of the galaxy population which not only provide a way to study evolutionary processes that only affect star formation (and not morphology), but (as suggested in recent work) may also represent the dominant route for galaxy evolution from blue to red, so understanding the rôle bars might play in their evolution is very interesting.

A bar is a standing wave in a cavity — understood using similar physics to explaining the modes of organ pipes or guitar strings. Forming a bar is very easy in a disc galaxy; the big question is really why some are observed not to have them. The answer may lie in the transitory nature of bars. It has been suggested that bars continually form and self-destruct in discs as they move material both inwards and outwards. Gas appears to be very important in this process. Simulations of passive discs (with no gas) make bars which are much more stable. This suggests that the high bar fraction in red discs is most likely due to the lack of gas — in that sense the bar, rather than 'killing the galaxy', is a symptom of the illness (the lack of gas supply) which is causing its death.

The next steps for this work are to test the above idea. If the bars come along with some external (environmental) process which is creating the red spirals, we expect to see that bar fraction has a dependence on galaxy environment. Previous research looking into the environmental dependence of the bar fraction is inconclusive, but with Galaxy Zoo we have the ideal sample to revisit the question, and are currently working on this.

The other thing to check is that the gas fraction is indeed the dominant factor. Luckily I have access to the ideal survey for such a comparison. The ALFALFA survey is now more than half complete — using the Arecibo radio telescope to do a blind survey of the neutral-hydrogen content of all galaxies in the high-Galactic-latitude sky above Arecibo. This survey overlaps significantly with the SDSS imaging area, providing gas contents for a large fraction of the Galaxy Zoo galaxies. I have just begun the analysis of this cross match.

I want to end by saying that "You can Zoo too!". What's currently running at www.galaxyzoo.org is Galaxy Zoo: Hubble, asking for classifications of tens of thousands of galaxies imaged by the *Hubble Space Telescope* over half of the age of the Universe. The comparison between these data and Galaxy Zoo 2 will allow us to study the bar fraction as a function of redshift and numerous other important questions in galaxy evolution. Also, the Galaxy Zoo 1 data are now public (both at www.data.galaxyzoo.org, and in the DR8 release of the SDSS), and we're starting to see some independent research using the classifications appearing in the literature. Finally, we will be having a Special Session on science from Galaxy Zoo at the Boston meeting of the American Astronomical Society in 2011 May.

Thank you Galaxy Zoo — particularly the more than 400 000 people who have signed up for accounts at 'The Zooniverse' (www.zooniverse.org). The contributions of volunteers to Galaxy Zoo are individually acknowledged at http://www.galaxyzoo.org/Volunteers.aspx; and volunteers who classified in Galaxy Zoo 2 (and wished to be acknowledged) are listed at http://zoo2.galaxyzoo.org/authors.

The President. Thank you very much, Karen.

Professor D. Lynden-Bell. I wanted to ask a crucial question: it seems to me when you've got a complicated spiral on top, it is very much harder to see a weak bar than otherwise. But then the question becomes: can you distinguish this by just asking about strong bars?

Dr. Masters. Yes, I skipped over that. I do think that the Galaxy Zoo bars are exclusively the strong bars. I do not think that the Galaxy Zoo classifications are picking out the weak bars. So I think that the trends we're seeing are trends with the strong-bar fraction. I haven't properly calibrated that. That's one of the fun things about Galaxy Zoo: we actually need to calibrate the classifications! I have started to look into this, and you can identify bars automatically. There is a recent sample of a couple of thousand galaxies (Barazza *et al.*); I have their bar identifications, and we see the same kind of trends, but they see a larger bar fraction, and my interpretation is that we're seeing the strongest ones only. And in fact they measure the length, and there is a little bit of a bias in that the bars that the Zoo people pick out tend to be the longer ones. So I think it's probably a safe bet that the Zoo bar identifications for this paper are strong bars.

Dr. M. Bureau. It wasn't clear to me whether you are arguing that bars make red galaxies or red galaxies make bars, and I heard an argument going both ways, so which do you think it is?

Dr. Masters. Could it be both? Could the bar help the spiral galaxy to evolve more quickly, but also once you get rid of the gas are you more likely to keep the bar there? I'm a little unsure as to the answer still, and I think that probably both effects seem reasonable. Obviously the bar fraction in blue galaxies, if it is a sort of an on-off thing that the bar is doing, is telling you about the duty cycle.

Professor R. C. Kennicutt. Let me add a third possibility to your list: Lars Hernquist gave a very nice colloquium at Cambridge last week where he is doing very-high-resolution numerical simulations of purely stellar disc galaxies, so they would be your red galaxies, and he finds that without a bar, he makes spiral arms, but only one or two per cent, things a Galaxy Zoo classifier would never see; so could it simply be that you need a bar to make spiral arms in these objects?

Dr. Masters. Yes, that is certainly possible. Arguably the spiral arms in the red spirals must be grand-design spirals, but that has to be triggered by a bar. In the results, I showed the bar fraction with colour, but we have not asked the question whether or not there are spiral arms there, we have just asked whether it is a disc. And if you look at the original Zoo classifications, the volunteers were actually quite reliable at picking out disc galaxies that didn't have particularly strong spiral arms; they could still tell there was a disc there. So I would say in that sample is likely to be all types of galaxy discs, whether or not they have spiral arms.

Dr. G. Q. G. Stanley. I wondered if Galaxy Zoo 3 will be able to look at different sorts of morphologies following on the results discussed in the Presidential Address last month?

Dr. Masters. I don't know about those results!

The President. One question would be, why does it go: blue-spiral, red-spiral, elliptical? What about S0s with bars?

Dr. Masters. You don't have small-bulge S0s, right?

The President. Complicated story!

Dr. Bureau. Alternatively, in response to Rob [Kennicutt]'s comment: I think you have a good measure of the environment, that's for sure. Of course the spiral can be triggered by interactions very easily and so can bars; so I think the measure of the environment is very important to disentangle what's doing what.

The President. Intriguing results, Karen, but we had better wind up. Thank you very much. [Applause.] One more figure on Galaxy Zoo, for those of you who couldn't read down on the bottom of Karen's graph, is that there are six people who classified all one-million galaxies, around the world.

It is my pleasure now to introduce the Eddington Lecturer. The Eddington Lecture is co-sponsored by the University of Cambridge and the Royal Astronomical Society, and this year's lecturer is Professor Shri Kulkarni, from Caltech, Director of the Palomar Observatory, and an expert on transient phenomena. His subject is 'Astronomy, a subject on the cusp'.

Professor S. R. Kulkarni. [It is expected that a summary of this lecture will appear in a forthcoming issue of *Astronomy & Geophysics*.]

Mr. P. Daniels. You've painted a very rosy picture of the growth of technology, and improving computers, and so on. But surely the biggest drawback in the future is going to be government apathy — not providing the funds to allow all these data to be analysed! You need more postgraduates, until a computer program gets to the point where you can invent the virtual postgraduate to provide the brainpower! [Laughter.]

Professor Kulkarni. Well, to be honest, it would be nice to have more money, I agree with you. But we had a discussion yesterday, and I don't readily have the numbers here but it is my estimate, just doing a quick computation, that in the US the amount of money spent per astronomer is over a million dollars per year. I think that's a large amount of money, for any field; I think a chemist would be very jealous of astronomers. Having said that, would I not like more money? Yes. But I think we'll cope, and I think this crowd-sourcing is an excellent example, you know: it is an innovation, all these people are working for free, and for doing honest research! Now there are not just the crowd-sourcers themselves writing papers, there are people writing papers on the results of crowd-sourcing! So, I'm sure we'll find something, I'm not too worried about this. It is better to have data than no data.

Dr. Bureau. I have a related question: telescopes like the *SKA*, or, for example, the accelerator at CERN, they generate so many data that actually they are not archived, but people apply filters to select what they are looking for, with *a priori* assumptions. So isn't there a big danger with these big projects where you can't handle the data, when you go looking for specific phenomena, that you lose the serendipity? This is essentially the way astronomy has progressed greatly — we were not looking for most things we have discovered. So, if the *modus operandi* changes in the future, we won't be discovering these things.

Professor Kulkarni. Oh, I don't know. First of all, I am a bit of a Zen person; I say the only constant in life is change, so we have to get used to it! I think it will just be different. When I was a student I had some old professors who would always tell me how difficult it was when they were doing astronomy, that how every day when they went to the observatory it snowed, morning and evening, and it was uphill both ways. [Laughter.] So it will just be a different game. Certainly, I don't think we will be any smarter — that's the one part I certainly agree with.

I myself have written papers making use of existing data, so let me illustrate one as an example — when *Simbad* first came along, I was interested in studying supernova remnants. At that point, I thought that with *Simbad* you could only search by position, so I put in the RA and Dec. of the supernova remnant, and then it gave all the related objects. And then I noticed one of them called a soft-gamma-ray repeater, something I had heard about in a talk the day before. Then I realized after another few days that soft-gamma-ray repeaters and supernova remnants were in fact coincident — and this was in 1992: at that point the distinction between soft repeaters and gamma-ray bursts was not known. So I wrote a paper which said that the soft repeater was of Galactic origin, which caused a modest amount of stir. I think you'll find that there will be a new breed of astronomers who will never go to a telescope, who will be very adept at database mining; hopefully they will know some physics so that when they make a discovery they will understand its importance. I think it is very hard for me to see these changes because they happen very, very quickly; but I don't think any of us can stop it, so the best is to prepare for that.

Professor P. G. Murdin. You painted a very optimistic picture of the development and progress that technology is bringing us. But I'm rather worried that we might be seeing some of the limits; we are at the stage now where some telescopes are the size of the Earth — VLBI telescopes; and some telescopes, the world can only afford one — *ALMA*; the *James Webb Space Telescope* (*JWST*) is sucking up all of the finances for hardware in the United States for astronomy, for however long it is. Do you think that the end of 'golden age' might be in sight in some ways?

Professor Kulkarni. Well, there is the danger of that; it is really up to us though. The danger is not that we can't build bigger things; the danger is that the bigger things will kill off everything else — the trees will be so vast that the shadow will dry up all the little things.

We should not use the SSC example of experimental high-energy physics: physics of that sort tends to be very reductionist. There is not that huge a rich phenomenology there; in fact, the whole point of that sort of physics is to reduce it to the bare minimum, the fewest numbers possible, for the fewest hypotheses possible. Astronomy in itself is an exploration, it's a game where you have to explore: it's a highly phenomenological subject. So in astronomy, if we allow many flowers or many projects to flourish, we will always do very well. We ourselves would be limiting our own subject by allowing these very large projects in effect, without voicing concerns, to shut others down.

As we speak, a day in the life of the *James Webb Space Telescope* is one-million dollars. Ten days ago, NASA headquarters decided to shut down *GALEX*, which is maybe 4 million dollars a year now, and doing pretty well, it's a mature project. But then, there are lots of things you can do — I agree, perhaps not Nobel Prize-winning things, but routine astronomy which is very important! And *XTE* (*X-Ray Timing Explorer*) costs only something like 1·5 million dollars a year, and for that sort of money is doing fantastic work on X-ray binaries; the US support for *Suzaku* is maybe a few million dollars a year. So with that one single announcement, they were able to save *JWST* by eight days: I don't think eight days delaying *JWST* is worth it; we could even delay *JWST* by 80 days. But I think we astronomers could blame ourselves for that situation.

The President. It's a rather thoughtful point to end on, I think. I have to point out one thing that is clearly false with Shri's talk, which is that he uses observations to avoid thinking [laughter]. Thank you very much, Shri. [Applause.] So, may I remind you that the next meeting here will be on Friday, May 13.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2011 April 19 at 10^{h} 50^{m}
in the Theatre, Venue Cymru, Llandudno

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

The President. Good morning ladies and gentlemen, let's make a start! It's my pleasure to welcome you to the Royal Astronomical Society National Astronomy Meeting. This is the showcase event of the Society, but it is one of those things that the RAS does to support astronomy in the UK. We have now a series of Fellowships, and two cycles of awards have been made so far, and we also offer grants. We run the Specialist Discussions on Fridays through the winter, and probably more important than anything, the journals are a major feature, producing a great deal of impact and a great deal of money for the Society.

This is the second time the National Astronomy Meeting has met in Wales — it was in Cardiff a few years ago, but this is the first time that it has met without a university department to host it. This is largely because of its success — it is a very large event, over 500 people have registered for this NAM; that's one of the highest numbers ever. Next year we will be in Manchester but it is too early to say where we will go in 2013.

Astronomy has never been more prominent in the public eye, in part due to Brian Cox's fantastic series of television programmes. You may not know, but each episode reaches about 6 million people, making it the second most popular BBC2 documentary in the last decade. So it is really a major hit with the public. And of course, 6 million people is 20% of the voting public, and that does mean that people in places of influence, those places where largely we get our funding from, are interested in ensuring that astronomy thrives and is healthy. This meeting, therefore, is a time for us to reinforce and build on that high profile.

It is now my honour to introduce Professor John Harries. John Harries is the first person to hold the position of Chief Scientific Advisor in Wales; he is Professor of Earth Observation at Imperial College, and a past President of the Royal Meteorological Society. He was the first director of the Space Science Department at Rutherford Appleton Laboratory, and that's in fact where I first came across him, getting his team to help out with some of the things we needed to do in preparation for the *Gemini* telescopes. So, with those remarks, I am very happy to hand over to Professor Harries who is going to tell us a bit about science in Wales.

Professor J. Harries. When I was at SERC and NERC, over several decades now, trying to get money for my research, I would turn up to board meetings ready to fight my corner and in the opposite corner were always the astronomers! Now I am on your side, so things do change.

Welcome to Wales. I am delighted that this year's meeting is being held in Llandudno. The population of Wales is only 4·5% that of the UK but behind it is an Assembly Government with a commitment to a powerful science and engineering base here, not only on cultural and educational grounds but also for the strength of the long-term economy. There is a deep feeling that we will need to raise the quality of our standards in science, engineering, and technology. Despite our small size we have some excellent facilities in astronomy and related areas — the excellent astronomy group at Cardiff, a very good Solar System Physics group in Aberystwyth, and some interesting collaborations in academic engineering and manufacturing.

I'd like to talk about the far-infrared (FIR) for the moment, as it touches on my own career. We have the groups at Cardiff on the *Herschel* and *Planck* projects, both of which, I'm sure that you will agree, are world class and very exciting. Whilst I was setting up a FIR spectroscopy group at the National Physical Laboratory to research the Earth's atmosphere and climate, a team at QMC, including Peter Ade, Ian Robson, Peter Clegg, Eric Martin, and John Beckman, began to apply far-infrared technology to astronomy and we did some collaborative work, so I have a strong personal interest in that area in particular. The Solar System group at Aberystwyth also has a long and proud track record in solar and atmospheric physics. I must also mention the industrial collaboration in North Wales with the involvement of Glyndŵr University with the *ELT* project. It is currently in the trial phase of producing sample mirrors and is in competition with a French team. I feel sure that the French government is active behind the scenes in order to get the contract, which is worth 250 million euros. The North Wales 'optics corridor' includes

Key Optics Ltd.; their cover glass has been used on as many as 80% of space missions — there are even pieces sitting on the surface of Mars at the moment.

In conclusion I would like to welcome you again. We are committed within the Welsh Assembly Government to science and technology, and astronomy in particular. It is a challenging and fascinating subject and it is also understood to be one of the most effective attractors of young people into science, so for that reason as well as economic ones we want to encourage more high-quality work in Wales. Thank you. [Applause.]

The President. One of the most pleasurable aspects of this job is recognizing the people who have been given medals and awards by the Society. I now will ask Haley Gomez and Tom Knight to read out the testimonials.

Dr. Haley Gomez. The Gold Medals are the Society's highest honour. Professor Richard Ellis has been one of the most influential British astronomers in the past thirty years. Professor Ellis resolved the 'faint blue galaxy' problem and subsequently made major advances in understanding the origin of the Hubble sequence of galaxies. He has conducted ultra-faint spectroscopic surveys which promise to detect the onset of neutral hydrogen in the intergalactic medium; and he has used strong lensing by clusters of galaxies to demonstrate the presence of intrinsically faint sources at redshifts of 6 to 10, likely to dominate cosmic re-ionization. He conducted the first deep redshift surveys highlighting luminosity-dependent evolution in the star-forming population and was the UK PI of the *2dF* Galaxy Redshift Survey, arguably the world's first large-scale cosmology project.

Professor Ellis established a new astronomy group at Durham and brought together what is now its instrumentation group. Later, as Director of the Institute of Astronomy in Cambridge, he promoted the importance of multi-fibre and multi-slit facilities at the *Anglo-Australian* and *William Herschel* telescopes. Currently, as Steele Professor of Astronomy at Caltech, he is playing a key rôle in the efforts to build a 30-metre telescope. Among many honours, Professor Ellis is an FRS and a CBE; he has won prestigious prizes, held visiting professorships in more than ten leading institutions, and has co-authored 340 refereed papers. On account of his all-round vision, scientific leadership, and rich legacy of contributions to observational cosmology, Richard Ellis is a very worthy Gold Medallist. [Applause.]

Mr. T. Knight. The Gold Medal for Geophysics is awarded to Professor Eberhard Grün. Professor Grün has been at the forefront of the *in-situ* study of cosmic dust for over 30 years and has probably done more than anyone else to advance our understanding of the distribution and dynamics of dust in the Solar System. In fact, the function which describes the size distribution at 1 AU, described in his 1985 paper, is still referred to as the 'Grün distribution'. He led the Cosmic Dust Group at the Max-Planck-Institut für Kernphysik in Heidelberg and oversaw the development of the MPI van de Graaff dust accelerator, one of only two such instruments still operational in the world today. He was coordinator for the *KOSI* project which gave impetus to develop the *Rosetta* cometary lander.

Professor Grün has been active in a wide range of societies and agencies including the IAU, COSPAR, EGS, ESA, ESF, and NASA. He has published over 350 scientific papers and edited *Interplanetary Dust*, which has been an essential reference for the last decade. In 2000, he was elected a Fellow of the American Geophysical Union and a Foreign Associate of the Royal Astronomical Society. Minor Planet 1981 EY20 was designated 4240 Grün. He was awarded the Kuiper Prize by the American Astronomical Society in 2002 and the COSPAR Space Science Award in 2006. Professor Grün is a dedicated internationalist and embodies all of the positive virtues of our subject and is a most deserving recipient of the Gold Medal. [Applause.]

Dr. Gomez. The Price Medal is awarded to Professor Roger Searle. Professor Searle has an international reputation in the use of imaging sonars and other geophysical methods to understand tectonic and magmatic processes on the ocean floor, particularly at mid-ocean-ridge spreading centres. His early work included the definition of plate boundaries to sub-kilometre resolution and determining the evolution of propagating rifts and oceanic micro-plates. Recently he has worked on the effects of mantle hot spots on plate accretion processes and the detailed tectonic patterns and axial volcanic systems of spreading centres. The sum of his work, including over 100 peer-reviewed articles, has resulted in major advances in understanding the Earth's most active/extensive geologic system — the globe-encircling oceanic spreading system and its array of faults that accommodate plate-boundary offsets and evolution in global driving forces. [Applause.]

Mr. Knight. The Eddington Medal is awarded to Professor Gilles Chabrier. Professor Chabrier is one of the world's leading theoretical astrophysicists. The Saumon–Chabrier Theory serves as a benchmark in all high-pressure experiments that seek experimental evidence of metallic hydrogen at high density. Similarly the Saumon–Chabrier–van Horn equation of state remains the fundamental work for matter under conditions characteristic of the interiors of low-mass stars, brown dwarfs, giant planets, and the outer regions of white-dwarf and neutron stars. With his collaborators he developed a complete theory describing the structure, observational properties, and evolution of low-mass stars and brown dwarfs. In addition, Professor Chabrier has significantly improved our understanding of Jovian-planet structure.

Given Eddington's own efforts to understand stellar interiors, it is fitting that it is for his fundamental work on the equation of state of dense matter in stellar and planetary interiors that Gilles Chabrier should be awarded the Eddington Medal. [Applause.]

Dr. Gomez. The Jackson-Gwilt Medal goes to Professor Matt Griffin. Professor Griffin is one of a select group of instrument scientists who opened up the sub-millimetre waveband to astronomy, a field in which the UK plays a leading rôle. In particular he is the Principal Investigator of the international team that built *SPIRE* (the *Spectral and Photometric Imaging Receiver*), one of two cameras on board *Herschel*. While it took *SCUBA* 20 nights to reveal five galaxies at sub-millimetre wavelengths, *SPIRE* has made it possible to detect ~ 6000 in eight hours! The development of *SPIRE*, in which Professor Griffin was involved from the initial-concept stage, involved scientists from 18 institutions in three continents over more than a decade. As Principal Investigator he has displayed outstanding scientific leadership and has provided a master class in instrument design, for which he is a worthy recipient of the Society's Jackson-Gwilt Medal. [Applause.]

Mr. Knight. The Fowler Award for Astronomy goes to Dr. Vasily Belokurov.

Dr. Belokurov's thesis involved an in-depth study of theoretical and observational aspects of gravitational micro-lensing. To improve the analysis of event probabilities, he developed fully automated methods of object classification in large astronomical data sets which, applied to the SDSS data, led to a succession of major discoveries which have revolutionized our picture of the outer Galaxy. His discovery of 'The Field of Streams' in 2006 was a turning point in near-field cosmology and its data were a treasure trove for finding faint dwarf spheroidal galaxies around the Milky Way. For that, and other achievements, including discovering the largest Einstein ring ever found and new ultra-cool white dwarfs, and his contribution to the preparation for the *Gaia* mission, he is a worthy recipient of the RAS Fowler Award. [Applause.]

Dr. Gomez. The Fowler Award for Geophysics goes to Dr. James Wookey.

Dr. Wookey's innovative work on seismological data and his cross-disciplinary research collaborations have improved our understanding of the deep mantle, especially the core–mantle boundary (CMB) which marks the Earth's most dramatic interface — in particular, the style of mantle convection, mantle mineralogy, Earth's early evolution, and CMB coupling, which in turn affects core convection and the Earth's magnetic field. Dr. Wookey is one of the few seismologists who have not only developed methods of analysing seismic data, but have linked them to mineral physics and geodynamics. His current research focus includes a variety of topics ranging from seismology on Mars to new approaches in exploration seismology. In summary, for his impact on the Earth-interior research community at a very early stage of his scientific career, Dr. Wookey is a worthy recipient of the RAS Fowler Award. [Applause.]

Mr. Knight. The Winton Capital Award for Astronomy is awarded to Dr. Sugata Kaviraj. Dr. Kaviraj has an outstanding record of research which is mainly focussed on the stellar populations and evolution of early-type galaxies. His analysis of ultraviolet observations of those galaxies has shown that they are far from the 'red and dead' systems previously thought. Instead, many contain significant populations of young and intermediate-age stars. He has extended the scope of his work to ultraviolet observations of the evolutionary precursors to present-day early-type galaxies, both at intermediate redshifts and the transitional 'E+A' galaxies at low redshift, as well as evolutionary modelling of the galaxies and studies of their globular-cluster systems. He has been an exceptionally productive early-career scientist, with more than 30 refereed papers in the past four years and is a fitting recipient of the Winton Capital Award. [Applause.]

Dr. Gomez. The Winton Capital Award for Geophysics goes to Dr. Leigh Fletcher. Dr. Fletcher, currently a Glasstone Science Fellow at Oxford, has carried out definitive work on the thermal structure, compositional variation, and seasonal evolution of Saturn's atmosphere utilizing data from the *Composite Infrared Spectrometer* on-board the *Cassini* spacecraft. This work includes a *Science* paper on the thermal structure of Saturn's polar hot spots, including revealing the presence of the north-polar hexagon in the troposphere, but not in the lower stratosphere, which has important consequences for dynamical structures within this region. In addition, the analysis demonstrated a cloud-free region at the centre of the cyclonic vortex centred at Saturn's South Pole. For this definitive study of Saturn's atmospheric structure in the thermal infrared Dr. Fletcher is a worthy recipient of the Winton Capital Award. [Applause.]

Mr. Knight. An Honorary Fellowship of the Society has been awarded to Professor Jan Palouš.

Professor Palouš has undertaken fundamental research in galactic astronomy and has played a key rôle in the development of astronomy in the Czech Republic. From early work on the formation of super-star-clusters and detailed modelling of the winds from them, he went on to investigate how nuclear starbursts drive galactic winds and feed central black holes. A key contribution has been the discovery of ubiquitous H I super-shells and how their dynamics influence the large-scale structure of a galaxy.

Professor Palouš has been tireless in encouraging and nurturing the next generation of Czech astronomers, and in promoting the position of the Czech Republic as an active centre of European astronomy. He is Chair of the National Committee of Astronomy, President of the Council for International Affairs of the Czech Academy of Sciences, of which he is a former Director, and Head of Galaxies and Planetary Systems at its Astronomical Institute. Professor Palouš facilitated his country's admission to ESO, and sits on the ESO Council. In 2006 he was Chair of the National Organizing Committee for the IAU General Assembly in Prague. For his fundamental research in galactic astronomy and his key rôle in the development of astronomy in the Czech Republic, Professor Palouš is most deserving of election to Honorary Fellowship. [Applause.]

The President. Thank you very much — please thank Haley and Tom again. [Applause.] The next speaker is Mark Thompson and the title of his talk is 'Einstein at teatime — the popularization of science'.

Mr. M. Thompson. The popularization of science is of considerable importance, not just in advancing the public understanding of science, but also ensuring that the public understand what it is that science does for them and helping to develop the scientists of the future. Astronomy has two groups of people that are unique to the field of science yet pivotal in the success of popularization: professional astronomers and amateur astronomers. Both can engage at different levels but together pose a formidable force in breaking down the barriers between science and the 'Great British Public'.

Of all the different media available to engage with the public, TV, social networking, and public observing are considered amongst the most successful, particularly when trying to engage with young people. TV is great at getting large numbers of people engaged; for example, *Stargazing Live* enjoyed 3·6 million viewers and *Wonders of the Universe* had on average 3·7 million viewers. Getting astronomy into non-science programmes is a good way to engage an unsuspecting audience, such as the astronomy films on the *One Show*, which reaches 5 million viewers. Social networking such as Twitter and other interactive services are fantastic at getting astronomy to the people, and modern smartphone apps can get total beginners whizzing round the sky identifying all sorts of objects with ease.

The key to good public engagement, though, is not in how you get the message out, but what the message actually is. There should be no stigma attached to converting the message for public consumption. Keep in the back of your head, "What would Mum understand?". And if you think she would get it then you are probably along the right lines. It's important not to be frightened of sensationalizing the story too. If it gets it out there, then the job is done.

Remember, science and public outreach have the same goal, but to reach the audience it must be relevant in some way to their lives. In essence, good scientific outreach is not just a simplified scientific message but often a different message with its own values. The trick in achieving that is to forget being a scientist, become an editor and ask yourself the question, "Will it sell papers or get someone to tune in to the show?". If the answer is "no", then keep working on it. [Applause.]

The President. Thanks very much. We do have a little time for discussion. Like all good scientific discourses, we now have data that need to be reconciled, and we should do that before going to questions. So the 3·7 million is, I think, the viewing figure?

*Mr. Thompso*n. Yes.

The President. The 6 million includes DVD sales and downloads. These figures come from Brian Cox himself so I hope they are right! [Laughter.] *The Solar System* series, during the four weeks for which it ran, was the second-most-popular programme downloaded. Can anybody guess what the most popular download was? — *Eastenders*. The second statistic to note from that first series, that I am aware of, is that during the month that the series ran sales of telescopes at John Lewis doubled.

*Mr. Thompso*n. Actually I have an interesting addition to that: when we did the *Stargazing Live* show, which was the second of a series of three shows, planispheres sold out on Amazon.

The President. So these things have a huge impact. We have a little bit of time for discussion and questions for Mark.

Mr. H. Regnart. A simple suggestion — that the approach is not to dumb down, but to help people to rise up!

*Mr. Thompso*n. Yes, that can sometimes be challenging, but you are absolutely right. It is all about raising people's awareness and education.

Dr. P. Marshall. It is interesting you mention *Eastenders* as the most popular TV programme. Is there a discussion at the BBC about how to get messages about reasoning and science and so on into ordinary programmes?

*Mr. Thompso*n. Alas not! [Laughter.] What was interesting was the *Stargazing Live* show, because, being the BBC, the science department is the education department. The *Stargazing Live* show was actually a combination of the entertainment team and the science team, because the science team are not used to doing live broadcasts. So there is actually a move towards live educational stuff now. You may have seen *Lambing Live* — I watched it during my tea once; most off-putting [laughter]. There is a trend towards making science live and more interactive, but as far as I am aware there is no engagement around trying to get science into shows like *Eastenders*. I'll be going into the BBC, so I'll make the suggestion.

Professor M. Hendry. I was interested in your comments about television, so following on with Phil's question, I wonder if you are familiar with the show called *The Sparticle Mystery*?

*Mr. Thompso*n. Yes I am! I have children as well! [Laughter.]

Professor Hendry. It is a programme where all the adults are taken off into another dimension. The science wasn't entirely accurate, but I wondered whether you are aware of what consultation took place for a programme like that, and is that a strategy that should be pursued more?

*Mr. Thompso*n. For that programme specifically I don't know, but what I do know is that there are more scientific programmes for children that the BBC, CBBC, and independent TV production companies are looking at. So that work is being done. I think it is down to members of scientific organizations to contact them. If you've got ideas or suggestions, you should speak to the media organizations directly. That said, there is a scientific department at the BBC, which does advise on things like that, but it depends on whether they've got the right people at the right time. Unfortunately it is down to luck at the moment.

Dr. P. Evans. I have friends who are not physicists, who are not scientists, and who complain that scientists are far too dogmatic. Forgive me, but there was an extremely irritating guy on Radio 4 a few months ago who suggested that nearly all scientists are closed-minded because of how dogmatic they are. Really, that did not reflect on any scientist that I know. Do you think there are ways of forming a close relationship between media and scientists to find a right balance there?

*Mr. Thompso*n. Yes, I think there is a danger that scientists will be perceived to be closed-minded. The whole essence of science is about being open-minded to experimentation, that's the very foundation of science, so you're absolutely right, there is that risk. As to how you tackle it, I don't know; I think if you were to look at science education 50 years ago, it would be very different from how it is now. It is getting better, and scientists like you are becoming more media-savvy, because the media are such a big part of our lives these days. I think it is about trying to take people with you on that journey. We need to simplify the message while still lifting their education. I do think you need to use storytelling — not dumbing stuff down, but you need to take your scientific information down to a basic level and take people with you, and that way people will actually grow to love you rather than hate you and think that you are closed-minded.

Dr. K. Masters. I remember at the time of the total solar eclipse in the UK in 1999; it was on *Eastenders* and it was absolutely awful. I actually saw a picture of the Moon going across the Sun. I think it's definitely worth some effort to get some scientific advice into those programmes. [Laughter.]

*Mr. Thompso*n. One of the difficulties with the great BBC — I have to be careful with what I say of course: there is no one here from the BBC here, is there? [laughter] — is that all the different departments tend to work in very individual ways. So you've got the kids section, the science section, the entertainment section, *etc*. They don't tend to interact all that well. There are plenty of people giving advice to them on how to reflect accurately a solar eclipse, but you somehow have to lift everybody — researchers, producers, directors — you've got to get the message out to all those people that this resource is available to them within the BBC. And the BBC is a vast organization! You know, for the *One Show* you've got people who produce films in Cardiff, not far from here, in Bristol, all over the place! There are so many disparate organizations that trying to get information out to say that all this expertise is available is very difficult. It is getting better; it's not right, it's a long way off, but I think it is getting better slowly.

The President. Thank you very much, Mark. I just want to highlight one other thing. Mark made the point that he is from Norfolk; we are in North Wales, so there is a very strong regional dimension to outreach. Your last remark said that a lot of this can happen in London but it is not much good if you're in the rest of the UK, and again dispersing these activities is very important! So every member of the Society can participate in this, and it is one of those things that bring all the disparate areas of the Royal Astronomical Society together. My final remark is that some very strong supporters of science come from Norfolk: for example, Ian Taylor, who chairs the Select Committee, and also Baroness Shephard, who used to be Secretary of State for Education under John Major; these people are still very influential and very strong supporters of science.

It is now time for the Whitrow Lecture, which will be given by Professor Alex Vilenkin. The Whitrow Lecture is on a topic in cosmology and is usually with particular reference to the philosophical aspects of cosmology. Alex Vilenkin is the director of the Institute of Cosmology at Tufts University, but he was educated in Ukraine before moving to the United States. He has written over 150 papers on topics including eternal inflation, cosmic strings, and the quantum creation of the Universe. So it is my pleasure to introduce Professor Alex Vilenkin to give the Whitrow Lecture on 'The Principle of Mediocrity'.

Professor A. Vilenkin. [It is expected that a summary of this talk will appear in a future issue of *A&G*.]

The President. There is time for some questions, so who wants to kick off?

Dr. A. Pontzen. You've presented the normal quasi-classical picture where bubbles nucleate at a certain rate. Do you think that picture's accurate, and if so, what determines the specific times of the state transitions? If not, and we really live in an Everettian quantum superposition, are the quasi-classical counting results definitely still right, or could they be inaccurate?

Professor Vilenkin. Of course, semi-classical approximation has its range of applicability. If the tunnelling action is large, in other words, if the nucleation rate of the bubbles is very low, that's when the semi-classical picture applies. It will not apply if, for example, we have a vacuum which has Planck energy density, but if the energy density stays well below Planck, and the nucleation rate is low, then there is no reason to doubt the semi-classical picture.

Mr. A. Hawken. On your principle of mediocrity, I think it's already failed: all our laws of physics, being derived from a laboratory on Earth, if extrapolated to the Universe at large, don't seem to work. About 95% of the Universe is made up of some weird stuff, which is very surprising. It's akin to wearing a hat at a conference and believing that everyone else will be wearing a hat, but it turns out that you are the only person wearing a hat, so why do we expect ourselves to be typical observers?

Professor Vilenkin. Well, if I understand you correctly, you are saying that we are not typical because most of the Universe has no observers at all, and so we are very atypical, because we are in some tiny corner of the Universe which has conditions suitable for life. Is that the point?

Mr. Hawken. Kind of. [Laughter.] But it says that you expect the laws of physics accessible to an observer to be typical of the greater laws of physics of the Universe?

Professor Vilenkin. No.

Mr. Hawken. No?

Professor Vilenkin. No. I gave a very specific formulation of the principle of mediocrity, that we are typical in a certain class, which I didn't really take great care to specify. There are certain classes of observers. I'm not saying that we live in a typical environment in the Universe — that is certainly not true. Even in our observable region, the typical environment in the Universe is intergalactic space, and it is not very hospitable for life. So we live, of course, only where observers can live, and when we calculate the probability for the cosmological constant, we look at the fraction of matter clustered in galaxies, and any matter can cluster in galaxies only if the cosmological constant is very low. So most of the Universe will not satisfy this requirement, but there will be no observers there.

Professor T. Shanks. If the observational evidence for the cosmological constant went away, how much could it affect your arguments for the multiverse?

Professor Vilenkin. Well, the picture of the multiverse (or at least of eternal inflation) follows from the theory of inflation. 'Follows' may be a bit too strong a word, because you can construct models of inflation in which inflation is not eternal, but only by mutilating the theory. So, if the evidence for the cosmological constant goes away, then you have a situation similar to the situation with a black hole. Einstein's equations tell you exactly what is going to happen to you when you jump into a black hole. You may or may not believe that. Unless you really jump into a black hole you are not going to find out for sure. But you should trust your theory, at least to some degree, based on the evidence that we have for other things that it predicts.

Professor M. Griffin. Can you comment on the credibility of the notion that the Universe and we are part of a computational simulation? [Laughter.]

Professor Vilenkin. No. I have nothing to say! [Laughter.]

The President. Where's Carlos [Frenk] when you need him? [Laughter.]

Professor G. Chabrier. Do you have any explanation for the fact that you must get rid of 120 orders of magnitude for the cosmological constant between the early stages of the Universe and the period now, which is kind of an anthropic nature itself, and how does it apply to other multiverses?

Professor Vilenkin. The 120 orders of magnitude is basically between the observed value of the cosmological constant and the Planck density. So this is the full range of variation of the cosmological constant. And in principle, in the multiverse we expect bubbles to have values within this full range with positive and negative energy of that magnitude, so most of those bubbles will have huge energy densities, and only a tiny fraction of bubbles will have energies where any observers can exist. But I am not sure that I'm addressing your question.

Professor Chabrier. Well it's not exactly my point. My point was that the cosmological constant has to be tuned in a way that it has a given value today that we can observe today, so to speak. But in the early stages of the Universe, it had to be orders and orders of magnitude larger than now, and it is not exactly clear how it goes from the early value to the value we observe today.

Professor Vilenkin. How it goes there — there is no mystery in that. Actually, the models with high-energy vacua have appeared in particle physics well before they were used in the theory of inflation.

A Fellow. But the vacuum energy doesn't give the proper quantitative value for the cosmological constant, does it?

Professor Vilenkin. The present cosmological constant? Not the present cosmological constant — that's the problem. The greatest minds in physics tried to explain this value, but it's very hard to explain! Nobody came up with a solution, and the only hope for a long time was that it is zero. And if it is zero, you can hope for some symmetry principle that will explain why it is zero. But when it was measured as non-zero, and it is such that it dominates the Universe at the present time, any hope of explaining it from particle physics disappears. The anthropic explanation accounts for all of those features, and I think at this time it is the only feasible explanation.

Professor D. Lynden-Bell. You gave us the example of the anthropic principle explaining the precise value of the neutron mass, as compared to the proton mass. Let's suppose that sometime in the future, the particle physicists will actually get their act properly together and give us a proper rational explanation of why the neutron is that much heavier than the proton. And this is just down to ordinary standard physics. Will you in any way retract anything you say? [Laughter.]

Professor Vilenkin. You know, being a physicist I always prefer the situation where you can predict the number up to the 12th decimal point, as we do for the case of, say, the magnetic moment of the muon, rather than predicting within a few orders of magnitude, like we do in the case of the cosmological constant. We don't really know whether or not all the numbers are fixed by some fundamental theory, as many physicists hope, or there is a multiverse. Nature has already made her choice, but we don't know. I think we should be open to both possibilities.

A Fellow. There is some tentative evidence that the fine-structure constant has different values, larger in one universe and smaller in the other; is that consistent with your theory?

Professor Vilenkin. I think it is. Well, of course, the fact that it may be variable would be very interesting. But the multiverse picture does not really require it to vary within the observable region. I have some suspicions that it's hard to understand how this can happen, because basically when a parameter like the fine-structure constant changes, the radiative correction to the vacuum energy also changes. And that change would be huge, for the proposed variation of alpha. So I find it hard to understand, but who knows, maybe something subtle is going on.

The President. This has been a fantastically mind-expanding lecture; let's thank Professor Vilenkin once more. [Applause.]

THE REAL ORBITAL PERIOD OF THE DOUBLE-LINED SPECTROSCOPIC BINARY HD 31738

By F. C. Fekel
Tennessee State University
and
R. F. Griffin
Cambridge Observatories

The extraordinarily short orbital period given for HD 31738 in this *Magazine* in 2009 was mistaken. The true value, a 1-day^{-1} alias of the published one, is even shorter, 0·3102061 days.

Introduction

In Paper 209[1] of the long-running series of spectroscopic-orbit papers in this *Magazine*, one of the present authors (RFG) put forward an orbit of extraordinarily short period (0·45 days) for HD 31738. That object had previously been observed by FCF, who had recognized it as double-lined but had not identified a period for it. Alerted privately to the period proposed by RFG, FCF not only found that his measurements accorded with it, but (through having observed it in previous seasons) he was able to offer a more accurate value for it, enabling it to be refined further by the use of published[2] (though single-lined) radial velocities obtained 30 years before.

A curious discrepancy between the FCF and RFG data came to light, inasmuch as there seemed to be an inexplicable difference in the *phasing* although the two sets of observations gave identical periods. Hindsight is of course always very clear, and it obviously indicates that something was seriously wrong with the orbit, but all that was done at the time was that the observers merely agreed that they would both make further measurements of the object and together would sort the matter out in the ensuing season — as they hereby do. With uncharacteristic impetuosity, however, RFG went ahead and published[1] his orbit, duly acknowledging FCF's assistance in refining the period.

Disclaimer and apology

RFG is obliged now to eat humble pie and admit that the period that he gave proves to be an alias of the true one, which is even shorter than the 0·45 days given in Paper 209. At 0·31 days, it represents an orbital frequency greater by exactly one cycle per day than the published one. RFG has been quite aware of the problem of aliasing, having identified it in the work of others in such cases as HD 16884[1], HD 23642[3], the Hyades stars van Bueren 34 and 38[4], 1 Gem B[5], HD 51565[6], HD 89959 and HD 143705[7], and HD 152028[8] (some of the cited publications are collaborative). Now, however, he apologizes to the astronomical public for having fallen into the error himself. He hopes partly to redeem his name by participating in the correction described below.

New observations and revised orbit

Both authors of this paper have made fresh observations of HD 31738. FCF observed with the remotely operated Tennessee State University 2-m *Automatic Spectroscopic Telescope*[9], situated in south-eastern Arizona at Fairborn Observatory, which enabled him to make a dense series of measurements without even moving from his desk. Although those observations were begun in early 2009, it was not until late 2010 that the observing frequency was greatly enhanced in accord with our agreement for a more intense contemporaneous campaign. RFG, more prosaically, undertook observations in person with the *Coravel*-type spectrometer[10] at the coudé focus of the Cambridge 36-inch reflector.

From the Fairborn Observatory échelle spectra velocities were obtained from approximately 170 regions between λλ4920 and 7100 Å, centred on the rest wavelengths of relatively strong, mostly Fe I lines that are not excessively blended with other nearby strong features. The always-blended line profiles were initially fitted with Gaussian functions, but the rotational broadening of the components is such that the overlapping Gaussians produced velocities that exhibited systematic residuals. The reductions were therefore repeated with an empirical profile that was rotationally broadened by shifting and adding together a set of similar profiles after weighting them according to the limb darkening adopted for the Sun. Fekel *et al.*[11] have provided additional information about the general velocity analysis.

From 2010 December onward, 149 Fairborn measurements have been made, which together with 45 earlier ones that were already in hand make a total of 194. They are all listed in Table I, to which have been added the 21 Cambridge velocities that formed the basis of the original orbit[1]. Also included are six Cambridge measurements made in the 2009/10 season after the orbit paper[1] had gone to press, plus 16 from the recent (2010/11) season. Two of the original 21 measurements were of irresolvable blends and are not useable in the solution of the orbit. Among the more recent observations, two of the velocities of the primary, made at almost identical phases and listed within brackets in Table I, have been rejected on statistical grounds. There is, therefore, a total of 233 measurements of the primary and 235 of the secondary available to serve as the basis for the new orbit. To homogenize the data from the two sources, the Cambridge velocities have been subject to an empirical adjustment of $-1{\cdot}8$ km s^{-1} and have been given half-weight in comparison with the Fairborn Observatory ones. Furthermore, all the velocities of the secondary star have been half-weighted with respect to the corresponding ones of the primary.

A few earlier observations have been utilized to refine the orbital period. In a number of seasons, FCF obtained spectra of HD 31738 at Kitt Peak

TABLE I

Radial-velocity observations of HD 31738

The observations flagged with an asterisk were made at Cambridge; all others with the Tennessee automated telescope in Arizona

Date (UT)		MJD	Velocity Prim. km s⁻¹	Velocity Sec. km s⁻¹	Phase	(O – C) Prim. km s⁻¹	(O – C) Sec. km s⁻¹
2008 Nov.	8·115 *	54778·115	+14·3	−19·5	0·970	+0·3	−1·0
	19·082*	789·082	+5·2	+20·6	36·324	−0·6	0·0
	23·082*	793·082	+9·1	+3·9	49·218	−0·4	+1·0
Dec.	3·012 *	803·012	+7·4		81·229	—	—
	7·091 *	807·091	+5·4	+28·0	94·378	+1·2	−0·2
	10·035 *	810·035	+11·9	−9·8	103·869	−0·4	+0·5
	27·008*	827·008	+3·8	+31·2	158·584	+0·4	−0·9
2009 Jan.	2·977 *	54833·977	+13·6	−18·7	181·050	−0·2	−1·0
	6·917 *	837·917	+8·2		193·751	—	—
	13·957 *	844·957	+3·3	+35·8	216·446	+0·3	+1·6
	18·889*	849·889	+6·3	+24·5	232·345	+1·1	+0·7
	19·931 *	850·931	+6·4	+17·3	235·704	−0·3	+1·1
	20·944*	851·944	+13·6	−19·7	238·969	−0·4	−1·2
	23·928*	854·928	+4·5	+32·9	248·589	+1·0	+1·3
	29·763 *	860·763	+4·6	+32·8	267·399	+0·8	+2·3
	29·952 *	860·952	+13·9	−19·8	268·008	−0·2	−0·8
	30·924*	861·924	+11·2	−9·8	271·142	−0·8	−0·9
Feb.	10·878*	872·878	+3·9	+36·1	306·454	+1·0	+1·5
	10·896*	872·896	+2·1	+36·4	·512	−0·6	+0·7
	11·161	873·162	+5·2	+27·1	307·367	+0·7	+0·3
	11·866*	873·866	+6·5	+27·3	309·639	+1·8	+1·2
	19·111	881·111	+15·5	−19·8	332·993	+1·4	−0·8
	21·261	883·261	+13·8	−17·1	339·924	+0·3	−1·2
	21·822*	883·822	+7·9	+10·9	341·733	+0·1	−0·4
	26·131	888·131	+4·2	+27·1	355·625	−0·1	−0·6
Mar.	15·158	905·159	+3·3	+36·5	410·515	+0·6	+0·8
	21·183	911·183	+13·5	−19·2	429·936	−0·2	−2·3
Apr.	7·139	928·139	+4·8	+31·1	484·596	+1·1	+0·2
Aug.	8·434	55051·435	+14·3	−16·6	882·060	+0·6	+0·5
	23·419	066·419	+4·9	+26·1	930·364	+0·3	−0·3
	27·496	070·496	+2·4	+36·3	943·506	−0·3	+0·5
Sept.	18·414	092·414	+11·9	−5·2	1014·165	+0·6	+0·4
	23·310	097·310	+15·4	−17·4	1029·946	+1·6	+0·1
	25·353	099·353	+2·2	+34·9	1036·533	−0·6	−0·3
	30·308	104·308	+2·1	+35·3	1052·506	−0·6	−0·5
Oct.	17·256	121·256	+12·7	−8·6	1107·142	+0·7	+0·3
	25·156 *	129·156	+4·5	+32·1	1132·607	+0·6	+2·3
Nov.	1·388	136·388	+14·0	−16·4	1155·921	+0·6	−0·7
	1·413	136·413	+14·8	−18·8	1156·002	+0·7	+0·2
	1·438	136·438	+14·4	−15·9	·082	+1·0	−0·5
	3·388	138·388	+4·8	+26·8	1162·369	+0·3	−0·2
	3·413	138·413	+3·1	+35·2	·450	+0·2	+0·8
	3·438	138·438	+2·9	+36·1	·530	+0·1	+0·8
	4·196	139·196	+14·3	−18·5	1164·972	+0·3	+0·1
	6·365	141·365	+14·5	−18·9	1171·966	+0·5	−0·5
	19·368	154·368	+13·0	−11·2	1213·882	+0·4	+0·6
	24·066*	159·066	+15·0	−20·2	1229·027	+1·0	−1·6
	24·366	159·366	+14·9	−20·1	·995	+0·8	−1·1
Dec.	23·058 *	188·058	+2·7	+36·0	1322·488	0·0	+0·3
2010 Jan.	29·931 *	55225·931	+2·9	+34·2	1444·577	−0·4	+1·6
	30·879*	226·879	+3·6	+28·2	1447·633	−1·0	+1·5
	31·907*	227·907	+12·8	−17·7	1450·947	−1·0	−0·1
Feb.	12·135	239·135	+12·1	−9·2	1487·143	+0·1	−0·5

TABLE I (*continued*)

Date (*UT*)	*MJD*	*Velocity* *Prim.* *km s⁻¹*	*Sec.* *km s⁻¹*	*Phase*	(*O–C*) *Prim.* *km s⁻¹*	*Sec.* *km s⁻¹*
2010 Feb. 27·106	55254·106	+4·1	+29·9	1535·403	+0·4	−1·0
Mar. 14·168	269·168	+14·7	−17·8	1583·960	+0·8	+0·4
20·139	275·139	+10·1	+2·2	1603·208	+0·2	+0·9
26·140	281·140	+2·3	+35·0	1622·552	−0·7	+0·6
Apr. 4·136	290·136	+3·1	+34·8	1651·552	+0·1	+0·5
11·117	297·117	+14·4	−17·3	1674·056	+0·6	+0·1
Sept. 19·461	458·461	+10·7	−4·7	2194·176	−0·2	−0·8
25·515	464·515	+6·6	+18·6	2213·693	+0·2	+0·6
Oct. 1·511	470·511	+13·9	−18·6	2233·020	−0·2	+0·2
8·488	477·488	+3·1	+35·2	2255·513	+0·4	−0·5
11·401	480·401	+13·2	−13·7	2264·904	+0·1	+0·5
16·464	485·464	+8·9	+4·8	2281·223	−0·4	+1·0
17·353	486·353	+13·1	−14·7	2284·090	−0·1	0·0
20·174*	489·174	(+7·9)	−1·7	2293·184	−2·8	+1·0
25·442	494·442	+10·3	−4·6	2310·168	−0·9	+0·5
30·501	499·501	+3·0	+35·3	2326·474	+0·3	−0·1
Nov. 4·476	504·476	+3·6	+35·4	2342·512	+0·9	−0·3
10·476	510·476	+11·8	−8·7	2361·855	−0·1	−0·2
15·072*	515·072	+6·8	+20·3	2376·670	+1·2	−1·2
16·388	516·388	+13·6	−14·5	2380·914	+0·3	+0·6
23·994*	523·994	+4·3	+34·1	2405·432	+1·1	+0·8
25·377	525·377	+13·6	−12·3	2409·890	+0·8	+0·4
26·090*	526·090	(+7·6)	−0·3	2412·189	−2·9	+1·6
27·101*	527·101	+3·2	+35·7	2415·448	+0·2	+1·3
27·988*	527·988	+5·8	+18·8	2418·307	−0·6	+0·8
Dec. 1·182	531·182	+4·4	+29·5	2428·604	+0·6	−0·6
1·232	531·232	+8·6	+7·4	·766	−0·3	+1·7
1·282	531·282	+13·6	−15·8	·927	+0·1	+0·4
1·332	531·332	+12·9	−15·9	2429·088	−0·4	−0·9
1·382	531·382	+8·5	+7·4	·249	+0·1	−0·8
1·432	531·432	+3·2	+30·5	·410	−0·3	−1·1
1·447	531·447	+2·8	+34·4	·457	−0·1	−0·4
2·132	532·132	+6·4	+21·1	2431·667	+0·9	−1·0
2·182	532·182	+11·1	−5·3	·828	0·0	−0·8
2·232	532·232	+14·1	−19·4	·989	0·0	−0·4
2·282	532·282	+10·9	−8·2	2432·150	−0·8	−0·5
2·332	532·332	+7·4	+17·9	·312	+1·2	−0·8
2·382	532·382	+2·5	+34·5	·473	−0·2	−0·9
2·432	532·432	+4·6	+26·1	·634	0·0	−0·6
2·447	532·447	+6·0	+18·9	·680	+0·1	−1·2
2·482	532·482	+9·9	+1·6	·795	−0·1	+0·9
3·132	533·132	+12·1	−13·6	2434·890	−0·7	−0·8
3·182	533·182	+13·9	−17·9	2435·052	+0·1	−0·3
3·232	533·232	+8·9	+2·6	·213	−0·8	+0·6
3·282	533·282	+4·0	+26·4	·374	−0·4	−1·3
3·332	533·332	+3·0	+34·4	·535	+0·2	−0·7
3·382	533·382	+6·8	+16·6	·696	+0·3	−0·8
4·232	534·232	+3·6	+33·5	2438·437	+0·5	−0·2
4·282	534·282	+4·3	+30·8	·598	+0·6	0·0
4·332	534·332	+9·0	+7·5	·759	+0·3	+0·6
4·382	534·382	+13·4	−15·7	·920	0·0	0·0
6·093*	536·093	+2·6	+32·2	2444·435	−0·5	−1·4
6·132	536·132	+3·3	+34·0	·561	+0·2	+0·2
6·182	536·182	+8·3	+13·0	·723	+0·9	−0·1
6·232	536·232	+13·3	−12·2	·884	+0·6	−0·1
6·282	536·282	+13·6	−18·4	2445·045	−0·3	−0·5
6·332	536·332	+9·4	+1·8	·206	−0·5	+0·9
6·382	536·382	+4·2	+26·6	·367	−0·3	−0·2
6·432	536·432	+3·1	+34·8	·529	+0·4	−0·6

TABLE I (*continued*)

Date (UT)		*MJD*	*Velocity Prim. km s⁻¹*	*Velocity Sec. km s⁻¹*	*Phase*	*(O–C) Prim. km s⁻¹*	*(O–C) Sec. km s⁻¹*
2010 Dec.	6·447	55536·447	+3·8	+32·0	2445·575	+0·5	−0·8
	6·482	536·482	+6·6	+18·3	·690	+0·3	−0·2
	7·453	537·453	+10·8	−3·5	2448·820	0·0	−0·2
	8·182	538·182	+10·7	−3·8	2451·170	−0·4	+1·0
	8·232	538·232	+5·7	+21·9	·331	+0·1	+0·1
	8·282	538·282	+2·4	+35·0	·492	−0·3	−0·8
	8·332	538·332	+5·2	+23·2	·654	+0·1	−0·8
	8·382	538·382	+11·0	−2·8	·815	+0·3	−0·3
	8·432	538·432	+14·3	−18·9	·976	+0·2	−0·2
	8·446	538·447	+14·1	−18·8	2452·022	0·0	0·0
	8·482	538·482	+11·5	−9·8	·137	−0·6	−0·3
	9·032*	539·032	+12·9	−15·3	2453·909	−0·3	−0·6
	9·132	539·132	+8·8	+4·9	2454·232	−0·2	−0·5
	9·182	539·182	+3·7	+29·2	·394	−0·2	−0·7
	9·232	539·232	+3·2	+33·7	·555	+0·2	−0·5
	9·282	539·282	+7·8	+13·6	·716	+0·6	−0·6
	9·332	539·332	+13·1	−11·2	·877	+0·6	+0·1
	9·382	539·382	+13·9	−18·2	2455·038	−0·1	0·0
	9·432	539·432	+9·6	+0·6	·199	−0·6	+0·8
	10·102	540·102	+4·9	+25·3	2457·357	+0·1	−0·2
	10·116	540·116	+3·4	+31·4	·404	−0·3	+0·4
	10·130	540·130	+2·7	+33·9	·450	−0·2	−0·6
	10·145	540·145	+2·6	+35·3	·497	−0·1	−0·5
	10·159	540·159	+3·2	+35·2	·543	+0·3	+0·4
	10·174	540·174	+3·8	+31·1	·590	+0·3	−0·5
	10·188	540·188	+4·7	+25·7	·636	+0·1	−0·7
	10·202	540·202	+6·9	+19·3	·682	+0·9	−0·4
	10·217	540·217	+7·9	+12·7	·729	+0·3	+0·7
	10·231	540·231	+9·0	+5·7	·776	−0·3	+1·8
	10·246	540·246	+10·1	−4·0	·822	−0·8	−0·4
	10·260	540·260	+12·3	−10·7	·869	0·0	−0·4
	10·275	540·275	+13·6	−15·4	·916	+0·3	−0·1
	10·289	540·289	+14·2	−18·4	·962	+0·2	−0·1
	10·304	540·304	+14·1	−19·5	2458·009	0·0	−0·5
	10·318	540·318	+13·5	−18·0	·056	−0·3	−0·6
	10·333	540·333	+12·2	−13·7	·102	−0·8	−0·2
	10·347	540·347	+11·1	−7·8	·149	−0·7	0·0
	10·362	540·362	+9·5	0·0	·196	−0·8	+0·7
	10·376	540·376	+8·9	+5·7	·243	+0·3	−1·5
	10·391	540·391	+7·3	+14·0	·290	+0·3	−1·2
	10·405	540·405	+5·7	+22·5	·337	+0·3	−0·1
	10·420	540·420	+3·6	+28·1	·384	−0·5	−0·7
	10·434	540·434	+2·8	+32·2	·430	−0·4	−1·0
	10·449	540·449	+2·7	+34·9	·477	0·0	−0·6
	10·463	540·463	+2·8	+35·4	·524	+0·1	−0·1
	11·332	541·332	+6·4	+21·5	2461·324	+0·6	+0·7
	11·382	541·382	+3·0	+35·5	·486	+0·3	−0·2
	11·432	541·432	+4·6	+24·4	·647	−0·3	−0·5
	12·020*	542·020	+3·6	+36·1	2463·542	+0·8	+1·2
	12·132	542·132	+13·7	−13·8	·903	+0·6	+0·3
	12·182	542·182	+13·4	−17·2	2464·065	−0·2	−0·4
	12·232	542·232	+9·2	+3·8	·226	−0·1	−0·4
	12·451	542·451	+13·8	−17·0	·932	+0·2	−0·4
	13·132	543·132	+12·0	−10·6	2467·127	−0·4	+0·1
	13·182	543·182	+7·4	+14·0	·288	+0·4	−0·9
	13·232	543·232	+2·5	+33·5	·449	−0·4	−0·9
	13·282	543·282	+4·3	+29·0	·611	+0·3	−0·4
	13·311	543·311	+7·5	+15·4	·704	+0·8	−0·8
	13·332	543·332	+9·3	+5·4	·772	+0·1	+0·8

TABLE I (*continued*)

Date (UT)		*MJD*	*Velocity Prim. km s⁻¹*	*Velocity Sec. km s⁻¹*	*Phase*	*(O–C) Prim. km s⁻¹*	*(O–C) Sec. km s⁻¹*
2010 Dec.	13·382	55543·382	+13·8	−18·1	2467·933	+0·2	−1·5
	13·432	543·432	+12·9	−14·8	2468·094	−0·2	−0·4
	14·132	544·132	+4·7	+24·7	2470·350	−0·3	+0·1
	14·182	544·182	+3·4	+35·6	·512	+0·7	−0·1
	14·232	544·232	+5·7	+21·0	·673	0·0	−0·2
	14·282	544·282	+11·5	−4·9	·834	+0·2	+0·5
	14·312	544·312	+13·7	−16·8	·929	+0·1	−0·4
	14·332	544·332	+14·2	−19·4	·995	+0·1	−0·4
	14·382	544·382	+11·9	−6·4	2471·156	+0·3	+0·4
	14·432	544·432	+5·9	+20·8	·318	−0·1	+1·1
	15·132	545·132	+3·1	+33·6	2473·574	−0·2	+0·7
	15·182	545·182	+8·2	+11·4	·735	+0·3	+0·5
	15·232	545·232	+13·6	−13·1	·896	+0·7	+0·3
	15·282	545·282	+13·5	−18·0	2474·058	−0·2	−0·7
	15·297	545·296	+12·1	−14·1	·104	−0·8	−0·7
	15·332	545·332	+9·0	+3·8	·219	−0·5	+0·7
	15·382	545·382	+3·2	+26·9	·380	−1·0	−1·5
	15·432	545·432	+3·3	+34·9	·541	+0·5	0·0
	16·132	546·132	+9·6	+1·9	2476·798	−0·5	+1·6
	16·182	546·182	+14·2	−18·0	·959	+0·3	+0·1
	16·232	546·232	+11·9	−12·3	2477·120	−0·7	−0·7
	16·282	546·282	+7·6	+12·5	·281	+0·3	−1·2
	16·296	546·296	+5·7	+20·4	·328	0·0	−0·9
	16·332	546·332	+2·4	+32·5	·443	−0·6	−1·5
	16·382	546·382	+4·3	+29·7	·604	+0·5	−0·5
	17·056*	547·056	+8·3	+5·3	2479·776	−1·0	+1·4
	18·132	548·132	+8·2	+6·8	2483·245	−0·4	−0·7
	18·182	548·182	+3·0	+29·7	·406	−0·6	−1·5
	18·232	548·232	+3·0	+33·2	·567	−0·2	−0·2
	18·995*	548·995	+12·5	−20·1	2486·027	−1·5	−1·4
	19·132	549·132	+2·3	+35·2	·468	−0·5	−0·1
	19·182	549·182	+4·3	+26·7	·630	−0·2	−0·5
	19·232	549·232	+9·8	+2·7	·791	0·0	+1·3
	19·282	549·282	+13·8	−18·7	·952	−0·1	−0·9
	19·296	549·296	+14·4	−19·4	·998	+0·3	−0·4
	19·332	549·332	+12·5	−12·8	2487·113	−0·2	−0·4
	19·382	549·382	+7·8	+11·1	·274	+0·3	−1·5
	19·432	549·432	+2·5	+32·3	·436	−0·6	−1·3
	20·232	550·232	+13·9	−19·6	2490·014	−0·2	−0·7
	20·282	550·282	+11·0	−3·2	·176	0·0	+0·8
	20·296	550·296	+9·0	+3·4	·222	−0·4	−0·2
	20·332	550·332	+5·6	+23·3	·337	+0·2	+0·7
	21·432	551·432	+13·5	−12·8	2493·883	+0·9	−0·9
	27·321	557·321	+12·0	−10·3	2512·867	−0·2	−0·2
2011 Jan.	3·276	55564·276	+7·7	+13·4	2535·288	+0·7	−1·5
	9·291	570·291	+6·8	+19·4	2554·677	+0·9	−1·0
	9·995*	570·995	+13·3	−18·3	2556·948	−0·5	−0·7
	13·274	574·274	+2·6	+35·5	2567·518	−0·1	−0·1
	14·167	575·167	+3·0	+29·4	2570·396	−0·8	−0·7
	14·946*	575·946	+11·3	−15·2	2572·908	−1·9	−0·6
	16·161	577·161	+11·0	−4·2	2576·825	0·0	−0·1
	17·215	578·215	+8·8	+4·0	2580·224	−0·5	+0·2
	18·215	579·215	+2·8	+34·1	2583·447	−0·2	−0·2
	18·929*	579·929	+8·8	+8·0	2585·748	+0·5	−0·8
	20·215	581·215	+13·3	−13·8	2589·894	+0·4	−0·7
	21·215	582·215	+12·6	−11·7	2593·117	0·0	+0·2
	22·215	583·215	+5·7	+22·5	2596·341	+0·4	−0·7
	23·338	584·338	+14·3	−18·1	2599·962	+0·3	+0·2

TABLE I (*concluded*)

Date (UT)			MJD	Velocity Prim. km s⁻¹	Velocity Sec. km s⁻¹	Phase	(O – C) Prim. km s⁻¹	(O – C) Sec. km s⁻¹
2011	Jan.	24·886*	55585·886	+13·5	−18·1	2604·951	−0·3	−0·4
		29·314	590·314	+8·7	+5·2	2619·225	−0·6	+1·0
		31·886*	592·886	+1·9	+35·4	2627·517	−0·8	−0·3
	Feb.	7·228	599·228	+14·4	−18·4	2647·961	+0·5	−0·2
		13·173	605·173	+13·2	−10·9	2667·126	+0·8	0·0
		17·147	609·147	+14·4	−16·6	2679·936	+0·7	+0·2
		23·176	615·176	+4·3	+27·6	2699·372	−0·1	+0·2
	Mar.	7·150	627·150	+14·8	−18·5	2737·974	+0·8	+0·2

National Observatory (KPNO) with the coudé-feed telescope. Tomkin & Fekel[12] have discussed in general the reduction and velocity measurement of such spectra. Fifteen observations of HD 31738, obtained between 1991 and 2003, have been adjudged useful here. However, the reductions of those earlier unpublished velocities were performed by approximating the line profiles with Gaussian functions, which we have found to produce systematic residuals. We have therefore used the KPNO velocities, which increase the time base nearly tenfold, only in a preliminary step simply to improve the orbital period, and have imposed the resulting value on our final solution, which is based on the Fairborn and Cambridge observations alone.

The new period, a close approximation to which was first divined by FCF on the basis of the intensive measurements with the automated telescope, is 0·31020614 days. Owing to the much increased time base of our own measurements, there is no longer any occasion to try to refine it by appeal to the old single-lined measurements published by Balona[2], although they do qualitatively exhibit the expected relationship to the orbit, following the velocity changes of the *secondary* component though with greatly muted amplitude.

With the originally proposed period[1] of 0·4502588 days, reasonably good fits appear to be obtained with each source of measurements *separately*, but when the two sets are concatenated the solution with that period goes haywire because the two sets are out of phase with one another by an amount related to the time-equivalent of the difference in longitudes between the observatories. Significantly, however, the new period is a much better fit to the original batch of secondary observations (*cf.* ref. 1, Fig. 7), whose phases were considerably falsified by the use of the wrong period in cases where the observations were made at appreciable hour angles. Indeed, in the original solution[1] the secondary velocities needed to be weighted only 1/10 in comparison with those of the primary, and the only reason that they were not worse still was because observations were restricted to relatively small hour angles by the low declination (~0°) of HD 31738 and physical obstructions to the movement of the Cambridge telescope. Of course the same phasing errors applied equally to the observations of the primary star, but the very much (fivefold) smaller velocity amplitude of that component tended to bury them in the noise and render them comparatively innocuous.

The old and new periods, inverted to frequencies, differ by 1·0027 day^{-1}. The reason that the difference is not the exact inverse day is that all intervals between observations are necessarily near to multiples of the time between successive culminations of the star, *i.e.*, of a *sidereal* day. Expressed in inverse sidereal days, the difference between the old and new frequencies (2·214881 and 3·214861 d^{-1} respectively) is seen to be almost exactly unity, differing by only two units in the fifth decimal place. The only time that a photometric period has been proposed for the star (by Strassmeier *et al.*[13], from observations in 1984–86) it was 4·55 days, which, expressed as a frequency in inverse sidereal days, is 0·219 d^{-1} and so is very probably another alias of the orbital period. That likelihood was noted in the paper[1] that gave the erroneous orbital period, but the reference there to the *sum* of the orbital and photometric frequencies being close to 2 inverse days should have been to the *difference*, as can be seen from the numbers here. There are other undoubted (but usually slow and irregular, as is evident from the *Hipparcos* 'epoch photometry') variations in the magnitude of HD 31738, which has the variable-star designation V1198 Ori.

The new orbit is illustrated in Fig. 1 and its parameters are as follows:

$P = 0{\cdot}31020614 \pm 0{\cdot}00000011$ days*	$(T_0)_{2156} =$ MJD $55446{\cdot}61866 \pm 0{\cdot}00013$
$\gamma = +8{\cdot}38 \pm 0{\cdot}03$ km s^{-1}	$a_1 \sin i = 0{\cdot}02445 \pm 0{\cdot}00020$ Gm
$K_1 = 5{\cdot}73 \pm 0{\cdot}05$ km s^{-1}	$a_2 \sin i = 0{\cdot}1170 \pm 0{\cdot}0003$ Gm
$K_2 = 27{\cdot}42 \pm 0{\cdot}08$ km s^{-1}	$f(m_1) = 0{\cdot}00000607 \pm 0{\cdot}00000015\ M_\odot$
$q = 4{\cdot}78 \pm 0{\cdot}04\ (= m_1/m_2)$	$f(m_2) = 0{\cdot}000664 \pm 0{\cdot}000006\ M_\odot$
$e \equiv 0$	$m_1 \sin^3 i = 0{\cdot}000971 \pm 0{\cdot}000008\ M_\odot$
ω is undefined in a circular orbit	$m_2 \sin^3 i = 0{\cdot}0002030 \pm 0{\cdot}0000029\ M_\odot$

R.m.s. residual for an observation of unit weight = 0·51 km s^{-1}

*Period determined with the inclusion of 15 earlier KPNO velocities not used in final solution. The 'true' period (in the rest-frame of the system) is 0·31019746 ± 0·00000011 days. It differs from the observed period by 76 standard deviations.

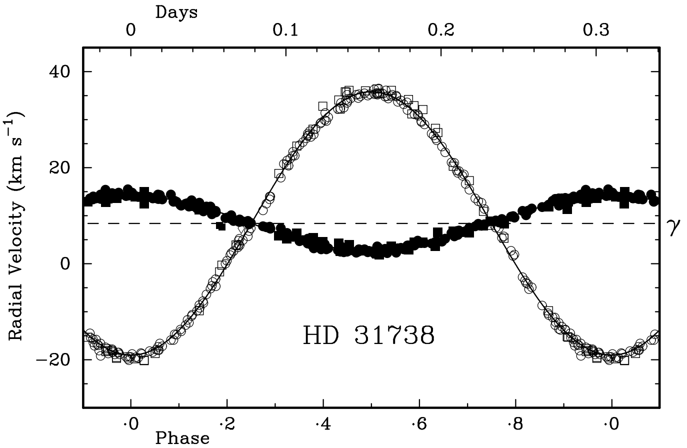

FIG. 1

The observed radial velocities of HD 31738 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. Radial velocities of the primary are plotted with filled symbols, circles for Fairborn and squares for Cambridge, while those of the secondary are indicated by corresponding open symbols. Two rejected measurements of the primary are shown as *small* filled squares near phase ·2.

The smallness of the formal standard errors of the amplitudes is misleading. There are differences, very significant statistically, between the amplitudes found from the Fairborn and Cambridge data sets separately. The overwhelming number of the Fairborn ones would cause them to dominate the solution, even if they were not weighted more heavily than the Cambridge ones, which are thereby made to appear worse than they really are because discrepancies that are actually systematic are treated as random. In the case of the secondary, the tendency of the Cambridge observations (squares) to be 'more extreme' near the nodes is obvious from Fig. 1. A sufficiently jaundiced eye might also notice a tendency for the velocities in general to be drawn towards the γ-velocity when they are within about 15 km s^{-1} of it; that could possibly occur as a result of non-sphericity (not considered in the radial-velocity reduction procedure) of the stars.

We give here a comparison of the amplitudes, their ratio q, and the mean-square residuals for each data set separately when computed from (*a*) Fairborn only, (*b*) Cambridge only, and (*c*) both, with Cambridge half-weighted (the same solution as in the informal table above). In the separate solutions (*a* and *b*), the mean-square residuals are given 'as computed' (the Cambridge ones have not been halved), but in all cases the secondary's 'raw' mean squares have been halved, since it is clear from all solutions that the observations of the secondary are less accurate than those of the primary.

Quantity		*Fairborn solution*	*Cambridge solution*	*Joint solution*
K_1		5·78 ± 0·05	5·29 ± 0·14	5·73 ± 0·05
K_2		27·33 ± 0·08	28·24 ± 0·25	27·42 ± 0·08
q		4·73 ± 0·04	5·34 ± 0·15	4·78 ± 0·04
Mean-square deviations (km s^{-1}):				
Fairborn	A	0·24	0·37	0·24
	B	0·24	0·49	0·25
Cambridge	A	0·71	0·55	0·34
	B	0·69	0·43	0·32

It is seen that the differences between the amplitudes computed from the two data sources individually are more than three times their formal standard errors, and the difference in q is about four standard errors. We have not identified any obvious reason for the discrepancies, nor are we in a position to recommend one solution in preference to another. We can only advise that the true uncertainties are considerably greater than the formal ones, and might prudently be taken to be comparable with the discrepancies seen in the little table immediately above. The perceptive reader will also find some interest in the tabulated mean-square deviations. The fact that the numbers for Fairborn data from their own Fairborn solution are about half those of the Cambridge data from the Cambridge solution validates our attribution of half-weight to the Cambridge velocities in the merged data set. The similarity of the numbers for A and B in every case validates our adoption of half-weight for the observations of B. The fact that, in the case of star A, for which the numerical value of the discrepancy in amplitudes is less than for B, the Fairborn velocities have smaller residuals than the Cambridge ones *even from the solution computed from the Cambridge measurements alone* further emphasizes (if only qualitatively) the superiority of the Fairborn observations in terms of random errors.

In comparison with the orbital elements given in 2009 on the basis of the erroneous period, most of the elements are in principle unchanged in the new solution, although the increase in the data base has appreciably improved their precision. The γ-velocity (when account is taken of the empirical correction made to the Cambridge observations here), the velocity amplitudes, and the zero eccentricity remain much as before; it is only the timing information and the repercussions that it has on the 'derived' elements (the separations and masses) that have changed. In particular, we note that the sum of the projected rotational velocities, previously determined independently by each of us[1,14] as being close to 23 and 9 km s^{-1} for the primary and secondary, respectively*, remains the same as the sum of the projected orbital velocity amplitudes to well within observational uncertainty, demonstrating that the stars are touching one another.

The original paper[1] neglected to remark how that conclusion is reinforced by the fact that the ratio of the rotational velocities (and thus of the stellar radii) is very close to the square root of the mass ratio of the components, confirming that the gravitational forces of the two stars balance (as they obviously ought to do) at the point of contact between them.

We turn now to the revisions needed to the original discussion[1] of the HD 31738 system. The shorter period requires the projected radii of the two stars (simply computed from the period and rotational velocities) to be reduced, to about 0·135 and 0·055 $R_\odot$ for the primary and secondary, respectively. The discussion[1] gave reasons (which are still valid), starting from the known parallax, for suggesting that the primary must be a little cooler and a little larger than the Sun, with a true radius close to 1·2 $R_\odot$; thus it now appears to be about nine times the projected radius, making $\sin i \sim 1/9$ or $i \sim 6°$. The reduction in period has the same effect in reducing the projected linear separation of the stars ($a_1 \sin i + a_2 \sin i$ in the informal table above), leaving the implied actual separation at about 0·0086 AU. Inserting that quantity, and the orbital period of 0·00085 years, into the Keplerian equation $m = a^3/P^2$, where all the quantities are in Solar System units, we obtain m, the sum of the masses, to be about 0·88 $M_\odot$. That is still remarkably small for a star whose size and colour put it not far from solar type, plus its companion, which is only about one magnitude fainter. There has clearly been opportunity for mass transfer in this contact binary, and we must suppose that some mass has been lost to the system.

Following the procedure of Strassmeier & Fekel[15] we have compared a KPNO spectrum of HD 31738, which has the components at a phase near maximum velocity separation, with the spectra of various G-dwarf stars with well-determined spectral types. For HD 31738 we conclude that the spectral type of the more-massive star is G8 V and that of the less-massive star is G2 V. Were this system to be seen at a significantly higher inclination, it would have the light-curve of a W UMa variable, and because the more-massive star is larger but cooler than its companion it would be assigned to the W-type subclass, *e.g.*, ref. 16.

For normal main-sequence stars the spectral types would suggest a total mass of nearly 2 $M_\odot$, which is clearly at odds with our estimate that the sum of the masses for HD 31738 is just 0·88 $M_\odot$. However, such a discrepancy is not particularly unusual for a contact system. For example, the work of Pribulla *et al.*[17] indicates that HD 31738 has properties somewhat similar to those of V345 Gem. That W-type contact binary has a shorter period of 0·275 days, a

*Re-determination of the rotational velocities by FCF from the abundant new Fairborn Observatory spectra yields values of 22 and 11 km s^{-1}.

higher mass ratio of 7·0, and an earlier spectral type of F7 V, but like HD 31738, its total mass, being in that case not much greater than 1·05 $M_\odot$, is clearly deficient relative to normal main-sequence stars.

More generally, Gazeas & Stępień[18] have tabulated masses derived from light-curve modelling of 112 contact binaries. From those data they determined period–mass relationships for the primary and secondary components. Those relationships show a great deal of scatter, but the resulting power-law fits would indicate a mass sum of 1·44 $M_\odot$ for HD 31738. Although that value is much larger than our estimate of 0·88 $M_\odot$, it is still significantly smaller than the expected main-sequence mass total.

While a comparison of the properties of HD 31738 with those of field W UMa stars, such as those listed in the catalogue of Pribulla *et al.*[16] and in an extensive series of papers by Rucinski, Pribulla, and their colleagues, *e.g.*, refs. 19 & 20, suggests that, although the magnitude difference and mass ratio of the HD 31738 components are larger than most, other general properties of the HD 31738 system are in accord with predictions for contact binaries. For example, the period–colour relationship[21] of W UMa stars produces $(B-V) = 0^{\rm m}\!\cdot\!73$, to be compared with an observed value of $0^{\rm m}\!\cdot\!71$. Likewise, if we adopt the apparent magnitude and $(B-V)$ colour from the *Hipparcos* catalogue, the absolute-magnitude calibration of Rucinski & Duerbeck[22] for stars of the W UMa type results in a parallax of $0''\!\cdot\!029$, a value that is spot-on the newly reduced[23] *Hipparcos* value of $0''\!\cdot\!0290$.

W UMa-type variables are often part of multiple systems. From their northern-sky sample, Pribulla & Rucinski[24] estimated a lower limit of 59% for the frequency of contact binaries that are part of triple systems. Although the solutions of our respective data sets produce slightly different γ-velocities, the velocities to date provide no strong evidence for a third component.

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SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 220: 60 PISCIUM, 27 ARIETIS, EZ URSAE MAJORIS, AND 4 EQUULEI

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The four bright stars have conspicuously variable radial velocities; their orbits could well have been determined 100 years ago. 60 Psc and 4 Equ have orbital periods near five years, whereas 27 Ari and EZ UMa have periods of only 130 and 184 days, respectively. The orbital eccentricities are moderate apart from that of EZ UMa, which is low. 27 Ari is distinguished by its high (negative) γ-velocity of -118 km s^{-1}. The large mass function of 27 Ari may indicate that its companion is itself a binary system.

Introduction

It is a constant source of amazement to the writer that there are so many stars that have late-type spectra ideally suited to radial-velocity determination, and are bright enough not merely to be in the *Bright Star Catalogue* but actually to have constellation designations, that still need to have their orbits determined, although in many cases they have been known for a long time to be spectroscopic binaries. The type of spectroscopy that was enabling the Lick Observatory to make a complete survey[1] of the radial velocities of the bright stars as far as $5^{m}\cdot5$ a hundred years ago, which produced velocities that were easily good to 1 km s^{-1} for late-type stars, could readily have obtained the orbits that the writer is at last furnishing here. This series of papers has already documented for the first time the orbits of more than 40 stars possessing constellation designations, not counting those that have received them only as variable-star designations or because they are faint companions to genuine examples of the ilk. Now, orbits are presented for another three 'genuine' ones, plus one that owes its constellation name to photometric variability although it is also a 'Bright Star' in its own right.

60 Piscium (*HR 216, HD 4526*)

60 Psc is a 6^{m} star in a visually barren area of sky; there is nothing appreciably brighter than δ Psc ($4^{m}\cdot4$, about 1° north of 60) for more than 10° around it. The star's magnitude and colours have been measured[2–4] as $V = 5^{m}\cdot98$, $(B-V) = 0^{m}\cdot94$, $(U-B) = 0^{m}\cdot70$.

The radial velocity of 60 Psc was measured from three plates taken with the 60-inch reflector at Mount Wilson in the 1913/14 season; a mean value was published quite promptly by Adams[5]. The spectra, which were classified as G6, were also used in connection with the development of the principles of 'spectroscopic parallaxes', stemming from the recognition that certain spectral characteristics are sensitive to stellar luminosity, or (to put it more accurately) to surface gravity. An initial paper on the method, with results for 500 stars, was published by Adams & Kohlschütter[6] in 1914. For stars such as 60 Psc, for which no parallax was known, they used the proper motion as a rough-and-ready indicator of luminosity; on that basis they estimated the absolute magnitude at −2, and from the spectroscopic criteria they obtained −3. Although those results were far too bright (the *Hipparcos*-based absolute magnitude is slightly fainter than zero) they did at least identify the star as a giant as opposed to a dwarf. A subsequent (1935) Mount Wilson paper[7] starts, "The determination of the absolute magnitudes of stars from their spectra has been continued as a major program of research since the method was first developed here *and reduced to a practical basis in 1916*." The italics represent an emphasis added by the present writer; the remark clearly recognizes the preliminary nature of the original Adams & Kohlschütter[6] paper of 1915. The Mount Wilson observers had a number of bites at the same cherry, increasing as they did so both their confidence in the method and the number of stars treated. 60 Psc features again in a 1921 paper[8] giving results for 1646 stars; in the case of interest the type was given as G8 and M_V as $+1^{m}\cdot 4$. Then in 1935 it was one of 4179 stars[7]; its type had reverted to G6, its absolute magnitude was given as $+0^{m}\cdot 5$, and its 'spectroscopic parallax' was put at $0''\cdot 007$ — exactly the value found by *Hipparcos*.

The earliest classification of 60 Psc was in the *Draper Catalogue*[9], where it was noted as type H; when one has got over the anachronism, that seems a very reasonable description for a star of type G-going-on-K. Even more astute was Miss Maury's classification[10] of Group *XIVa*, the italics indicating that the type was intermediate between Groups XIV and XV, which we might now call about G5 and K0. The type star for Group *XIVa* was κ Gem, whose received type now is[11] G8 III. The *HD*[12] type of 60 Psc is G5. The first MK classification, by Nassau & van Albada[13] from low-dispersion objective-prism spectra, was G8 III, which was agreed by (A. P.) Cowley & Bidelman[14], although Harlan[15] called it K0 III.

The individual radial-velocity measurements whose mean was published by Adams[5] were listed much later by Abt[16]. They appear here at the head of Table I. Their r.m.s. deviation from their own mean is considerably *less* than that from the star's actual velocity at the relevant times as extrapolated from the now-known orbit, so they certainly could not be held to demonstrate the variability of that velocity. Since it seems that no further measurements were made for more than 70 years, the annotation 'V' (for variable) against the mean velocity in the *Bright Star Catalogue*[17] can only be seen as clairvoyant! Eventually, de Medeiros & Mayor[18] obtained two measurements with the Haute-Provence (OHP) *Coravel*, in 1986 and 1987. They gave them only as a mean, but its standard error indicated that the two values differed by some 14 km s^{-1}, clearly indicating 60 Psc to be a spectroscopic binary. They also gave a $v \sin i$ value of $1\cdot 7$ km s^{-1}. Exactly the same information as was given by de Medeiros & Mayor[18] was repeated three years later by de Medeiros, da Silva & Maia in a paper[19] in a different journal. In 2002 the former authors placed the individual measurements of the stars that featured in their paper on file with the Centre de Données Stellaires, whence the present author selected 60 Psc for observation with the Cambridge *Coravel*. Since 2002 he has obtained the 53 measurements shown in Table I. They readily yield the orbit that is portrayed in Fig. 1; its elements are presented in Table V below, together with those of the other stars discussed here. As usual in this series of papers, the OHP measurements and those cited from the literature[5] have been increased by $0\cdot 8$ km s^{-1}. The mean-square deviations of the OHP velocities from the Cambridge orbit are ten times, while those of the Mount Wilson ones are a thousand times, those of the Cambridge ones. The OHP measures could be brought right into line if it were accepted that the Cambridge zero-point should be adjusted by $-0\cdot 5$ km s^{-1}.

TABLE I

Radial-velocity observations of 60 Piscium

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

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O – C)* *km s*$^{-1}$
1913 Nov. 8·32*	20079·32	+15·1	$\overline{20}$·907	−4·5
Dec. 18·19*	119·19	12·6	·931	−7·6
1914 Jan. 7·14*	20139·14	19·1	$\overline{20}$·943	−1·8
1986 Oct. 8·96†	46711·96	20·1	$\overline{4}$·928	0·0
1987 Aug. 14·02†	47021·02	34·2	$\overline{3}$·114	−0·2
2002 Aug. 2·12	52488·12	28·3	0·403	+0·1
Sept. 2·11	519·11	27·9	·422	+0·1
28·05	545·05	27·5	·437	0·0
Oct. 27·98	574·98	27·2	·455	+0·1
2003 Jan. 11·83	52650·83	26·7	0·501	+0·4
17·83	656·83	26·0	·505	−0·2
Aug. 3·13	854·13	24·1	·623	+0·1
Oct. 12·06	924·06	23·0	·665	−0·2
Dec. 7·92	980·92	22·6	·700	0·0
2004 Jan. 2·83	53006·83	22·0	0·715	−0·3
Aug. 7·13	224·13	20·0	·846	+0·2
Sept. 5·08	253·08	19·8	·863	+0·2
Oct. 7·06	285·06	19·2	·883	−0·3
Nov. 12·95	321·95	19·5	·905	−0·1
Dec. 17·84	356·84	20·1	·926	+0·1
2005 Jan. 8·80	53378·80	20·7	0·939	+0·1
July 18·11	569·11	33·7	1·054	0·0
Aug. 13·14	595·14	34·4	·069	+0·1
Sept. 8·08	621·08	34·7	·085	+0·1
Oct. 5·07	648·07	34·5	·101	−0·1
Nov. 5·04	679·04	34·1	·120	−0·2
Dec. 8·90	712·90	34·1	·140	+0·1
2006 Jan. 4·75	53739·75	33·9	1·156	+0·3
July 15·12	931·12	30·9	·271	0·0
Aug. 11·16	958·16	30·7	·288	+0·2
Oct. 3·11	54011·11	29·4	·319	−0·5
Dec. 3·91	072·91	29·0	·357	−0·1
2007 Aug. 30·11	54342·11	26·2	1·519	+0·2
Oct. 5·02	378·02	25·4	·540	−0·2
Nov. 8·96	412·96	25·3	·561	+0·1
Dec. 16·80	450·80	24·9	·584	+0·1
2008 Jan. 7·87	54472·87	24·6	1·597	+0·1
Aug. 15·14	693·14	21·9	·730	−0·1
Oct. 2·03	741·03	21·4	·759	0·0
Nov. 7·94	777·94	20·9	·781	−0·1
Dec. 6·90	806·90	20·7	·798	0·0
2009 Jan. 2·83	54833·83	20·3	1·814	−0·1
July 30·15	55042·15	20·8	·940	+0·1
Aug. 16·14	059·14	21·4	·950	0·0
30·13	073·13	22·1	·958	0·0
Sept. 13·10	087·10	+23·0	·967	0·0

TABLE I (*concluded*)

Date (UT)		MJD	Velocity km s⁻¹	Phase	(O–C) km s⁻¹
2009 Oct.	9·04	55113·04	+25·0	1·982	0·0
	25·00	129·00	26·6	·992	+0·1
Nov.	7·95	142·95	27·6	2·000	–0·2
	17·89	152·89	28·6	·006	–0·1
Dec.	6·84	171·84	30·2	·018	–0·2
	20·88	185·88	31·6	·026	+0·1
2010 Jan.	1·82	55197·82	32·5	2·033	+0·3
Sept.	1·13	440·13	33·0	·179	–0·1
	15·06	454·06	32·9	·187	0·0
Oct.	7·02	476·02	32·6	·201	0·0
	27·97	496·97	32·3	·213	0·0
Nov.	26·96	526·96	+31·6	·231	–0·2

* Mount Wilson observation[5,16], weight 0.
† OHP observation (from CDS — see text), weight 1.

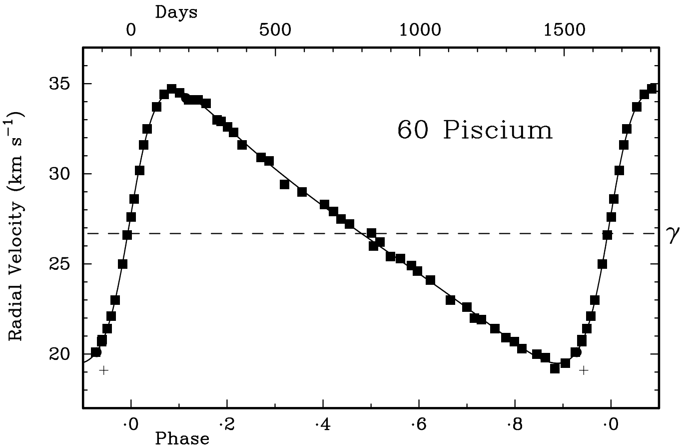

FIG. 1

The observed radial velocities of 60 Psc plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The great majority of the measurements, plotted as filled squares, were made with the Cambridge *Coravel*. Two semi-published OHP velocities are shown as filled circles. The only one of the three Mount Wilson[5,16] velocities that falls within the confines of the diagram is represented by a plus.

It has often been found that a negative correction is an improvement for stars other than late-K types, but not one quite as great as 0·5 km s^{-1} for a star with the colour index of 60 Psc. By analogy with 27 Ari, which has a similar colour, a correction of –0·4 km s^{-1} has been made to the Cambridge data.

Very little other interest has been shown in 60 Psc. Its metal abundances have been found just slightly sub-solar by McWilliam[20] and Clariá, Piatti & Lapasset[21]. Henry *et al.*[22] checked it for photometric variability and found none; it was then used, with apparent satisfaction, as the check star in photometric campaigns on HD 2842[23] and HD 4313[24]. The *Hipparcos* parallax solution is an 'acceleration' one, indicating that the satellite did notice the duplicity of the star. Mason *et al.*[25] accordingly tried to resolve it by speckle interferometry, but did not succeed. The mass function of 0·05 $M_\odot$ demands for the secondary a mass of no more than 0·7 $M_\odot$, corresponding to that of a late-K dwarf, if the mass of the primary is arbitrarily taken as 2 $M_\odot$, so if $\sin i \sim 1$ there could be a Δm of about 7^{m} between the components. In that case the mass ratio would be about 3:1, so the semi-major axis of the relative orbit would be about four times the $a_1 \sin i$ value shown in Table V, *i.e.*, 4 AU, so (since $\omega \sim 270°$, so the orbit is viewed 'side-on' with its major axis more or less in the 'plane of the sky') the angular separation at apastron might be up to $4(1+e)$ times the parallax, or about 0″·04.

27 Arietis (HR 731, HD 15596)

Like 60 Psc, 27 Ari is a 6^{m} object in a visually rather empty area of sky; in this case it is about 8° south-following Hamal (α Ari). It must be said at once that its principal distinguishing characteristic is its apparent membership in an old stellar population whose members have high space motions and low abundances of metallic elements in their atmospheres. The space motion of 27 Ari is, rather unusually, dominated by its radial velocity of nearly −120 km s^{-1}; the annual proper motion is less than 0″·1, which at the 100-pc distance of the star represents a transverse motion of less than 50 km s^{-1}.

The broad-band magnitude and colours of 27 Ari have been measured several times[3,4,26–29], with accordant results close to $V = 6^{\mathrm{m}}\!\cdot\!23$, $(B-V) = 0^{\mathrm{m}}\!\cdot\!90$, $(U-B) = 0^{\mathrm{m}}\!\cdot\!54$. The spectral type appears as G5 in the *Henry Draper Catalogue*, and has been given as G5 III–IV by Keenan & Keller[30] and K0 III by Miss Roman[26] (in both cases in papers concerned with 'high-velocity' stars), and as G8 IV by Harlan[31]. Chopinet[32] reviewed the strengths of atomic lines, the G band of CH, and the λ4200-Å CN band in the spectra of a number of late-type high-velocity stars. Adopting the Keenan & Keller type of G5 III–IV, she compared 27 Ari with HR 1327 (G5 III) and found that the atomic lines and G band were normal but CN was weak. Later, she considered that the K0 III classification of Miss Roman was more correct, especially in the light of Keenan & Keller's own conclusion that high-velocity stars exhibited a "general weakening of the lines of hydrogen and the metals in spectra of types F5–G5, and the marked weakening of the bands of CN in types G8–K3". She presented[33] a revised set of conclusions, which in the case of 27 Ari were that the metal lines were slightly weak, the G band slightly strong, and the CN ('CH' at the head of the column in her table is an obvious misprint) very weak. The extraordinary weakness of CN in 27 Ari is confirmed by the writer's own quantitative measurements[34] made more than fifty years ago, in which 27 Ari proved to have a 'CN ratio' of only 2·01, implying no excess absorption in the CN region at all; it was one of the lowest ratios obtained in the whole programme of more than 700 stars, apart from some dwarfs and a few stars of particularly early (F) types. The *Hipparcos* parallax of just over 0″·01 corresponds to a distance modulus of $4^{\mathrm{m}}\!\cdot\!91 \pm 0^{\mathrm{m}}\!\cdot\!18$ and so to an absolute magnitude of $+1^{\mathrm{m}}\!\cdot\!32$, with the same uncertainty, so the early Keenan & Keller[30] luminosity class of III–IV does it reasonable justice. Those authors themselves gave the spectroscopic parallax as 0″·012. The absolute magnitude derived by Wilson[35] for 27 Ari from the width of the *K*-line emission was abnormally far from the mark, at $+2^{\mathrm{m}}\!\cdot\!7$.

Several investigations[36–40] of stellar chemical abundances have featured 27 Ari among the stars they treated; those conducted photometrically, as well as the actual spectroscopic measurements, have been unusually accordant in finding [Fe/H] near –0·7. In several cases, other elements have been found to be somewhat less under-abundant than iron. Cottrell & Sneden[39] deduced that the mass of 27 Ari is only 0·2 $M_\odot$; Shetrone, Sneden & Pilachowski[41] (an overlapping syndicate, we note) gave it as 0·5 $M_\odot$. There has not been time since the beginning thereof for stars of such small masses to reach the evolutionary stage that 27 Ari appears to be in, so the only possibility that is consonant with those masses is that most of the star's mass must somehow have been shed. That would scarcely happen otherwise than during evolution to the top of the giant branch of the H–R Diagram, *i.e.*, comparatively recently. There does not, however, appear to be any evidence at present of circumstellar absorption or any other indicators of a local envelope of material lost by the star. Shetrone *et al.*[41] found 27 Ari to exhibit a $^{12}C/^{13}C$ ratio of 20, confirming that the star has passed the rather late evolutionary stage at which products of nuclear burning in the core have been 'dredged up' to the surface.

The radial velocity of 27 Ari was first measured by Adams *et al.*[42] at Mount Wilson. From three plates taken in the 1925/6 season they obtained a mean of –116·0 km s^{-1} with a 'probable error' of 2·8 km s^{-1}. The three velocities, as can be seen from their individual values which were published much later by Abt[16], did not agree very well together, showing a range of nearly 13 km s^{-1}, but that was not enough for the authors to suggest variability. All the same, the r.m.s. deviation of the three observations from the orbit presented below is reduced to 3·5 km s^{-1} from the 6·5 km s^{-1} that is the r.m.s. error computed from their own mean. In 1961 Woolley & Harding[43] took four spectra with the coudé spectrograph of the Mount Wilson 100-inch telescope. Three were taken on almost consecutive nights and the fourth a month later. That last observation disagreed with the other three by about 7 km s^{-1} — an unacceptable discrepancy for spectra of such a quality — and prompted the conservative note, "Probably variable". Beavers & Eitter[44] obtained no fewer than 17 velocities of 27 Ari in 1978–83 with their photoelectric spectrometer[45] at the *Mather* 24-inch reflector of the Fick Observatory at Iowa State University. The measurements have a range of more than 13 km s^{-1} and definitely show the velocity to be variable, although at a 1984 conference Beavers[46] included 27 Ari in a listing of stars "with no confirmed velocity variability greater than about ± 2 km s^{-1}". They are, however, unusually ragged for Fick velocities, and their poor phase distribution, that can now be seen retrospectively, would prevent them from supporting an orbit determination. De Medeiros & Mayor[18] reported two measurements made with the OHP *Coravel*, and subsequently made the individual details available through the Centre de Données Stellaires; they also gave a $v \sin i$ value of 1·6 km s^{-1}. Just as in the case of 60 Psc, de Medeiros, da Silva & Maia repeated exactly the same information in another paper[19]. Three other velocities have been retrieved from the *Elodie* archive maintained on the web by the Observatoire de Genève.

What prompted the writer's interest in 27 Ari was finding, when he was concerned with the composite-spectrum binary 93 Leo[47], that in a previous investigation of that system Batten *et al.*[48] had used 27 Ari as a surrogate for the ~G7 III primary; it seemed hard to believe that the spectrum of a star with such a high radial velocity would be a good match to that of a star of the local low-velocity population. Attention was thereby drawn to the evidence for duplicity of 27 Ari; the star was placed on the Cambridge radial-velocity observing programme in 2003 and has been measured 53 times. All the observations, including those found in the literature, have been entered in Table II.

TABLE II

Radial-velocity observations of 27 Arietis

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

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
1925 Oct. 30·39★	24453·39	−121·6	$\overline{218}$·636	−1·7
Nov. 25·36★	479·36	−108·9	·835	+5·9
1926 Jan. 6·19★	24521·19	−117·5	$\overline{217}$·155	+1·4
1961 Oct. 1·33†	37573·33	−113·6	$\overline{117}$·053	−0·3
3·43†	575·43	−115·4	·069	−1·0
4·43†	576·43	−113·7	·077	+1·2
Nov. 3·26†	606·26	−122·6	·305	−0·9
1978 Jan. 11·08‡	43519·08	−124·6:	$\overline{72}$·560	−3·7
25·03‡	533·03	−121·0	·667	−1·6
29·09‡	537·09	−122·6:	·698	−3·8
Oct. 23·35‡	804·35	−122·0	$\overline{70}$·744	−4·2
1979 Nov. 14·26‡	44191·26	−119·6:	$\overline{67}$·705	−0·9
15·24‡	192·24	−115·2:	·712	+3·3
1980 Oct. 5·29‡	44517·29	−119·4	$\overline{64}$·200	+0·8
Nov. 25·19‡	568·19	−119·4	·590	+1·2
Dec. 19·15‡	592·15	−117·5	·773	−0·6
1982 Jan. 8·10‡	44977·10	−117·8	$\overline{61}$·719	+0·6
14·11‡	983·11	−116·3	·765	+0·9
Feb. 6·07‡	45006·07	−109·6:	·941	+0·8
Nov. 18·20‡	291·20	−116·4:	$\overline{58}$·124	+1·2
Dec. 17·13‡	320·13	−123·1	·345	−1·3
1983 Sept. 23·37‡	45600·37	−118·8	$\overline{56}$·490	+2·7
Oct. 28·27‡	635·27	−115·5:	·757	+1·9
Nov. 18·24‡	656·24	−111·3	·917	0·0
1986 Nov. 18·83§	46752·83	−121·5	$\overline{47}$·310	+0·2
1987 Sept. 16·11§	47054·11	−119·8	$\overline{45}$·616	+0·4
1996 Oct. 7·10¶	50363·10	−110·4	$\overline{20}$·943	0·0
2000 Jan. 21·75¶	51564·75	−118·3	$\overline{10}$·140	0·0
2003 Sept. 23·07	52905·07	−121·8	0·398	+0·1
2004 Feb. 3·81¶	53038·81	−121·9	1·422	−0·1
7·74	042·74	−121·7	·452	0·0
Mar. 1·80	065·80	−120·8	·628	−0·7
Sept. 1·15	249·15	−111·8	3·032	+0·1
Oct. 7·09	285·09	−121·4	·307	+0·3
19·08	297·08	−121·6	·398	+0·3
Nov. 13·03	322·03	−120·6	·589	0·0
Dec. 17·92	356·92	−113·8	·856	+0·1
19·97	358·97	−113·4	·872	−0·1
20·91	359·91	−113·0	·879	0·0
26·87	365·87	−111·0	·925	0·0
31·90	370·90	−110·1	·963	−0·1

TABLE II (*concluded*)

Date (UT)			*MJD*	*Velocity* km s⁻¹	*Phase*	*(O–C)* km s⁻¹
2005	Jan.	4·90	53374·90	−110·4	3·994	−0·2
		8·86	378·86	−111·8	4·024	−0·3
		12·89	382·89	−113·7	·055	−0·3
		14·82	384·82	−114·5	·070	−0·1
		16·79	386·79	−115·4	·085	0·0
	Feb.	8·81	409·81	−121·6	·261	−0·3
		15·77	416·77	−121·4	·315	+0·3
	Aug.	11·15	593·15	−119·1	5·664	+0·4
	Sept.	17·13	630·13	−110·2	·948	+0·1
		29·09	642·09	−112·2	6·039	+0·2
	Oct.	10·16	653·16	−117·5	·124	+0·1
		26·04	669·04	−120·8	·245	+0·3
	Nov.	9·97	683·97	−121·8	·360	+0·1
		29·95	703·95	−121·2	·513	+0·1
	Dec.	16·96	720·96	−119·9	·643	−0·1
		26·92	730·92	−118·6	·719	−0·2
2006	Jan.	4·81	53739·81	−116·5	6·787	0·0
		28·82	763·82	−110·0	·971	−0·1
	Feb.	25·80	791·80	−120·1	7·185	−0·3
	Sept.	20·12	998·12	−117·0	8·764	+0·2
		30·14	54008·14	−114·5	·841	+0·1
	Oct.	27·03	035·03	−112·4	9·047	+0·5
	Dec.	8·98	077·98	−122·0	·375	−0·1
2007	Jan.	1·92	54101·92	−121·0	9·558	−0·1
		25·89	125·89	−118·3	·742	−0·5
	Feb.	6·80	137·80	−114·8	·833	+0·1
		14·80	145·80	−112·3	·894	0·0
	Nov.	11·96	415·96	−110·0	11·962	0·0
	Dec.	5·95	439·95	−118·5	12·146	0·0
		7·96	441·96	−119·3	·161	−0·2
2008	Sept.	28·07	54737·07	−121·7	14·420	+0·1
	Oct.	13·06	752·06	−120·9	·534	+0·3
	Dec.	26·97	826·97	−117·0	15·108	−0·2
2009	Jan.	18·90	54849·90	−121·7	15·283	−0·2
	Aug.	30·14	55073·14	−109·9	16·992	+0·2
	Sept.	10·17	084·17	−114·7	17·076	+0·2
	Dec.	6·94	171·94	−117·5	·748	+0·1
2010	Sept.	1·18	55440·18	−115·9	19·801	+0·2
		15·12	454·12	−111·6	·908	+0·1
	Nov.	10·99	510·99	−121·9	20·343	−0·1
		27·03	527·03	−121·9	·466	−0·3

* Mount Wilson observation[5,16], weight 0.
† Mt. Wilson coudé photographic observation[43], wt. 1/16.
‡ Ames photoelectric observation[44], wt. 0·02 (but see text).
§ OHP observation (from CDS — see text), weight ½.
¶ Retrieved from *Elodie* archive, weight 1.

The observations from the OHP *Coravel* and *Elodie* have had 0·8 km s⁻¹ added to them before being entered in Table II; the Ames data, which often have proved to need a similar adjustment to agree with the Cambridge ones, have in this case been increased by 1·5 km s⁻¹, and the Mount Wilson coudé velocities have had 2 km s⁻¹ added. The Cambridge data themselves have been 'corrected' by –0·4 km s^{-1} from the 'as initially reduced' velocities, a not unusual quantity for a star having the colour of 27 Ari. In the solution of the orbit, the Cambridge and *Elodie* velocities have been given full weight, the OHP *Coravel* ones half, the Mount Wilson coudé photographic velocities 1/16, and the Ames ones 1/50. (Seven of the seventeen were noted as 'quality B' by their authors and their weighting has been further halved; they are identified in Table II by colons after the velocities.) The early Mount Wilson measures have not been included in the solution. With those preliminaries settled, the orbit readily follows; it is illustrated in Fig. 2, and its elements are included in Table V below. Largely because of the high γ-velocity of the system, the 'true' period, in the 27 Ari rest-frame, is significantly longer (by more than six standard deviations) than the observed one.

The mass function is very small; at best it is a blunt instrument for deducing anything about the secondary star, and in this case the uncertainty is compounded by that of the mass of the primary. If the primary is really only 0·2 $M_{\odot}$ then the secondary does not have to be more than 0·05 $M_{\odot}$ — well down into the brown-dwarf range. But there is nothing, apart from its not having been detected, to prevent its being more massive than that to almost any extent, although it would be surprising if it were more massive than the primary but nevertheless undetected. The $a_1 \sin i$ value from the orbital elements is well under a tenth of an AU, so it would subtend less than a single millisecond of arc at the ~100-pc distance of 27 Ari. But that amount would be increased by the secant of the inclination, which is unknown, and the actual separation of the components would be also be larger by the factor $(1 + q)$, where q is the unknown mass ratio. We might recall in this context a report[49] in this *Magazine* by an experienced observer of lunar occultations, to the effect that on one occasion in 1899 the reappearance from occultation of 27 Ari at the dark limb

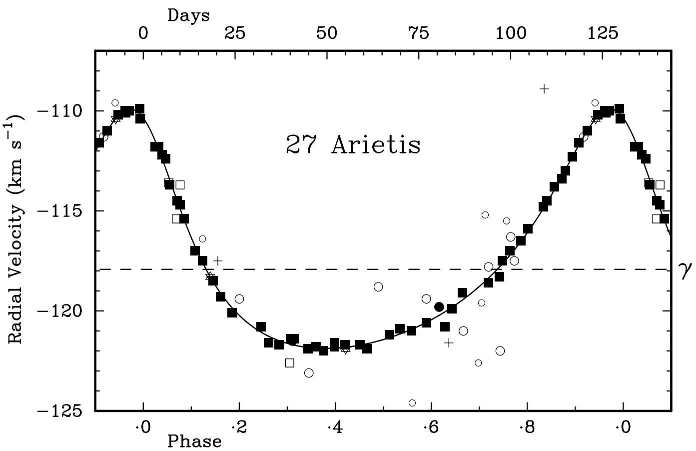

FIG. 2

As Fig. 1, but for 27 Ari. Cambridge velocities are again represented by filled squares, OHP ones by filled circles, and Mount Wilson[42,16] ones by plusses. Here there are, in addition, four velocities[43] from the Mount Wilson 100-inch coudé, plotted as open squares, seventeen from Ames[44], plotted as open circles in two different sizes corresponding to the different qualities of the respective observations, and three from the online *Elodie* archive, shown as open stars.

of the Moon appeared to be "hardly instantaneous", the implication being that it was a very close double star with components of not enormously dissimilar magnitudes.

EZ Ursae Majoris (*HR 3722, HD 80953*)

EZ UMa is a 6^{m} star in the north-preceding corner of Ursa Major; it is about 1° north-preceding the $3^{\mathrm{m}}\cdot 7$ star 23 UMa, whose lack of a Greek-letter designation is surprising, since τ UMa, only 3° preceding, is a magnitude fainter. (The star 23 UMa is, however, also designated h UMa.) Rybka[50] gave EZ's magnitude and colours (revised from previous publications[51,52]) as $V = 6^{\mathrm{m}}\cdot 27$, $(B-V) = 1^{\mathrm{m}}\cdot 47$, $(U-B) = 1^{\mathrm{m}}\cdot 74$; Elvius & Häggkvist[53] gave almost the same values for V and $(B-V)$. In a second paper, which reported that it took into account photometry of EZ UMa that it ascribed to Ljunggren & Oja, whose paper, however, does not include the star, Rybka[54] gave the V magnitude as $6^{\mathrm{m}}\cdot 23$. That the discrepancy could be understood as arising from real variation was demonstrated by Henry, Fekel & Hall[55] in 1995. They stumbled upon the variability, and on the nature of EZ UMa as a chromospherically active star, when they adopted it as the photometric comparison star in an investigation of FF UMa, itself an active-chromosphere object whose orbital period was at one time asserted[56] to vary but has recently been shown by the present writer[57] not to do so. Henry *et al.* found EZ UMa to vary by $0^{\mathrm{m}}\cdot 03$ in B, in a period of 97 ± 3 days, and attributed the variation to star-spots. Their findings led to the star being gazetted[58] as a variable star under the designation by which it is being identified in this present paper. The 97-day photometric period is not far from half the 184 days of the orbital period derived below, so it seems likely that in the relevant observing season the star-spots were fortuitously concentrated in two azimuths differing in stellar longitude by nearly 180°. *Hipparcos* did notice that the star's magnitude was not quite constant, but did not assign any period to its variation.

The star was observed for radial velocity with the 74-inch reflector and one-prism spectrograph at the David Dunlap Observatory, probably at a reciprocal dispersion of 33 Å mm^{-1} at Hγ, four times in the early 1940s. The result was given[59] as a mean of +8·1 km s^{-1} with a 'probable error' of 1·5 km s^{-1}; the observations were not listed individually, and variability was not claimed, but the object was flagged as having discordances between the four measurements "somewhat greater than would be expected from the character of the lines", and the total range was noted as being 10 km s^{-1}. The spectral type was put at K3; the *HD* type was K2, and it seems only too likely that the *Bright Star Catalogue* lists the type as K2 III on the basis of that classification plus evidence from photometry that it is not a main-sequence object (the $(U-B)$ colour index is far too red). Henry *et al.*[55] obtained a spectrum, which they classified as K4 III, in fair agreement with the colour indices; the radial velocity, at +12·2 km s^{-1}, was in sufficient agreement with the DDO value for them to enter "const:" in their table of results, in a column giving their conclusions regarding velocity variability. They gave the rotational velocity as 4 ± 2 km s^{-1}, and then, on the basis that the 97-day photometric period represented the period of axial rotation, inferred a minimum stellar radius of $7\cdot 7\ R_{\odot}$. Fekel[60] (a member of the Henry *et al.* syndicate) subsequently listed the same tentative conclusion of constant radial velocity, and the same $v \sin i$ (but made to look much more accurate, as 4·0 km s^{-1}). In fact the uncertainty in the underlying rotational velocity, to say nothing of the assumption about the period of rotation, meant that the radius was so uncertain as hardly to be worth listing.

De Medeiros & Mayor[18] published a mean of two radial-velocity measurements, of +11·57 ± 3·13 km s^{-1}; since there were just two observations, the selfsame expression ought to give the two velocities individually, and it obviously implies variation. The projected rotational velocity was also listed, as 1·2 ± 1·3 km s^{-1}. As in the cases of the two stars already treated above, exactly the same information was repeated, in a different journal, by de Medeiros, da Silva & Maia[19]. When, in 2002, de Medeiros & Mayor lodged with the CDS the individual velocities that in their paper[18] they had given only as means, they were not quite the values expected from their paper, the mean being +12·00 km s^{-1}, but the difference between them was the expected one. The writer then made one observation of the star himself, obtaining a radial velocity that disagreed with both those of de Medeiros & Mayor, but it was not until 2005 that he began a systematic study of it. He has now accumulated a total of 53 radial velocities (coincidentally the same number as for 60 Psc and 27 Ari); they are set out in Table III, which also includes the two retrieved through the CDS, but not those obtained at the DDO[59] or by Henry *et al.*[55], whose dates are not available. The orbit obtained from the data in Table III has elements that have been added to Table V below, and it is plotted in Fig. 3.

The period is seen to be very close to six months, and is almost twice the photometric period that was found by Henry *et al.*[55]. The orbital eccentricity, being more than twelve times its own standard error, is certainly non-zero. An idiosyncrasy seen in Table III is an apparent non-randomness of the velocity residuals, particularly the fact that all eight of them in the first well-observed season have residuals of the same sign, which would happen by chance only about one time in 2^7 (128) trials. There is no clear long-term trend, but if the residuals are plotted directly against time (Fig. 4) they certainly do not appear to be randomly distributed, but seem to show a systematic variation in which a periodicity of about four years might perhaps be divined. No suggestion can be offered here as to the origin of any such variation. The star is a giant of quite a late type, and the dips that it gives on radial-velocity traces are among the strongest (equivalent width about 7 km s^{-1}) seen in any but supergiant stars; its apparent radial velocity may well be subject to an intrinsic 'jitter' for whose cause one might need to look no further than the star-spots that are presumably responsible for its photometric variations, but such a jitter should have a period equal to the photometric period or to the star's period of rotation which may be about double that.

The mean value of the projected rotational velocity, measured from the author's 53 radial-velocity traces, is 3·70 km s^{-1}, with a formal standard deviation of only 0·13 km s^{-1}, but in view of the rather cavalier assumptions that lie behind the determination, notably that 'turbulence' has the same effect in broadening line-profiles in all stars, such determinations are never claimed to be more accurate than ±1 km s^{-1}. The stellar radius that would correspond to a circumference that is traversed in the same period as the orbit (184 days) at 3·7 ± 1 km s^{-1} is about 13 ± 4 $R_\odot$, which would be reasonable as a minimum value for a K4 III star but lacks the precision needed to make it very informative — it is calculated here only to set against the value suggested by Henry *et al.*[55]. If the star's rotation is *pseudo*-synchronized, it would take about 174 days, making it more difficult to reconcile with the 97-day photometric period, and decreasing the minimum radius by about 6%. The parallax, very close to 4 arc-milliseconds, corresponds to a distance modulus of 7 magnitudes and therefore shows EZ UMa to have an absolute magnitude approaching -1^{m} — right on the ridge-line of luminosity class III according to Keenan's post-*Hipparcos* H–R

TABLE III

Radial-velocity observations of EZ UMa

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

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O–C)* *km s*$^{-1}$
1988 Mar. 8·00*	47228·00	+9·7	$\overline{29}$·921	+0·1
Nov. 28·12*	493·12	15·9	$\overline{27}$·360	+0·1
2002 May 30·89	52424·89	19·2	0·125	−0·9
2005 Mar. 23·11	53452·11	3·4	5·700	−0·7
Apr. 4·96	464·96	3·9	·770	−0·2
May 7·87	497·87	11·2	·948	−0·1
8·90	498·90	11·2	·954	−0·5
13·93	503·93	13·3	·981	−0·1
21·91	511·91	15·9	6·025	−0·2
27·89	517·89	17·4	·057	−0·4
June 9·93	530·93	19·8	·128	−0·3
Nov. 4·26	678·26	10·4	·927	+0·4
14·17	688·17	13·7	·981	+0·3
19·19	693·19	15·4	7·008	+0·3
25·19	699·19	17·0	·041	0·0
30·19	704·19	18·4	·068	+0·1
Dec. 15·15	719·15	20·8	·149	+0·4
2006 Jan. 26·02	53761·02	14·9	7·376	−0·2
29·13	764·13	14·6	·393	+0·2
Feb. 16·00	782·00	10·4	·490	+0·2
21·05	787·05	9·0	·518	−0·1
Mar. 1·03	795·03	7·6	·561	+0·1
22·91	816·91	4·7	·680	+0·4
Apr. 4·01	829·01	3·9	·745	0·0
10·95	835·95	4·4	·783	+0·2
25·93	850·93	6·6	·864	−0·1
May 1·97	856·97	8·0	·897	−0·3
Oct. 27·24	54035·24	7·3	8·865	+0·6
Nov. 1·25	040·25	8·6	·892	+0·6
2007 Jan. 2·15	54102·15	20·1	9·228	+0·1
14·02	114·02	18·7	·292	+0·4
23·08	123·08	16·5	·341	−0·1
Feb. 7·03	138·03	12·9	·423	−0·2
15·04	146·04	11·3	·466	0·0
Mar. 21·95	180·95	4·8	·656	0·0
Oct. 19·20	392·20	5·2	10·802	+0·6
Nov. 24·16	428·16	14·7	·997	+0·3
Dec. 11·19	445·19	19·4	11·090	+0·3
2008 Feb. 9·05	54505·05	13·7	11·414	+0·2
25·03	521·03	9·8	·501	0·0
2009 Jan. 14·13	54845·13	19·6	13·260	+0·3
24·16	855·16	17·1	·315	−0·5
Mar. 5·02	895·02	8·4	·531	−0·2
20·97	910·97	5·6	·617	−0·1
Apr. 29·88	950·88	5·0	·834	−0·5
May 23·89	974·89	11·7	·964	−0·6
Dec. 21·21	55186·21	+19·8	15·111	0·0

TABLE III (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2010 Feb. 21·03	55248·03	+12·0	15·447	−0·1
Mar. 22·96	277·96	6·1	·609	+0·2
Apr. 12·91	298·91	4·1	·723	+0·2
May 11·88	327·88	6·7	·880	−0·7
Nov. 28·25	528·25	12·7	16·968	+0·2
Dec. 9·20	539·20	16·8	17·027	+0·6
2011 Jan. 10·14	55571·14	20·6	17·200	+0·2
May 9·92	690·92	+5·7	·850	−0·4

*OHP observation (from CDS — see text), weight 1.

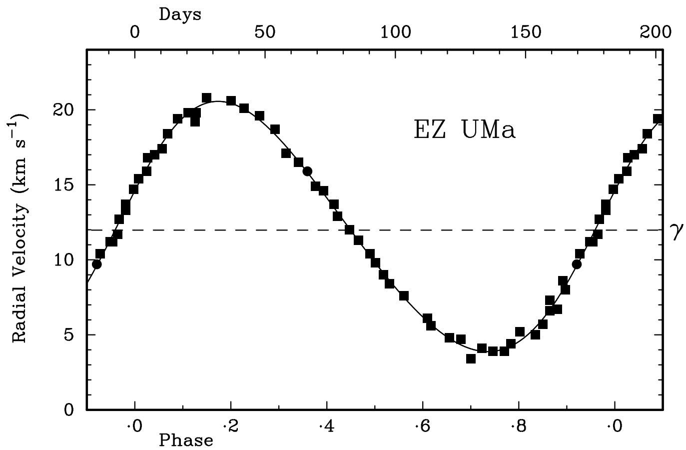

FIG. 3
As Fig. 1, but for EZ UMa.

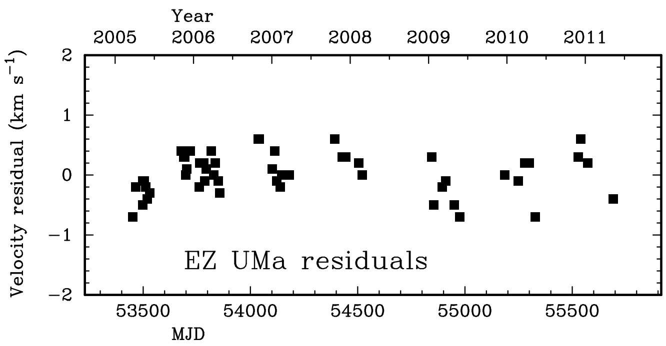

FIG. 4
Residuals of Cambridge velocities of EZ UMa, plotted directly against time.

diagram[61]. The writer believes that the corresponding radius for a star having the type and colour of EZ UMa is considerably greater than the minimum that has just been estimated from the rotational velocity, implying that the axial inclination is far short of 90°.

The mass function given by the orbital elements is small, and does not require the mass of the secondary to be greater than about 0·4 $M_{\odot}$ if the primary mass is arbitrarily supposed to be 2 $M_{\odot}$, but the likelihood that the factor $\sin^3 i$ in the mass function is markedly different from unity means that we cannot usefully assign any particular mass to the companion star; all that can be said is that no secondary dip has been noticed in the radial-velocity traces. The calculated separation of the components of the binary is not strongly dependent on the value of $\sin i$ but remains near 100 Gm or about two-thirds of an astronomical unit, so the anticipated maximum angular separation of the pair on the sky is of the order of two-thirds of the parallax — less than 0″·003 — so the system is beyond resolution by speckle interferometry, although it should be readily accessible to interferometers with separated apertures if the magnitude difference is not too great.

4 Equulei (*HR 8077, HD 200790*)

4 Equ, another 6^{m} star, is to be found about 3° preceding α Equ, a well-known composite-spectrum binary system[62] that is the only star as bright as the fourth magnitude in the constellation. The *IDS* [63] lists 4 Equ as a double star on the strength of a measurement by Burnham[64] of a 12^{m} object about 35″ away. A rough measurement by the writer on a comparatively recent picture brought up by ALADIN through *Vizier* gave the distance and position angle of the faint star as 30″·9, 295°, indicating over a baseline of nearly 100 years a change that almost exactly reflects the proper motion of the principal star and showing that the companion is merely optical.

Photometry of 4 Equ has been published by a number of authors[4,65–67], with accordant results of $V = 5^{m}{\cdot}94$ and $(B-V) = 0^{m}{\cdot}54$, and not-so-accordant values of $(U-B)$ that have a mean of $+0^{m}{\cdot}03$ and a range of $0^{m}{\cdot}07$. The spectral type has been given by both Harlan[68] and Cowley[69] as F8 V. The *Hipparcos* parallax[70] of 27·06 ± 1·06 arc-milliseconds puts its distance modulus at $2^{m}{\cdot}84 \pm 0^{m}{\cdot}09$; thus its absolute magnitude should be close to $+3^{m}{\cdot}1$, with the same uncertainty — almost a whole magnitude brighter than a main-sequence F8 star is supposed to be. Van Leeuwen's re-reduction[71], however, gives the parallax as 20·44 ± 1·68 milliseconds, which yields an M_V of $2^{m}{\cdot}51 \pm 0^{m}{\cdot}18$ — a magnitude and a half above the main sequence and fully worthy of luminosity class IV. It is of course a bit disconcerting for the user of astrometric material to find that a re-reduction of the same data produces a result that is more than six standard deviations away from the original one. Halliwell[72] included 4 Equ in a listing of 'possible nearby stars', with a 'photometric parallax' of 44 ± 4 arc-ms which he presumably obtained by equating its absolute magnitude to a tabular value for type F8 V.

Eggen kept asserting that 4 Equ was a member of his 'Wolf 630 group', and he repeated its photometry and other properties time and again[73–82] in papers that were ostensibly on all sorts of different subjects. The 'group parallax' that he deduced for 4 Equ remained rather constant throughout the papers and corresponded to absolute magnitudes that ranged only between extreme values of $2^{m}{\cdot}75$ and $3^{m}{\cdot}2$ and are thus compatible with the more recent estimates. An assessment by McDonald & Hearnshaw[83] of the reality of the 'Wolf 630 group', however, concluded that either the underlying observations were much less accurate than they were claimed to be, or half the stars assigned to the group were non-members, or else the 'cosmic scatter' in the group was much greater than in, for example, M 67.

The literature evinces an extraordinary interest in the abundance of light elements in 4 Equ, with many papers which we refrain from listing here since the matter is at best of oblique relevance to the orbit; the principal result seems to be that lithium is unobservably weak.

The radial velocity of 4 Equ was first measured at the Dominion Astrophysical Observatory, Victoria (DAO), under the identity[84] Boss 5428, in 1919; five velocities, having an overall range of some 6 km s^{-1}, were published from there in 1921 by Plaskett *et al.*[85]. Several small sets of measurements have since been published. One[86] that *Simbad* attributes to the Crimean Astrophysical Observatory is actually just a citation of 4 Equ's mean velocity as printed in the then-current radial-velocity catalogue[87], from which it was adopted in the Crimea as a standard. There were two measures from Mount Wilson (given first as a mean by Wilson & Joy[88] in 1950 and later individually by Abt[89]); then in 1960 two Mount Wilson coudé velocities were published by Woolley, Jones & Mather[90], and the following year two results from the Mount Wilson 60-inch Cassegrain *X spectrograph* were given (but only as mean) by Jones & Woolley[91]. The Mount Wilson velocities published from the RGO[90,91] were in quite serious disagreement (ΔV near 10 km s^{-1}) with those already in the literature; Eggen[80] remarked, concerning 4 Equ, "Both the magnitude and radial velocity of this blue straggler may be variable". Photometric variability has not been confirmed, and the characterization of the star as a 'blue straggler', when its position a little above the main sequence is probably just a result of evolution, may be equally wide of the mark, but Eggen seems to have been the first to articulate the likelihood that 4 Equ is a spectroscopic binary just from a comparison of published velocities. The star had, however, already been identified as such in 1972 by Anderson & Kraft[92], who observed a considerable number of stars of approximately solar type repeatedly with the Lick 120-inch coudé at high dispersion (5 Å mm^{-1}) on purpose to detect small-amplitude spectroscopic binaries. Among those that they identified, orbits for two (HR 2452[93] and HR 7955[94]) have already been published by the present writer.

Further confirmation of velocity variability, with the actual data given, was forthcoming in 1986, when Beavers & Eitter[44] published 17 observations of 4 Equ, showing a total range of well over 20 km s^{-1}. Two years later, they[95] reported in a conference abstract that they had obtained orbits for 18 stars, of which 4 Equ was one, but the orbital elements were not given and do not appear ever to have been published. Nordström *et al.*[96] reported the existence of 14 radial velocities of 4 Equ in the *Coravel* data base in Geneva; it is likely that all but one are the present writer's own observations, made with the Haute-Provence (OHP) (or in one case the ESO) *Coravel* and entered in Table IV here. That table includes the published observations insofar as their individual dates and results are available, and in addition the 61 measurements made by the writer with his own *Coravel* instrument in Cambridge as well as three that he made at the DAO in 1999. There are 103 observations in total. In an effort to maintain the zero-point normally adopted in this series of papers, all the published observations have been adjusted by +0·8 km s^{-1} before being entered in Table IV, and the Cambridge data have been corrected by –0·5 km s^{-1} from the 'as initially reduced' values.

TABLE IV

Radial-velocity observations of 4 Equulei

Except as noted, the sources of the observations are as follows:
1993–1998 — OHP Coravel (weight ¼); 1999–2010 — Cambridge Coravel (weight 1)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O–C) km s⁻¹*
1919 Nov. 8·12★	22270·12	−23·9	$\overline{14}$·464	+1·4
1920 Aug. 5·35★	22541·35	−22·2	$\overline{14}$·602	+1·6
Dec. 5·05★	663·05	−26·0	·663	−3·5
14·05★	672·05	−19·6	·668	+2·8
1921 July 10·35★	22880·35	−21·1	$\overline{14}$·773	−2·5
1930 Aug. 11·34†	26199·34	−30·1	$\overline{12}$·452	−4·7
1948 Sept. 20·20†	32814·20	−22·8	$\overline{9}$·799	−5·6
1959 Oct. 8·19‡	36849·19	−14·6	$\overline{7}$·840	0·0
12·25‡	853·25	−14·1	·842	+0·4
1976 Aug. 3·35§	42993·35	−7·3:	$\overline{4}$·949	−1·7
1977 Sept. 26·11§	43412·11	−15·1:	$\overline{3}$·161	+4·6
Oct. 4·15§	420·15	−16·5:	·165	+3·5
1978 Aug. 5·29§	43725·29	−22·5:	$\overline{3}$·319	+2·3
14·24§	734·24	−22·1::	·324	+2·8
23·22§	743·22	−28·1:	·328	−3·2
Nov. 8·09§	820·09	−25·2	·367	+0·1
Dec. 13·99§	855·99	−29·3:	·385	−3·9
1980 Aug. 22·23§	44473·23	−22·1	$\overline{3}$·698	−0·6
Sept. 7·19§	489·19	−20·6	·706	+0·7
1981 Sept. 20·14§	44867·14	−10·1:	$\overline{3}$·897	−0·1
1982 July 13·37§	45163·37	−6·1::	$\overline{2}$·047	+2·6
25·31§	175·31	−7·6	·053	+1·8
Aug. 18·24§	199·24	−11·7:	·065	−0·9
1983 July 14·33§	45529·33	−22·4:	$\overline{2}$·232	+0·6
Aug. 25·21§	571·21	−24·2:	·253	−0·6
26·21§	572·21	−23·0::	·254	+0·6
1993 July 9·07	49177·07	−12·0	0·078	+0·3
Sept. 11·97	241·97	−15·7	·110	+0·1
Nov. 5·02¶	296·02	−17·8	·138	+0·3
1994 Jan. 1·75	49353·75	−21·0	0·167	−0·9
Aug. 2·03	566·03	−24·1	·274	0·0
Dec. 10·75	696·75	−25·1	·341	0·0
1995 Jan. 1·76	49718·76	−25·0	0·352	+0·2
June 3·08	871·08	−26·0	·429	−0·6
Dec. 31·77	50082·77	−25·5	·536	−0·7
1996 Nov. 15·76	50402·76	−21·4	0·698	+0·1
Dec. 15·74	432·74	−20·9	·713	+0·1
1997 July 19·02	50648·02	−16·3	0·822	−0·4
Dec. 21·80	803·80	−9·3	·901	+0·4

TABLE IV (*continued*)

Date (UT)			*MJD*	*Velocity* $km\ s^{-1}$	*Phase*	$(O-C)$ $km\ s^{-1}$
1998	July	9·03	51003·03	−5·5	2·001	−0·8
1999	Apr.	16·53‖	51284·53	−18·9	1·144	−0·3
	July	9·45‖	368·45	−21·0	·186	+0·1
	Nov.	6·23‖	488·23	−23·3	·247	+0·1
	Dec.	29·71	541·71	−24·2	·274	−0·1
2000	Apr.	30·14	51664·14	−25·0	1·336	0·0
	June	18·06	713·06	−25·5	·361	−0·3
	July	19·06	744·06	−25·4	·376	−0·1
	Aug.	28·99	784·99	−25·2	·397	+0·2
	Sept.	20·94	807·94	−25·1	·409	+0·3
	Oct.	19·87	836·87	−25·3	·423	+0·1
	Dec.	14·69	892·69	−25·5	·452	−0·1
2001	July	4·11	52094·11	−23·9	1·553	+0·7
	Aug.	15·03	136·03	−24·2	·575	+0·1
	Sept.	29·90	181·90	−24·1	·598	−0·2
	Oct.	30·83	212·83	−23·4	·614	+0·2
	Nov.	26·81	239·81	−23·5	·627	−0·2
	Dec.	29·73	272·73	−23·2	·644	−0·2
2002	May	27·10	52421·10	−20·9	1·719	−0·1
	June	23·02	448·02	−20·2	·733	+0·1
	July	21·04	476·04	−19·8	·747	0·0
	Aug.	21·05	507·05	−19·0	·762	+0·1
	Sept.	23·95	540·95	−18·4	·780	−0·1
	Oct.	23·90	570·90	−17·8	·795	−0·3
	Dec.	9·73	617·73	−15·9	·818	+0·2
2003	Jan.	5·72	52644·72	−15·4	1·832	−0·2
	June	21·10	811·10	−8·3	·916	0·0
	July	14·07	834·07	−7·1	·928	+0·2
	Aug.	15·05	866·05	−6·2	·944	−0·2
	Sept.	14·97	896·97	−4·8	·960	+0·2
	Oct.	27·82	939·82	−4·5	·981	−0·2
	Nov.	27·75	970·75	−4·5	·997	0·0
2004	July	5·10	53191·10	−15·1	2·108	+0·5
	Aug.	11·08	228·08	−17·5	·127	−0·2
	Sept.	6·01	254·01	−18·2	·140	+0·1
	Oct.	25·88	303·88	−20·0	·166	0·0
	Nov.	26·79	335·79	−21·0	·182	−0·1
2005	Jan.	2·71	53372·71	−22·2	2·200	−0·4
	May	23·12	513·12	−23·8	·271	+0·2
	June	28·08	549·08	−24·1	·290	+0·3
	Aug.	6·98	588·98	−25·0	·310	−0·3
2006	July	3·11	53919·11	−25·0	2·477	+0·3
	Aug.	10·06	957·06	−25·1	·496	0·0
	Sept.	9·00	987·00	−25·1	·511	−0·1
	Oct.	24·86	54032·86	−24·9	·534	−0·1
2007	July	13·04	54294·04	−22·1	2·666	+0·3
	Aug.	6·04	318·04	−22·5	·679	−0·4
2008	July	13·11	54660·11	−13·8	2·852	0·0
	Aug.	4·05	682·05	−12·7	·863	+0·2
		30·05	708·05	−11·6	·876	+0·3
	Sept.	26·93	735·93	−10·7	·890	−0·1
	Oct.	16·90	755·90	−9·9	·900	−0·2

TABLE IV (*concluded*)

Date (UT)		*MJD*	*Velocity* km s⁻¹	*Phase*	*(O – C)* km s⁻¹
2009	June 12·10	54994·10	−5·9	3·021	+0·1
	July 2·11	55014·11	−7·0	·031	−0·1
	20·12	032·12	−7·8	·040	+0·1
	Aug. 8·08	051·08	−9·0	·049	0·0
	30·00	073·00	−10·0	·061	+0·3
	Oct. 8·91	112·91	−13·0	·081	−0·3
	Nov. 3·89	138·89	−14·2	·094	−0·1
	Dec. 20·73	185·73	−16·3	·118	+0·1
2010	Jan. 6·71	55202·71	−17·1	3·126	+0·1
	June 23·11	370·11	−22·2	·211	0·0
	Aug. 5·02	413·02	−23·1	·233	−0·1
	Sept. 15·93	454·93	−23·6	·254	0·0

* DAO photographic observation[85], weight 0·01.
† Mt. Wilson Cassegrain photographic observation[88,89], wt. 0.
‡ Mt. Wilson coudé photographic observation[90], wt. ¼.
§ Ames photoelectric observation[44], wt. 0·02 (but see text).
¶ Observed with ESO *Coravel*, weight ¼.
‖ Observed with DAO 48-inch telescope, weight 1.

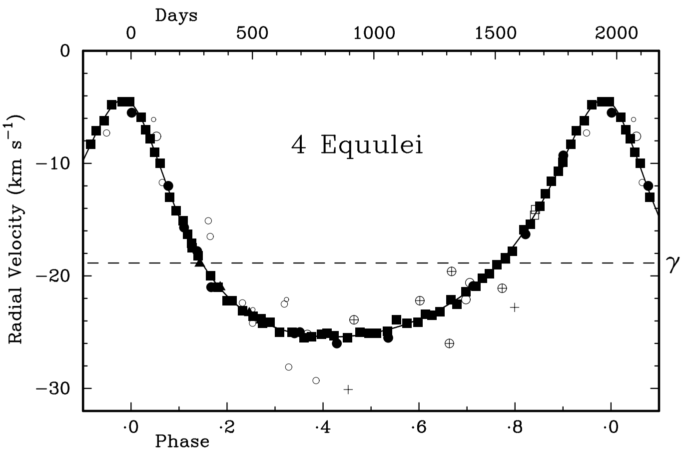

FIG. 5

As Figs. 1 and 2, but for 4 Equ. Three different qualities are recognized here among the Ames velocities, and are plotted with different sizes of open circles. Circles with plusses in them denote early DAO measurements[85].

In the solution of the orbit, the Cambridge observations have been attributed full (unit) weight; they represent by far the main weight of the whole data set. The 1999 DAO measures have also received full weight. The OHP/ESO velocities and the two from the 100-inch coudé have been weighted ¼, and the Ames ones 0·02 for those noted by their authors as quality A, with reduction by factors of 2 and 5 for those given qualities B and C (identified in Table IV by one and two colons after the velocities), respectively. The five early DAO measures warrant a weighting of only 0·01, and the two early Mount Wilson ones have not been included in the solution at all. On that basis, the orbital elements are as given in the last column of Table V; the orbit is illustrated by Fig. 5.

TABLE V

Orbital elements for the four stars

Element	*60 Psc*	*27 Ari*	*EZ UMa*	*4 Equ*
P (days)	1662·3 ± 1·4	130·655* ± 0·008	184·258 ± 0·023	1976·6 ± 0·8
T (MJD)	53480·1 ± 2·5	53637·0 ± 0·4	54060·2 ± 2·4	52976·8 ± 2·7
γ (km s^{-1})	+26·68 ± 0·03	−117·92 ± 0·03	+11·98 ± 0·05	−18·85 ± 0·03
K_1 (km s^{-1})	7·55 ± 0·04	5·98 ± 0·05	8·34 ± 0·08	10·54 ± 0·05
e	0·488 ± 0·004	0·366 ± 0·007	0·102 ± 0·008	0·389 ± 0·003
ω (degrees)	275·5 ± 0·7	22·2 ± 1·3	287 ± 5	14·2 ± 0·6
$a_1 \sin i$ (Gm)	150·6 ± 0·8	10·00 ± 0·08	21·01 ± 0·19	264·0 ± 1·3
$f(m)$ ($M_\odot$)	0·0494 ± 0·0008	0·00234 ± 0·00006	0·0109 ± 0·0003	0·1881 ± 0·0028
R.m.s. residual (wt. 1) (km s^{-1})	0·16	0·23	0·34	0·22

*The true period (in the rest-frame of the system) is 130·706 ± 0·008 days. It differs from the observed period by 6·8 standard deviations.

The mass function of 4 Equ, at nearly 0·19 $M_\odot$, is large enough to be very significant. If it is assumed that the mass of the primary star is about the received value for a main-sequence star of its type, say 1·3 $M_\odot$ (and authors[96–98] who have listed models for sets of stars have concurred with such a value), then the mass function demands for the secondary a minimum of almost exactly one solar mass. That must imply a secondary that is not much later in type than the Sun, if indeed it is a main-sequence star at all. Its absolute magnitude could scarcely be fainter than +5, so it could at most be 2–2½ magnitudes fainter than the primary, and being of later type its spectrum would match the mask in *Coravel*-type instruments better than the primary and so would give a proportionately stronger 'dip' in radial-velocity traces. That makes it a bit surprising that no sign of it has been noticed in such traces; moreover, the velocity curve in Fig. 5 shows not the least indication of the inflections that characterize incipiently double-lined binaries in the vicinity of the γ-velocity, where blending between the components 'drags' the observed velocities systematically towards the γ-velocity. There is, perhaps, a possibility that the secondary could be a white dwarf, but in that case it could be expected to have featured rather conspicuously in ultraviolet sky surveys, in view of the comparative proximity of the system to the Sun. If the writer were a gambler, he would be inclined to bet that the secondary is itself a binary system. It could then consist of stars with masses only of the order of 0·5 $M_\odot$, with a total luminosity so small as to put it safely out of comparability with the primary; the period should be expected to be weeks rather than days, otherwise some evidence of chromospheric activity could be expected.

If for the moment we consider the secondary to be a single star, the size of the mass function leaves little scope for the orbital inclination to depart far from 90°, so we might take $\sin i$ as 1 in the expression, $(1 + q)(a_1 \sin i)$, for the projected semi-major axis of the relative orbit of the components of 4 Equ, making it about 600 Gm or 4 AU. At their distance of 40 (or perhaps 50) parsecs their angular separation should amount to 0″·1 (or nearly so), and the system ought therefore to be readily resolved by speckle interferometry if the magnitude difference were not prohibitive, as it might be if the secondary is either a white dwarf or a binary. In the latter case, $\sin i$ might depart appreciably from unity, so the actual (as opposed to projected) separation of the components could be somewhat greater than has just been calculated, but not by any large factor.

The mean value for the rotational velocity of 4 Equ is 5·7 km s^{-1} according to the Cambridge radial-velocity traces; it reproduces very well from one observation to another, the r.m.s. deviation of the individual values being less than 1 km s^{-1}. Very similar values of $v \sin i$ have already appeared in the literature[97,99].

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NOTES FROM OBSERVATORIES

CCD OBSERVATIONS OF THREE RR LYRAE-TYPE STARS: CL ERI, CM ERI, AND CN ERI

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A total of 1072 B, V, and I_c-band CCD frames have been taken for three *GCVS* RR Lyr-type variables (CL Eri, CM Eri, and CN Eri), for which only coordinates were known. Observations were made with the 76-cm telescope of the South African Astronomical Observatory using an SBIG CCD ST-10XME. The inferred periods and light-curves confirm that CL Eri ($P = 0^{d}{\cdot}644$), which was found to exhibit the Blazhko effect, and CN Eri ($P = 0^{d}{\cdot}580$) are RR Lyr-type variables. CM Eri is most probably a Population-II Cepheid with a period of $0^{d}{\cdot}824$.

Introduction

The variability of CL Eri, CM Eri, and CN Eri was discovered photographically in 1963 by Hoffmeister[1], who determined the coordinates of those stars and classified them as RR Lyr-type variables, although he could not determine their periods. Those stars were included in the *GCVS*[2] and no one has observed them since then.

We included them in our programme of CCD observations of southern-hemisphere pulsating variables and here we report the results. We determined the periods and constructed the light-curves, which confirm Hoffmeister's[1] classification for CL Eri and CN Eri, whereas CM Eri is most probably a Population-II Cepheid.

Observational data

We carried out CCD observations of the three stars in 2010 December and in 2011 January (over the JD 2455543 – 2455580 time interval) with the 76-cm telescope of the South African Astronomical Observatory at Sutherland. We used an SBIG CCD ST-10XME camera equipped with BVI_c filters of the Kron–Cousins system[3].

We first reduced only the observations made during photometric nights. On each such night we used the method ofYoung & Irvine[4] to determine atmospheric extinction at two-to-three-hour intervals by observing two pairs of extinction stars (one red and one blue) in succession: one pair was located near the zenith, and the other near airmass 2. We also computed the extra-atmospheric magnitudes of extinction stars, which we then used to measure extinction based on observations of one of the two star pairs near the centre of the two-to-three-hour interval mentioned above. We used the following standard stars from E regions[5] as our extinction stars: E146, E167, E201, E205, E272, E274, E303, E394, E408, E431, E443, E4100, E4108, E501, E507, E534, E607, E651, E698, E750, E791, E793, E802, and E875.

We also used the same measurements of the above standards to determine the transformation coefficients ζ and μ from extra-atmospheric magnitudes b, v, and i into magnitudes of the BVI_c system of Kron & Cousins[3]:

$$B = b + \zeta_B\,(B - V) + \mu_B \quad \text{and} \quad V = v + \zeta_{BV}\,(B - V) + \mu_{BV} \quad (1)$$

$$V = v + \zeta_{VI}\,(V - I)_c + \mu_{VI} \quad \text{and} \quad I_c = i + \zeta_I\,(V - I)_c + \mu_I \quad (2)$$

We used the observations made during the best nights to determine the average coefficients $\zeta_B = 0{\cdot}0156 \pm 0{\cdot}0007$, $\zeta_{BV} = -0{\cdot}0675 \pm 0{\cdot}0006$, $\zeta_{VI} = -0{\cdot}0673 \pm 0{\cdot}0006$, and $\zeta_I = 0{\cdot}0113 \pm 0{\cdot}0006$, which we employed to determine the zero points, μ, for each night by formulae (1) and (2).

Transformation of instrumental magnitudes into the standard system requires several iterations. In the process of the first iteration the colour indices $B - V$ and $V - I_c$ are unknown and set equal to zero. After each iteration we computed the colour indices and stopped the process once they changed by less than $0^{m}{\cdot}001$.

As a result of the reduction of the data for all photometric nights we obtained a catalogue of positions and magnitudes of all objects on the best CCD frames. We identified constant stars from this catalogue and used them as comparison stars for the differential photometry of all stars on all CCD frames including those taken on non-photometric nights, for which we made atmospheric corrections based on the average extinction coefficients: $\alpha_B = 0{\cdot}267 \pm 0{\cdot}019$, $\alpha_V = 0{\cdot}142 \pm 0{\cdot}016$, and $\alpha_I = 0{\cdot}065 \pm 0{\cdot}007$.

Results

We obtained a total of 1072 CCD frames. We have provided the results of reduction in a table which is available in electronic form at the CDS *via* anonymous ftp to cdarc.u-strasbg.fr (130.79.128.5) and we show them graphically in Fig. 1, where we used the ephemerides that we determined from our own observations:

$$\text{CL Eri: } JD_{max} = 2455577{\cdot}060 + 0{\cdot}64448\,E$$
$$\text{CM Eri: } JD_{max} = 2455578{\cdot}319 + 0{\cdot}82374\,E$$
$$\text{CN Eri: } JD_{max} = 2455577{\cdot}307 + 0{\cdot}57967\,E$$

The light-curves in Fig. 1 confirm the classification of CL Eri and CN Eri as RR Lyr-type variables; the relatively large scatter of data points on the light-curves of CL Eri ($0^{m}{\cdot}050$ (B), $0^{m}{\cdot}038$ (V), and $0^{m}{\cdot}029$ (I_c), compared with $0^{m}{\cdot}024$ (B), $0^{m}{\cdot}019$ (V), and $0^{m}{\cdot}017$ (I_c) for CN Eri and $0^{m}{\cdot}022$ (B), $0^{m}{\cdot}019$ (V), and $0^{m}{\cdot}018$ (I_c) for CM Eri) suggests that this star may exhibit a Blazhko effect[6]. As for CM Eri, the shape of its light-curves indicates that it must be a Population-II Cepheid: it exhibits a characteristic hump making the maximum of its light-curve look broad and flat[2,7]. The scatter of data points on light-curve plots shows that observational errors are close to $0^{m}{\cdot}01$.

Acknowledgments

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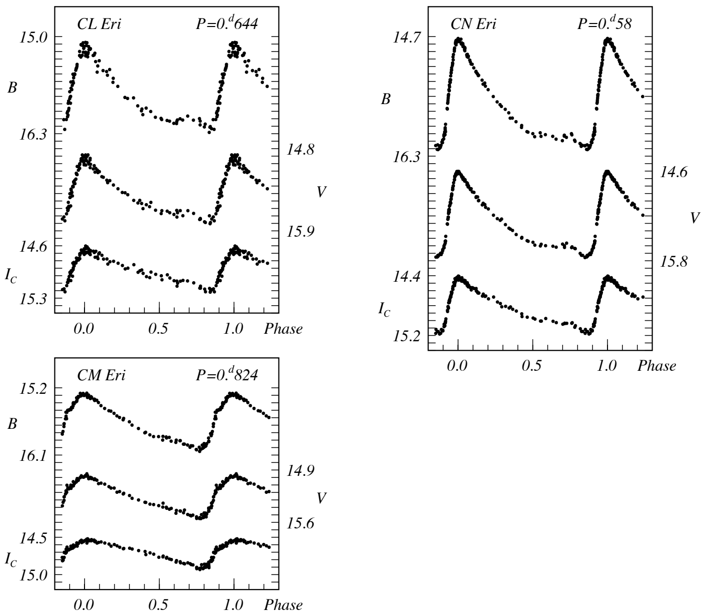

FIG. 1

Light curves of the RR Lyr variables CL Eri and CN Eri and of the probable Pop. II Cepheid CM Eri.

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REVIEWS

Discoverers of the Universe: William and Caroline Herschel, by M. Hoskin (Princeton University Press, Woodstock), 2011. Pp. 272, 24 × 16·5 cm. Price £20·95/$29·95 (hardbound; ISBN 978 0 691 14833 5).

Michael Hoskin is undoubtedly the authority today on the life and work of William and Caroline Herschel, as we have already seen in two scholarly works reviewed in these pages: *The Herschel Partnership* and *Caroline Herschel's Autobiographies* (see **127**, 198 & 199, 2007). His latest book, *Discoverers of the Universe*, encapsulates those years of study in a wonderful portrayal of the Herschel family, from the birth of William & Caroline's father, Isaac, in Magdeburg through to the completion of William's great catalogue of nebulae by his son John.

In this beautifully produced volume, complete with colour plates, extensive notes, and a list for further reading, we are taken on a journey from the family home in Hanover dominated by music to England, where the musical endeavours were eventually usurped by William's passion for astronomy; and where his source of income from teaching, composing (I have two CDs of his music which demonstrate his considerable abilities), and conducting the great works of Handel *et al.*, was replaced by a pension from King George III (to whom he dedicated his discovery of Uranus) plus money from the manufacture of telescopes. And this transformation swept his sister, Caroline, from domestic drudge dominated by her harsh mother in Hanover, through rescue by William to become a music teacher and singer in England, and on to become his invaluable assistant in his great astronomical labours. Indeed, as Michael makes very clear, it is probably fair to say that without her help and organization, he would never have become the towering figurehead of British observational astronomy that he did.

Other bit parts are played in this drama by William's brothers: Alexander, who also came to England and helped in the telescope factory, Jacob, who was mysteriously strangled in Germany, and Dietrich, who also remained in Germany and to whom Caroline returned after William's death; and by William's English wife, Mary, who produced the great scientist Sir John Herschel, and who himself is the subject of the final chapter.

This splendid account brings the characters to life and will be enjoyed by anyone with the remotest interest in the history of science, and even those who just like a good story. But one thing puzzles me: it is quite clear that William's great 40-foot telescope never lived up to expectations — Michael makes it obvious that it was really a bit of a disaster — so why is it used as the badge of the Royal Astronomical Society? — DAVID STICKLAND.

Sextants at Greenwich, by W. F. J. Mörzer Bruyns (Oxford University Press and the National Maritime Museum, Greenwich), 2009. Pp. 323, 32 × 25 cm. Price £125 (hardbound; ISBN 978 0 19 953254 4).

In the utilitarian science league, astronomers are usually rather embarrassed by the fact that their subject hovers close to the foot of the second division. That said, there is one useful thing that we astronomers think, in our heart of hearts, we should be able to do. With an almanac, clock, sextant, and a clear sky, the professional astronomer should be able to work out, to at least the nearest minute of arc, their latitude and longitude on the globe. In the pre-space-age days of maritime navigation this was usually more than just useful, it was essential and life saving.

This is why the National Maritime Museum and the Old Royal Observatory at Greenwich, London, are knee-deep in sextants and their artificial-horizon accessories, as well as the sextant's historical predecessors, these being mariner's quadrants and astrolabes, cross-staffs, back-staffs, octants, quintants, and reflecting circles.

To the Museum's and the Observatory's great credit they have embarked on a publishing and cataloguing venture producing not only the present volume on sextants, but other companion volumes on globes, sundials and horary quadrants, astrolabes, and finally, marine chronometers.

The volume under review is exemplary. The quality is superb, the clarity of the illustrations is first class, and the text is thorough, informative, and learned. Dr. Willem Mörzer Bruyns is an international authority on the subject, and was formerly the Senior Curator of Navigation at the Scheepvaartmuseum in Amsterdam. He starts by reviewing the history of celestial navigation and how it is dependent on the accurate measurement of the altitude of a specific astronomical body above the horizon at an accurately known moment of time. He then discusses how the accuracy of this measurement was improved over time, by detailed instrumental, micrometer, and vernier-scale development. The main section of the book is an in-depth catalogue of 347 instruments, each carefully illustrated and described. Origin, provenance, maker, inscriptions, and dimensions are clearly stressed.

As a collector myself, I was especially interested in the way in which the Museum's collection was 'accumulated'. The inclusion of a brief review by Richard Dunn (the NMM's Curator of the History of Navigation), is especially welcome. This highlights the rôle of three specific sources: the original collection of Sir James Caird; the instruments of George Hugh Gabb that were acquired in 1937; and those of Edgar Tarry Adams, that were bequeathed in 1973. Luckily things did not stop there. As with all good museums, great efforts have been subsequently made to 'fill in the gaps', and now Greenwich is proud to have the world's finest collection in the field.

Forget your electronic books, the book under review is what a book should be like. It is a physical and intellectual joy and something to be treasured for ages. — DAVID W. HUGHES.

Galileo's Medicean Moons: Their Impact on 400 years of Discovery (IAU Symposium No. 269), edited by C. Barbieri, S. Chakrabarti, M. Coradini & M. Lazzarin (Cambridge University Press), 2010. Pp. 271, 25 × 18 cm. Price £70/$125 (hardbound; ISBN 978 0 521 19556 0).

The year 2010 was the 400th anniversary of Galileo's discovery of the four main satellites of the planet Jupiter. While the discovery itself is well documented in scientific history, something that tends to be overlooked is that his observations helped lay the foundations of present methods of observational astronomy. This volume recognizes that, and is a record of IAU Symposium 269 held at the University of Padova, where Galileo worked between 1591 and 1610.

The first third of the book explores Galileo's discovery of those moons, which he called the Medicean Moons, in honour of the de Medici brothers, who included Cosimo, Grand Duke of Tuscany. Galileo realized that his discovery supported Copernicus' heliocentric Universe, in which the planets revolve around the Sun. In 1611, Galileo travelled to Rome in order to reconcile the Copernican model with the geocentric view of the Universe then adhered to by religious leaders. This was to prove a long, difficult, and only partially successful process. Nevertheless, it planted an important seed: the notion of using physical arguments, combined with observations, in order to deduce the motions and compositions of celestial bodies.

The last part of the book emphasizes applications of Galileo's discovery, among them the use of the Galilean-moon motions in establishing a system of longitude for navigation purposes, and in Rømer's measurement of the speed of light (1676). These articles also serve as a timely reminder of how incredibly far we have come in our exploration of the Jovian system in just 400 years — studies of the atmosphere, moons, magnetosphere, and aurorae of the Jovian system which have been made possible through *in-situ* observations by spacecraft such as *Pioneer*, *Voyager*, and of course *Galileo*. A highlight is the paper by Kivelson *et al.* (UCLA group) on the interaction between the Medicean moons and Jupiter's rotating magnetosphere. In particular, we learn how a careful analysis and combination of spacecraft magnetic data and simulations can be used to probe the structure of Ganymede's internal magnetic field, and the possible existence of an induced field generated by a putative subsurface ocean.

This volume is an excellent summary of the scientific history and studies of the Jovian system, and a reminder of how important Galileo's discovery was in shaping our present-day view of the Universe. Many beautiful diagrams and photographs of the relevant scientists and moons are found herein, a number of which would have benefitted from the use of colour in their reproduction. — NICHOLAS ACHILLEOS.

A Photographic Atlas of Selected Regions of the Milky Way, by E. E. Barnard; foreword by G. E. Dobek (Cambridge University Press), 2011. Pp. 358, 27·5 × 27·5 cm. Price £75/$125 (hardbound; ISBN 978 0 521 19143 2).

Some years ago I had occasion to go through E. E. Barnard's letters to the RAS Secretaries, to seek additional material for Bill Sheehan's excellent biographical study. Most correspondence related to his publications in *Monthly Notices*, from which it rapidly became clear that Barnard was a very fussy proof-reader, constantly making last-minute changes and additions. He was never quite satisfied with what he had written, and this hesitation would result in the manuscript of the present *Atlas* being completed by others after his death. But Barnard was a prolific author, and in his career as an astronomer successively at the Vanderbilt, Lick, and Yerkes observatories, he authored more than 800 publications. As far as I can tell, *The Observatory* did not review the first edition of the *Atlas* (1927), but its high quality easily won acclaim from all quarters, and it has become a collector's item.

Barnard (1857–1923) pursued a photographic programme extending over many years. In the summer of 1889 he first recorded the rich star clouds of the Milky Way with a 17-cm-aperture photographic telescope. He was soon capturing the intricate dark lanes in the Milky Way, along with numerous clusters and nebulae, many features showing up for the first time upon his plates: Barnard would eventually catalogue some 350 dark nebulae, including many small black spots, now called 'Bok globules'. Initially thinking that the dark lanes were regions devoid of stars, the accumulation of results forced him to accept them to be obscuring matter. Systematic work began in 1892, interrupted by Barnard's departure from Lick in 1895, and resumed in 1905 during an expedition to Mt. Wilson with a new lens paid for by Catherine Bruce. Barnard always worked long hours with almost no sleep, sometimes continuing his exposures (one of which exceeded ten hours) over more than one night, tracking with a guide telescope.

With Barnard being a perfectionist, his efforts to find a suitable publisher for the Lick plates extended over years. In 1914, publication finally occurred in volume 11 of the *Publications of the Lick Observatory*. But then he also had the Mt. Wilson plates, which were better and much more numerous, as well as an on-going photographic programme at Williams Bay. In addition to the many new objects, the 3500 plates taken by Barnard revealed a runaway star, now called 'Barnard's star', and even remote Pluto, but Barnard did not have the time to compare many of those plates which covered areas already photographed. Soon after publishing the Lick atlas he began to worry about how best to publish the on-going series, and ultimately he would personally inspect each of the 35 700 photographic prints needed for the 700 copies of *A Photographic Atlas of Selected Regions of the Milky Way*. In the end, Barnard's text had to be finished (and the work seen through the press) by Yerkes Director Edwin B. Frost and Barnard's niece, Mary R. Calvert.

The *Atlas* now before us is basically a reprint of the two-volume original, and contains a collection of over 50 beautifully reproduced plates, each with a hand-drawn key (prepared by Miss Calvert) upon the facing page. There is also a page of description for each plate. Galactic longitudes 230–310° were inaccessible from the latitudes where Barnard worked, and, as he wrote at the time, there is necessarily a concentration of photographs in the most interesting regions and dense star-clouds of Ophiuchus, Sagittarius, and Scorpius. There is a catalogue of all the dark objects. Epoch 1875·0 was used in the original edition, but Dobek has usefully produced a second list with epoch 2000·0 positions given for locating objects on modern charts. The publishers have reproduced the illustrations from the original photographic plates. Barnard's classic shots of the nebula of Rho Ophiuchi and the North American Nebula are among my personal favourites. Dobek also adds a short biography of Barnard and a further plate giving an electronic montage of all of Barnard's 50 exposures in a rather impressive manner: this was something Barnard had wanted to do for himself but couldn't, in order to see how all the dark lanes would connect and relate together.

This is a really beautiful *Atlas*. Its high price will deter many potential individual collectors, but nevertheless I'm sure there will be plenty of purchasers. Observatories possessing a rare copy of the original should also strongly consider adding it to their collections. — RICHARD MCKIM.

Astronomical Spectroscopy: An Introduction to the Atomic and Molecular Physics of Astronomical Spectra, 2nd Edition, by J. Tennyson (World Scientific, Singapore), 2011. Pp. 223, 23·5 × 15·5 cm. Price £26/$37 (hardbound; ISBN 978 1 86094 529 8).

I was both surprised and delighted to receive for review a book on spectroscopy for astronomers, because I had been under the impression that astronomers were not that interested in spectroscopy these days, or at least that spectroscopy is not a major component in an undergraduate curriculum in astronomy today. Yet here, from University College London, where I learned my basic spectroscopy under Professor Allen and Dr. Garstang half a century ago, is a splendid little book that should certainly be considered by anyone teaching the subject.

The level of the book is rather more than would be needed in the second year of the four-year Canadian curriculum. For a third-year class, it would supply the basics, but most teachers would take it a little bit further. The level is perhaps ideal for students and astronomers who want to be sufficiently familiar with the basics of atomic and molecular spectra to be able to interpret intelligently the spectra that they are likely to come across in astronomical sources, but who do not want to get too involved in frightfully difficult theoretical texts such as Condon & Shortley.

The book covers the basic structure of hydrogen-like and helium-like spectra, alkali and alkali-earth spectra, pure rotation, vib-rot, and electronic spectra of diatomic and simple polyatomic molecules. The short chapter on X-ray spectra is restricted to the outer-electron spectra of such highly ionized atoms as occur in places such as the solar corona, and does not deal with the inner-shell transitions of heavy atoms that one normally thinks of as X-ray spectra. If you are embarrassed to admit that you are not sure what are meant by *LS*-coupling, spin–orbit interaction, terms, levels, states and multiplets, Grotrian diagrams, autoionization, dielectronic recombination, Franck–Condon factors, the Born–Oppenheimer approximation, and Hund's coupling cases, this is a book where you will find the concepts explained gently, clearly, and accurately, with worked examples. If you are already familiar with these terms, you will be looking for a book more advanced than this one.

A particularly attractive feature of the book is that numerous examples are given to illustrate spectroscopic principles from astronomical sources, and in all wavelength regions — X-ray, visible, and radio. I was unfamiliar with many of these astronomical examples, and reading about them greatly increased my enjoyment of the book. The astronomical content of the book should not deter anyone, however, from using it to teach a class of non-astronomy physics students.

Near the beginning of the book there is a section on wavelengths, wavenumbers, and frequency. Apart from the careless use of the Greek letter ν for frequency and for wavenumber in the same paragraph, the author misses another important point, namely that it is customary to quote wavenumbers *in vacuo* but wavelengths in standard air, so one is not exactly the reciprocal of the other. This is not a trivial point, for the student will quickly become frustrated when he or she cannot correctly calculate wavelengths from a table of term values and will not be able to understand why.

While the structures of atomic and molecular spectra are covered very well, there is very little on intensities or on line profiles and their interpretation, so teachers who wish to cover these important topics will need to supplement the text. While Einstein coefficients are mentioned, the *B*-coefficient gets its usual short shrift and is not defined adequately. In particular, there is a formula for "intensity" of an absorption line. (The strength of an *absorption* line is correctly described by its equivalent width, not by its "intensity".)

While the book is largely 'Système International' (the $4\pi\varepsilon_0$ is included correctly in most equations), dipole moments are expressed in "Debyes", which are undefined. A debye (correctly written with a small d) was apparently 10^{-18} cgs esu of dipole moment, but is there really anyone around these days who knows what a 'cgs esu of dipole moment' might once have been? In atomic and molecular calculations, most practitioners usually express dipole moments in atomic units of a_0e, which are about two-and-a half 'debyes'. The author from time to time uses such curious phrases as some molecule does "not possess a dipole", or some other molecule "has a dipole".

I have been asked to teach a similar course this fall, and I expect to make a lot of use of the book, and I would happily recommend it strongly to anyone who is also faced with teaching such a course. It may well save you from having to prepare your own set of teaching notes from scratch. I shall not recommend it, however, to the students as a text to study from. This is largely because the text appears to have been set more or less directly from the author's teaching notes with little change. I had somehow imagined that a book publisher employed an army of experienced professional proof-readers and copy-editors to weed out typing and other mistakes, but perhaps I have not kept up with the times. Is a publishing house now nothing more than a printing press for reproducing an author's typescript with no further editing? I don't think I have ever come across a scientific book with quite so many spelling, grammatical, and punctuation mistakes as this one — even though it's a second edition. Even vectors (which the author has underlined, presumably intending them to be printed in boldface, though he would be using the wrong copy-editor's symbol for this) appear in print as presumably originally typed — thus $\underline{r}$ rather than $\mathbf{r}$. This is inexcusably sloppy mathematical typesetting, and it makes formulae unnecessarily difficult to read.

For the most part the spelling mistakes will be easily recognized as such, and will not detract from the otherwise good science and pedagogy. However, poor proof-reading does occasionally intrude into the equations. Thus I found one equation in which a factor me^2 in the middle of a long formula was rendered as m_e^2 (yes, that does make a difference), and the haphazard use of $\underline{v}$, $\underline{V}$, and V meaning sometimes speed and sometimes electric potential in the same set of equations renders the explanation of spin–orbit interaction incomprehensible.

As a teacher already familiar with much of the material, I am not unduly disturbed by such careless mistakes. I recognize them as such and I can get on with reading and understanding the text, which is otherwise good. Thus, I am grateful for this book, which will help me greatly with my class — but I shall not distribute it to my students. — JEREMY B. TATUM.

Extra-Solar Planets: The Detection, Formation, Evolution and Dynamics of Planetary Systems, edited by B. A. Steves, M. Hendry & A. C. Cameron (CRC Press, Boca Raton, FL), 2011. Pp. 289, 24 × 16 cm. Price £76·99 (hardbound; ISBN 978 1 4200 8344 6).

Whilst the study of extrasolar planets is often regarded as a recent development, it is certainly not a new phenomenon. The 1940s saw several claimed detections of planets *via* astrometry, around objects including 61 Cyg and 70 Oph, and later Barnard's star. The conclusion of radial-velocity programmes at several of the great observatories led Otto Struve[1] to propose, in these pages in 1952, the effort to be redeployed towards searches for planets. A Jupiter-mass planet in a 1-day orbit around a solar-mass star induces a velocity variation of 210 m/s, which was within the reach of the most powerful coudé spectrographs of the day. Struve also noted the possibility of discovering planets *via* transits across their parent stars, an issue which was subsequently investigated by Rosenblatt[2] among others. Despite this, the community had to wait until 1995 for the first widely accepted detection of an extrasolar planet around a normal star[3], and until late-1999 for announcement of the first transiting specimen[4,5]. Although the subject has a long history, its current fast pace means monographs about extrasolar planets risk becoming dated very quickly.

The subject of this review comprises a collection of lectures delivered at a summer school held on the Isle of Skye over two weeks in 2007 May/June, dedicated to the study of extrasolar planets. An unfortunate three-year delay in publication means the book itself is out of date at birth. For example, the diagram on page 42 includes nine transiting planets, whereas by my own count there are 115 such objects at the time of writing. At various places through the book we discover that several well-known space missions are still to produce results or even to leave the ground, including *CoRoT* (launched 2006 December), *Kepler* (2009 March), and *Herschel* (2009 May). The crucial question is therefore whether this book possesses what is glibly termed 'legacy value'.

The volume begins with a useful review of the detection of transiting extrasolar planets (Andrew Cameron), which has good coverage of the basic ideas. Two lectures on microlensing (Martin Dominik) follow, and are an ideal quick introduction for graduate students. After a dip into polarimetry, there are four chapters on the formation and evolution of planetary systems, and then five meaty missives on dynamical effects. The final article is a study on the motion of Earth's magnetic poles 13 000 years ago, which is out of place given its specificity. The last study surprised me in that it included *hand-drawn* diagrams, on which substantial time has clearly been lavished. Such an approach is novel to those of us who are too young to have experienced the VAX/VMS and MS-DOS era, but the legibility of the results is in pleasant contrast to other diagrams which have been pixellated into submission.

Other gripes? There is a 'color' figure which is not in colour, and another which does not have the stated "right panel". And "Earth gravitational potential" occurs twice in succession in the index, referenced to two different pages. Overall, however, the book is nicely presented if out of date. It has only patchy coverage of the observational side of things, but a very good overview of evolutionary and dynamical effects. Would I buy one myself? No, but my review copy will be lent to several colleagues interested in specific chapters. — JOHN SOUTHWORTH.

References

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(3) M. Mayor & D. Queloz, *Nature*, **378**, 355, 1995.
(4) G. W. Henry *et al.*, *ApJ*, **529**, L41, 2000.
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An Introduction to Star Formation, by D. Ward-Thompson & A. P. Whitworth (Cambridge University Press), 2011. Pp. 208, 25 × 18 cm. Price £40/$65 (hardbound; ISBN 978 0 521 63030 6).

An Introduction to Star Formation is a textbook aimed at students in the last years of an undergraduate programme or the first years of a graduate one. It aims to present the well-established fundamentals of star formation from a foundations-up perspective. The authors assume prior knowledge of maths and physics, but not necessarily of astronomy. Although it is intended as a textbook, there are no problem sets or exercises. However, there is a suggested reading list at the end of each chapter.

The book is laid out well, starting with well-established foundations of star formation. It follows the formation of a solar-mass star from the earliest stages in a molecular cloud until it reaches the main sequence. However, there is also a chapter discussing high-mass star formation and a section discussing brown dwarfs. Theory and observations are interspersed throughout the text, providing a good balance for the reader. The authors are good about including cautionary statements for things that are not well-established. They also include a list of currently unsolved questions in the field as motivation for some of the topics discussed in the book.

There is a table at the back of the book that lists all of the symbols used in the text along with their definitions. It is helpful to have such a reference list of all of these so that the reader does not have to go searching through the text for the first instance that the symbol is defined, particularly since the symbols vary somewhat from book to book within this field.

Star-formation regions are visually quite pleasing but the book's small black-and-white figures do them no justice. Although colour figures greatly increase production costs, to do it would probably have been worth it as the numerous astronomical images would have been vastly more appealing to the reader's eye had they been in colour. Many of the plots and diagrams in the book are reprinted with permission directly from journal publications. While this is out of the authors' hands, several of the data and axis labels on reprinted figures are difficult or impossible to read because either the text is too small or the resolution is too low. — CASSANDRA FALLSCHEER.

Binaries — Key to Comprehension of the Universe (ASP Conference Series, Vol. 435), edited by A. Prša & M. Zejda (Astronomical Society of the Pacific, San Francisco), 2010. Pp. 463, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58131 750 6).

This meeting was convened in Brno in 2009 June and attracted 145 attendees of whom about 60% came from Eastern Europe. There are nine sections in the book starting with 'New frontiers' occupying 39 pages, then followed by 'Modelling advancements' (67), 'Population synthesis and catalogues' (33), 'Formation and evolution' (41), 'Spectroscopy, interferometry and polarimetry' (57), 'Active, interacting and chemically peculiar binaries' (105), 'High-mass and X-ray binaries' (45), 'Low-mass binaries and exoplanets' (35), and 'Other topics' (10).

The introductory note by Andrej Prša sounded a very optimistic tone and particularly stressed the need for the community to address itself to the problems of data reduction when such projects as *GAIA*, *LSST*, and *PanSTARRS* come to fruition, to say nothing of the current accumulation of data by *CoRoT* and *Kepler*. I then turned the page ready to read the introductory lecture by Edward Guinan only to find that it was not included — presumably it had missed the deadline. Checking just to see that the summary was actually in place I was rather surprised to find in the 'Concluding remarks' a rather brutal summing-up by Slavek Rucinski. He laid into the tendency by speakers to try and get through too many viewgraphs in too short a time, and a tendency to include papers in which one eclipsing system is presented along with the elements of the orbit when, to quote, "nobody will ever use these solutions", and "such papers achieve nothing much except padding of publications lists". One can almost see the justification for this point of view, particularly since one of those papers has 60 authors discussing one, admittedly interesting, system. Perhaps he is being too hard, because whilst the tendency will be for large numbers of binaries in need of treatment when the current and near-future surveys are concluded, when all is said and done, each system is a unique one and examining a single binary in great detail must be regarded as a very useful observational experience for young researchers, not to mention standing in front of your peers and talking about it.

I was intrigued by the rather ambitious subtitle — '*Key to Comprehension of the Universe*'. This was justified by the fact that distances to eclipsing binaries can be found accurately by the method called the direct distance estimation in which models of light curves in absolute flux from an eclipsing system are compared to observed absolute flux curves. When used in eclipsing binaries in external galaxies this appears to give distances to the LMC at the 3% level with the potential to get to 1%. There were two papers dealing with the use of wide binary stars to assess the possible existence of dark matter, and some contributions on the binary SNe Ia which now of course mark the accelerating expansion of the Universe.

A notable paper on the observational front was an assessment by Andrei Tokovinin of the current and future state of instrumentation for binary research, and came to the conclusion that filar-micrometer measures are no longer of any use. He will find at least some resistance to this point of view from this writer. Many fine observers have spent a lifetime in literally back-breaking toil to accumulate data on visual binaries without which we would be much the poorer in terms of our knowledge of stellar masses for pairs with periods between about 50 and 200 years. The speckle technique may produce very accurate positions but it covers 40 years at most, whilst traditional techniques have been going for almost 200 years. And whilst on the subject of the old methods it was very encouraging to read that plate archives still have much to contribute. A paper by Samus *et al*. described how 478 new variable stars were found in an area of sky 10 × 10 degrees covered by 254 plates taken in the Crimea over a period of 19 years. Of those stars 156 appear to be eclipsing binaries.

The volume appears in the now-familiar layout but I found in this case the conference photos to be unflattering and some appear dark on the page as if the photographer was wary of using flash for fear of disturbing speakers. I'm not sure that images of people in action are all that desirable — it is surely better to get them to pose outside the sessions. There is no index. — ROBERT ARGYLE.

High Energy Astrophysics, 3rd Edition, by M. S. Longair (Cambridge University Press), 2011. Pp. 861, 25 × 19·5 cm. Price £50/$85 (hardbound; ISBN 978 0 521 75618 1).

The third edition of Malcolm Longair's *High Energy Astrophysics* has returned to a single volume after the second edition, of which two of the three intended volumes were published. The result is a very well-written and highly informative book which advanced undergraduates and researchers at all levels will find stimulating and useful. The book is divided into four sections, the first of which is a very readable introduction to astronomy, and gives some context for the remaining sections, which cover physical processes, applications to high-energy astrophysics in our Galaxy, and a major section on extragalactic processes. The introduction is really very nice, with concise summaries of our state of knowledge of a wide range of topics. Although the book has sacrificed some historical material from previous editions, there is some history of the developments, and it is interesting to see traditional methods juxtaposed with more modern treatments in places. The other sections are familiar territory for those who have read previous editions, and the fine balance between detailed mathematics and readability is maintained. Where the mathematics become a little more involved, there is often a physical argument which precedes them, helping to develop insight and preparing the reader for the more rigorous calculations. The calculations themselves are well-presented, with plenty of explanation and helpful physical interpretation of the results. This combination of physical arguments and more-rigorous calculations is a hallmark of Malcolm Longair's books and one which makes them such an excellent resource for a student or researcher. Moreover, the book covers an impressive array of topics, and at over 800 pages it would be a major undertaking to study it all, but it is possible to be selective and cover a subset of the material. It is also a valuable resource to dip into, to find concise descriptions of current results and present-day understanding, along with the underpinning physics where appropriate. All in all, this is a beautifully written and authoritative book, delivered with enthusiasm and offering the reader the chance to gain insight and understanding of a fascinating topic. — ALAN HEAVENS.

UP2010: Have Observations Revealed a Variable Upper End of the Initial Mass Function? (ASP Conference Series, Vol. 440), edited by M. Treyer, T. K. Wyder, J. D. Neill, M. Seibert & J. C. Lee (Astronomical Society of the Pacific, San Francisco), 2011. Pp. 416, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 760 5).

Claims both in favour of, and against, the universality of the Initial Mass Function (IMF) prompted what appears to have been a very successful workshop in Sedona, Arizona, in 2010 June. Presumably 'UP' in the workshop title is intended to signal that the focus here involves the upper end of the mass function, *i.e.*, high-mass stars.

There are two reasons why I would have liked to have attended UP2010: firstly because Sedona would have been an incredibly spectacular location for a meeting — I briefly witnessed its numerous red-rock formations *en route* from Phoenix to Flagstaff; and secondly because the focus of the workshop is close to my own research interests (my own well-publicized ESO press release on this subject was spectacularly ill-timed, coming as it did only a few weeks after the meeting). Both the organizers and ASP are to be congratulated for having produced a copy of the proceedings so rapidly, less than ten months after the meeting.

The proceedings themselves contain various observational and theoretical articles involving the upper mass function, sub-divided into four sessions. From 'near to far' these were: 'Star clusters', 'Resolved stellar populations', 'Integrated galaxy light', and 'The high-redshift Universe'. To provide a flavour of the material included, my personal favourites from these are words of caution from Phil Massey on claims relating to the upper mass limit and variations in the slope of the IMF; Brad Whitmore's confidence that individual stars at distances up to 20 Mpc can be "relatively easily" identified from *HST/WFC3* imaging; Erik Hoversten and Karl Glazebrook's evidence favouring IMF variations due to surface-brightness variations in galaxies; and James Neill on the increasing rôle of extreme supernovae in assessing the upper stellar IMF in faint star-forming galaxies.

As always, such proceedings provide an instantaneous snapshot of the community's perspective on questions relating to possible variations in the upper end of the IMF, albeit with rather limited legacy value since there is no formal record of the (apparently) heated discussions, which is a real pity for those of us unable to participate in person. Still, the proceedings themselves are well produced and certainly well worth reading for anyone whose research involves the detailed form of the high-mass IMF, although given the cost I'd advise borrowing a copy from your institute's library before splashing out yourself. To complement these proceedings an extensive *ARA&A* review of the IMF has recently been written by Nate Bastian *et al.* while the IMF@50 meeting from 2004, commemorating the golden anniversary of Ed Salpeter's first introduction of the IMF, is also worth a look. — PAUL CROWTHER.

Canonical Gravity and Applications: Cosmology, Black Holes, and Quantum Gravity, by M. Bojowald (Cambridge University Press), 2010. Pp. 305, 25·5 × 18 cm. Price £45/$75 (hardbound; ISBN 978 0 521 19575 1).

A canonical formulation of a physical theory is one where the variables of the theory are written in terms of objects existing in space and their momenta, the equations of motion for the theory then cast as a system of first-order time-evolution equations for these quantities. This approach is typically extremely useful when considering the consistency, stability, and symmetry properties of a theory. An example in the case of classical mechanics is the Hamiltonian formalism where the appropriate canonical quantities are the instantaneous positions of point particles and their momenta. The focus of this book is the canonical formulation of field theories, in particular that of the prevailing classical theory of the gravitational field: General Relativity (where now the 'objects existing in space' are the descriptors of the geometry of space itself).

The emphasis on canonical methods is atypical amongst textbooks on General Relativity, and allows here for a detailed discussion of areas rarely seen elsewhere, among them the Dirac analysis of constrained Hamiltonian theories, the Holst action, Ashtekar–Barbero variables, and symmetric criticality.

In addition, the book applies the formalism extensively to familiar areas in gravitation. The suitability of such methods in a particular gravitational problem is in part tied to the aptness of the use of a preferred notion of time in the space–time. Therefore canonical methods can be especially useful in cosmology, where there is strong evidence for a preferred state of rest in the Universe and thus a preferred synchronization of clocks. This is well illustrated in the book *via* treatments of cosmological perturbations in the canonical formalism, and the Bianchi space–times.

Exposition throughout the book is clear and rigorous with a strong emphasis on the rôle of Poisson geometry in addition to the more familiar Riemannian geometry. A good selection of examples and exercises is provided to illustrate concepts. Although the book has a dedicated mathematical appendix, it should be noted that some knowledge of differential geometry and variational principles is assumed. In summary, the combination of rarely found material and the presentation of a very powerful formalism with which to solve problems in astrophysics and cosmology makes for a highly recommended book and a very valuable addition to the literature. — TOM ZLOSNIK.

Self-Organized Criticality in Astrophysics: The Statistics of Non-Linear Processes in the Universe, by Markus Aschwanden (Springer-Praxis, Heidelberg), 2011. Pp. 430, 24 × 17 cm. Price £108/$169/€119·95 (hardbound; ISBN 978 3 642 15000 5).

Self-organized criticality (SOC) is what has happened when a non-linear system, driven somewhat chaotically, gives rise to a power-law distribution of a number of something *versus* the size, energy, time scale, *etc.*, of that something. Author Aschwanden first encountered the concept while trying to understand solar activity, especially statistics of solar flares, but his astronomical examples trod the Universe from lunar-crater sizes to some aspects of galaxy formation and cosmology. Conflict-of-interest statement one: I saw some of the astrophysics material in advance of submission and complained that some of the models used to make things come out right (*e.g.*, for star formation) were not the current best-buy models.

In addition, on beyond the Universe, Aschwanden looks at laboratory experiments (sand piles and vortices in superconductors), human activities (yes, the stock market), the Earth (quake statistics), and a variety of numerical, analytical, and experimental methods that are useful for recognizing SOC and analyzing systems suffering from it that do not depend upon the precise system at hand. Each chapter ends with a textbook-like summary and set of problems, though I suspect self-instruction will be the commonest use of the volume.

Ordinary self-organization (ripple-type sand dunes, for instance) also gets a look-in along with the SOC sort (avalanches of sand piles when additional grains are dropped slowly on top). The ordinary sort yields a preferred scale: distance between the dunes (or, perhaps, between spiral arms of galaxies), rather than a power-law distribution.

I predict that every scientist reading parts of this book will encounter items that trigger the reaction, "Of course, that is the way this stuff works; why didn't I think of it first?", and others that invite the response, "I don't believe a word (or an equation — there are many) of it!" They won't be the same items for all readers, and the "of course" reaction is perhaps more likely outside one's own field, as is often the case for 'grand design' ideas (Durrant or Gibbon on history; anybody on evolution; Bryson on anything).

Second conflict-of-interest statement: for a decade or so, Markus and I were co-authors of a yearly review of the entire astronomical-journal literature. The extreme clarity of his writing, which made the collaboration work for so long, is evident in this book. — VIRGINIA TRIMBLE.

Thermal Design and Thermal Behaviour of Radio Telescopes and Their Enclosures, by A. Greve & M. Bremer (Springer, Heidelberg), 2010. Pp. 398, 24 × 16 cm. Price £108/$159/€119·95 (hardbound; ISBN 978 3 642 03866 2).

Early development of radio astronomy after WW2 started with metre-wavelength antennae and receivers: there was plenty of expertise, the antennae were simple wire structures, and surplus radar equipment could be recycled. Most importantly, as Grote Reber discovered, the sky seemed dominated by emission which was stronger at low frequencies. Unfortunately, that meant that the angular resolution was limited: $\theta = \lambda/D$, as Martin Ryle used to point out, was correct in the 'radio-astronomers' units', λ in cm, and D in miles gave θ in arcseconds! This forced the development of interferometers and of aperture synthesis. Moving to higher frequencies required the construction of parabolic dishes, but the builders soon found that the surface precision (and therefore the shortest wavelength that could be observed) was so much worse for a big antenna that the resolution achieved was actually better for a small antenna. Things got better with the clever idea of *homology*, where the gravity deflection at different attitudes resulted in a good approximation to a paraboloid at all elevations, even if the actual focal length and point changed (the sub-reflector and feed were moved as required). Then the problems remaining were less predictable: the thermal behaviour and the wind-loading.

The authors of this book provide a detailed study of the thermal behaviour of antennae. They combine theory and modelling of telescopes with a substantial amount of real data from various observatories (most frequently from their home base, IRAM). They start with descriptions of all the types of structure commonly found in steerable antennae, and discuss the merits of enclosures. The thermal environment, heat transfer, real measurements, and model calculations of the thermal behaviour of antennae are all treated in considerable detail. For moderate wavelengths and antenna sizes, passive temperature control may be sufficient (white paint, insulation, forced-air temperature equalization). For more extreme requirements, some active control — heating, cooling — might be required. This volume appears to me to be a valuable assessment of the current state of this complex set of problems. — GUY POOLEY.

A Passion for the Planets, by W. Sheehan (Springer, New York), 2010. Pp. 217, 23·5 × 15·5 cm. Price £31·99/$34·95/€34·95 (paperback; ISBN 978 1 4419 5970 6).

I have known William Sheehan as a planetary observer and an astronomical historian, so I initially expected this book to be an autobiographical account of how his interests grew and developed. It is true that as soon as I saw the subtitle: 'Envisioning other worlds from the Pleistocene to the age of the telescope', I began to wonder, because 'envisioning' (to me) means seeing something as in a vision and particularly as it will be in the future. But skipping over what is perhaps an unfortunate choice of word, we still have 'from the Pleistocene'.

This gave me pause for thought. How can we know anything about what early human ancestors in pre-literate society thought about the heavens in general and the planets in particular?

Sheehan's ideas become much clearer, however, when you discover that he is a professional psychiatrist, and that his idea is that amateur astronomers, especially planetary observers, are driven by the behaviours established genetically in the Pleistocene, as explicitly stated on p. 48: "… modern humans are still wired up neurologically and biologically to the life ways of the savannahs of Africa, and — for those of European ancestry — to those of the Eurasian Mammoth Steppe of the Ice Ages." Slightly earlier (p. 34) he establishes that he and others passionate like him "have the heart of the hunter-nomad" arising from "the seven-repeat of the human dopamine receptor D4 (DRD4) gene … associated with both attention-deficit/hyperactivity disorder [ADHD] and … novelty-seeking …". It has indeed been suggested by others that this particular gene (which arose relatively late in humans) is responsible for the expansion of hominid species from their origins in Southern and Eastern Africa.

Sheehan states (p. 26) that "I … as a professional psychiatrist, estimate conservatively that at least a quarter and possibly half of all the really single-minded amateur astronomers … have had some version of Asperger's Syndrome …". On the same page he says that "my clinical training enables me to easily recognize that this fictional character suffered from Tourette's syndrome." He later equates the "passion for planets" with (in particular) "adolescent human males" — it is slightly odd, but striking, that in every instance, he gives the word 'testosterone' a capital letter 'T' — and that the characteristics were accentuated by the climatic conditions accompanying the Ice Ages, and that study of the planets is "a much-sublimated form of the timeless passion to hunt" (p. 38).

I am rather uneasy with all this, and, for example, the suggestion that Arthur Stanley Williams was eccentric because he lived on a houseboat, as was Will Hay, because he was an actor. (At that rate, practically everyone is eccentric.) As with actual clinical diagnosis, I suspect that once a syndrome such as ADHD has entered the American Psychiatric Association's *Diagnostic and Statistical Manual of Mental Disorders* there is a tendency to apply it to all manner of human behaviours, neglecting the fact that a myriad other factors (including an unknown number of genes) affect an individual's interests and attitudes. It is the age-old 'Nature and Nurture' debate cloaked in modern science.

Sheehan continues this general tone in later chapters, discussing possible Palaeolithic and Neolithic links, before moving on to a more conventional description (albeit with psychiatric overtones) of Mesopotamian, Greek, some Mesoamerican, and subsequent astronomies, although not touching on Indian or Chinese concepts. He then includes the well-known histories of Copernicus, Tycho Brahe, Kepler, and Galileo. His treatment of these subjects is somewhat patchy, with some errors and significant omissions. He dismisses Harriot's lunar drawings, for example, in comparison with Galileo's, completely ignoring Harriot's highly detailed lunar drawings in the Egremont archives, which are far superior to Galileo's.

There is no bibliography as such, all references being given in footnotes (which sometimes repeat the text, word for word). There are a number of editorial blunders, including the acceptance of certain common misconceptions, such as the statement that the Gulf Stream keeps Europe warm. (It does not. Quite apart from an error in the name of the current involved, atmospheric heat transport across the Atlantic is far more significant.)

Readers will have to decide for themselves whether they agree with the hypotheses on the roots of astronomical interests, pursuits, and obsessions. This book is said to be the first of a trilogy. Presumably any later ones will explore the work of later planetary observers. Personally, I am not sure whether I am anxious to read them. — STORM DUNLOP.

[Note added in proof: This review refers to an edition that was not widely distributed and is no longer available.]

E. T. Talk: How Will We Communicate with Intelligent Life on Other Worlds?, by F. J. Ballesteros (Springer, Heidelberg), 2010. Pp. 231, 23·5 × 15·5 cm. Price £31·99/$34·95/€34·95 (paperback; ISBN 978 1 4419 6088 7).

In this, perhaps the most interesting and informative book yet published on the subject of possible communication with intelligent life on other worlds, Ballesteros covers the key aspects of the subject in three parts.

In the first, 'With whom? Finding life in the Universe', he looks at where other life forms might be found. This covers the search for other suitable planetary systems and how life might arise given the right conditions. A final chapter discusses the possibility of finding other life within our own Solar System.

The second part, 'With what? The search for extraterrestrial intelligence (SETI)' concisely relates the story of SETI from the initial seminal paper by Cocconi & Morrison which, in 1959, pointed out that given two reasonably-sized radio telescopes it would be possible to communicate over interstellar distances, and the first attempt made the following year by Frank Drake in Project Ozma. The historical survey continues with the false alarm triggered by the serendipitous discovery of pulsars by Jocelyn Bell in 1967 — who called the first source of such pulsing signals LGM 1 for "Little Green Men 1"! The 'Wow' signal detected in 1977 by the Ohio State University's 'Big Ear' telescope and which is still a mystery continues the story, which is brought up to date with a survey of recent and on-going SETI observations. These include Project SERENDIP, which still employs the giant Arecibo telescope, and the SETI Institute's Project Phoenix that for 5 years observed over 800 Sun-like star systems with both the Arecibo dish in Puerto Rico and the *Lovell Telescope* at Jodrell Bank in simultaneous observations to enable any signals from within our Solar System to be discounted. In what proves to be an excellent survey, Ballesteros brings us up to date with the building of the *Allen Telescope Array* (now, as of 2011 April, sadly mothballed due to major US spending cuts) and the prospect of using the *Square Kilometre Array*, which it is hoped will be completed in 2024.

The book concludes with a part entitled 'How? The language of communication'. This forms the main thrust of the book and makes fascinating reading. There is one *non sequitur*: early in the book he points out that the period when a civilization gives rise to unintended signals that could be received across the Galaxy is very short (our powerful analogue TV transmitters are now being replaced by low-power digital transmitters, cable, and satellite). In my view, this means that the chance of us listening at just the right time to another civilization is exceedingly low. But here he states that "it is likely that the first emission we detect from an extraterrestrial civilization will be a signal in their natural language". Whilst I would disagree with this last statement, it does lead on to a truly excellent analysis of language and its form and structure that I found really illuminating. We are also shown how we can produce messages that are self-decoding — with the 'Arecibo message' transmitted in 1974 towards the globular cluster M 13 given as an interesting example.

A final chapter entitled 'Is there anybody out there?' discusses Fermi's Paradox, which implies that advanced civilizations — who could have existed some 4 billion years before ours — would naturally colonize all the habitable planets in a galaxy and so, as we do not believe that an advanced civilization has visited us, could imply that no other advanced civilizations exist within it. We are given some plausible reasons why this might not be so! There follow three appendices: about the Drake Equation, what form advanced life might take, and the fact that we could live only in a three-dimensional universe.

If only we had more accurate information about the factors within the Drake Equation that determine how likely it is that other advanced civilizations exist we would perhaps get a better feel as to whether interstellar communication is more than just a faint possibility. Within a year or so the *Kepler* mission, referred to in the book, will tell us how often habitable planets exist within our Galaxy. But even if such planets are very common and could perhaps support intelligent life I worry that their governments would not fund the considerable cost of sending out signals for us to detect. But, as Sir Patrick Moore often says, "we just don't know", and Cocconi & Morrison ended the paper that inspired the SETI searches with the words, "the probability of success is hard to estimate, but if we never search the chance of success is zero".

Whatever the likelihood of success, the subject matter of this book strikes a chord with many people (interestingly, Ballesteros does consider if music can form a language) and this book will give its readers an up-to-date and insightful study of the many aspects of the subject. I recommend it highly. — IAN MORISON.

Foothold in the Heavens: The Seventies, by Ben Evans (Springer, Heidelberg), 2010. Pp. 533, 24 × 16·5 cm. Price £40·99/$44·95/€44·95 (paperback; ISBN 978 1 4419 6341 3).

In these financially-strained times, manned spaceflight seemingly has little innovative future. We now have to generate our excitement by re-living past exploits. And this is where Ben Evans has come to our aid. Evans is writing a five-volume history of human space exploration. The first volume (*Escaping the Bonds of Earth: The Fifties and the Sixties*, Springer, 2009) is now followed by a book that concentrates on the USA's Apollo era and the USSR's attempt to beat them to their lunar destination. On the American side we have a story of great success, with twelve men eventually walking on the lunar surface. Over in the USSR we are confronted with failure, a rocket that did not work, and an enforced change of goal — towards the establishment of a long-term presence in low Earth orbit. Politically we see a cold war that is gradually thawing; the United States and the Soviet Union becoming friendlier; and both their space programmes suffer due to loss of popular appeal and government funding. Excitement and innovation is gradually replaced by frugality, retrenchment, and disappointment.

This is a well-written, gripping, and engrossing story. Evans' account is also thorough, detailed, and knowledgeable. Anyone with even the vaguest interest in what was surely the greatest adventure of mankind in the last century will benefit hugely from reading this book. Evans has concentrated on the human side of space exploration. And the bravery, single-mindedness, and dedication of the astronauts and cosmonauts shine out from every page. — CAROLE STOTT.

Surveyor — Lunar Exploration Program, edited by R. Godwin (Apogee Books; and from Gazelle, Lancaster), 2010. Pp. 175 + CD ROM, 25·5 × 18 cm. Price £14·95 (paperback; ISBN 978 1 894959 65 0).

It now seems hard to believe, but there was a time when serious questions were being asked about the depth of the Moon's surface dust, the effects of the harsh lunar environment on spacecraft, and how easy or safe it would be to land humans on our neighbouring world. Many of these questions were answered by the US Surveyor missions, which prepared the way for the Apollo era of manned exploration.

This addition to the acclaimed series of space-related technical volumes published by Apogee Books details a short chapter of lunar exploration during which seven automated Surveyor spacecraft were dispatched to scattered landing sites between 1966 May and 1968 February. Five of the Surveyors successfully achieved soft landings and sent back a mass of images and other data about potential Apollo landing sites. One of them became the first spacecraft to take off from the surface and relocate a few metres away.

Most of the volume is taken up by original NASA outreach material on the Surveyor programme and the individual missions. More in-depth analysis is provided by a number of technical papers that describe investigations of the pieces of hardware and regolith brought back from the *Surveyor 3* landing site by the crew of *Apollo 12*. The book also contains some details of second- and third-generation designs, which were studied but never implemented.

Perhaps the most valuable part of the package is a CD-ROM which contains over 1800 pages of original materials related to the first three missions, plus a summary of the results from the Surveyor programme (NASA SP–184). It also contains a Surveyor slide show, which is, unfortunately, made up of rather blurred images with no captions or explanation.

The relevance of this material may seem obscure to those who were not alive during the pre-Apollo era, but this slim volume successfully summarizes the technical achievements and discoveries that were made by those hardy craft. Highly recommended for anyone who wants to know more about the first golden age of Moon exploration. — PETER BOND.

The Sky at Night, by Patrick Moore (Springer, Heidelberg), 2010. Pp. 184, 23·5 × 15 cm. Price £26·99/$29·95/€29·95) (paperback; ISBN 978 1 4419 6408 3).

This is the return of an old favourite. I still have three issues of the series published by the BBC about 40 years ago, and I had no idea that there were 12 previous volumes in all, but it seems that Philip's took up the baton after the BBC stopped publishing them, and now Springer are involved. This being the 13th, Sir Patrick is therefore somewhat cautious about the reception it might have but he has little need to worry. It is the formula as before — short summaries from a number (42 in this case — is this value significant?) of *Sky at Night* programmes. The earlier programmes were more one-man efforts and of late a number of co-presenters and helpers have been co-opted to keep this astronomy flagship programme going, but Sir Patrick's enthusiasm still lights up each page. It only needs a look at the list of those who have appeared on the programmes included here to realize that he still has his finger firmly on the pulse of popular astronomy in the UK. Although the date of each programme is no longer included, it is clear that this batch covers the period from 2005 to 2009 and in terms of space hardware it deals with the refurbishment of the *Hubble Space Telescope*, the launch of *Herschel*, the Mars landers, and the 40th anniversary of the Moon landings, but the variety of subjects discussed ranges from meteors and aurorae through the Solar System to the stellar neighbourhood and M 31 and on to gamma-ray bursters and dark energy.

There are a few minor spelling errors — usually people's names, which is not surprising given how many were involved in one way or another, but it is a little disappointing to be promised on page 88 a list of those who attended the 50th-birthday celebrations of the programme, only to discover that the list is not there, and neither is Patrick, contrary to the caption accompanying the photo on page 129! — ROBERT ARGYLE.

Choosing and Using a Refracting Telescope, by N. English (Springer, Heidelberg), 2010. Pp. 296, 23·5 × 15·5 cm. Price £35·99/$39·95/€39·95 (paperback; ISBN 978 1 4419 6402 1).

There was a time when telescope manufacturers and dealers were few and far between. Many amateur astronomers made their own instruments — mostly reflectors, as refractors were either very small or, if large, very expensive. Now, there is such a large range of equipment on the market that choice is difficult, especially for the beginner. It is therefore of particular value to have accessible a comprehensive guide to the availability and use of refractors, which have a special attraction and romance — especially for those who have used and appreciated the best of them. There is much to be gleaned from this volume. The reader is guided through the various aspects of achromats and apochromats of numerous types, with discussion of sizes, focal lengths, focal ratios, mounts, filters, and optical testing — all intertwined with judgements, opinions, and, most vividly, personal experiences and anecdotal stories by the author, plus many others contributed by colleagues and correspondents. The numerous illustrations include colour photographs of the *Great Refractor* designed and manufactured by Fraunhofer for the Dorpat Observatory in the 1820s, and a 9-inch Clark refractor dating from 1915. These alone are sufficient to draw attention to the superb engineering and optical quality of such instruments, and the advantages of the refractor for specific types of observation. The short but useful appendices include glossaries of terms and formulae, and data on a selection of double stars which can be used for testing refractors of various apertures. This book is therefore not just a guide for potential buyers and users, though with its wealth of information on currently available equipment, that is its primary purpose. It also includes a short chapter on imaging, but in essence it states the case for visual observing with refractors, the necessity of thorough familiarity with the equipment being used, and the rewards to be obtained from placing the eye at the eyepiece — all of which is underpinned by the author's unwavering enthusiasm. In these days of automated and remote telescopes and the decline of traditional observing skills, this is perhaps one of the most valuable aspects of this volume. Even for those with only the slightest interest, it would certainly inspire them. — R. A. MARRIOTT.

THESIS ABSTRACT

Tracer Populations in the Local Group

By Laura Watkins

So often in astronomy, an object is not considered for its individual merits, but for what we may learn from its properties regarding some larger population. The existence of dark matter is a prime example of this; we cannot see it directly but we can infer its presence by noting its effects on the stars orbiting within its potential. This thesis describes how various sets of tracer populations can be used to probe the properties of a variety of galaxies in the Local Group.

I begin by describing the extraction of a variable-star catalogue from the Sloan Digital Sky Survey Stripe-82 dataset and then use that catalogue to select a high-quality set of RR Lyrae stars. Analysing the distribution of the RR Lyraes reveals three significant substructures in the Milky Way halo: the Hercules–Aquila Cloud and the Sagittarius Stream, which were already known to exist, and the Pisces Overdensity, which was previously undetected. It is a faint, extended structure found at ~80 kpc and is of unknown origin. Altogether, I find that nearly 80% of the RR Lyraes are associated with substructures, consistent with the theory that galaxy haloes are predominantly, or even entirely, made up from disrupted satellites. I also investigate the density distribution of RR Lyraes in the halo, finding that it is best fitted by a broken-power-law model, in good agreement with previous work.

I go on to develop a set of tracer mass estimators that build on previous work which make use of actual (and not projected) distance and proper-motion data, reflecting the amount and quality of data now available to us. I show that proper-motion data are, in theory, very useful and can greatly increase the accuracy of the mass estimates; in practice, however, current analysis is hampered by the large errors inherent in the proper-motion data. The results are also subject to mass-anisotropy degeneracy, which current data are not yet able to break. Nevertheless, I am able to estimate the mass of the Milky Way to be $M = 2{\cdot}7 \pm 0{\cdot}5 \times 10^{12}\ M_\odot$ and the mass of M 31 to be $M = 1{\cdot}5 \pm 0{\cdot}4 \times 10^{12}\ M_\odot$.

Andromeda XII and Andromeda XIV are two M 31 satellites that have been dubbed "extreme" and are thought to be on first in-fall into the M 31 system. I modify the classical 'timing argument' so that it can be applied to two external galaxies and then apply it to M 31 and each of And XII and And XIV in turn to investigate the properties of their orbits. I then run a series of Monte Carlo simulations to investigate how likely it is that such satellites exist and conclude that they are not as unusual as previously believed.

Finally, I discuss three upcoming wide-field, all-sky surveys and their implications for the future of the study of the Local Group. — *University of Cambridge; accepted 2011 April.*

A full copy of this thesis can be requested from: laura.watkins@cantab.net

EDITORIAL

Subscription Prices

The Editors are pleased to announce that, for a further year — and in spite of severe inflationary pressures — there will be no price increase in the annual subscription for 2012. The price will remain £70 for institutions and libraries and £15 for individual subscribers within the UK. For all overseas subscribers (institutional and individual) there will be a surcharge of £5 which, for the first time in 2012, will be applied to *individuals* taking their copies directly (*i.e.*, not *via* the RAS, which has already applied that surcharge). The cost to subscribers, principally in the USA, who pay in US dollars, will remain $140 for institutions and $30 for individuals, since the airmail surcharge is already included in that price.

However, it is cautioned that a rise in the surcharge — and probably in the amount included in the basic price — will almost certainly be required for 2013 because of the annual, well-above-inflation price rises imposed by Royal Mail. For example, already in 2011, the cost of air-mailing a typical 100-gram magazine plus envelope is £1·49 to Europe and £2·07 to the 'Rest of the World'; *i.e.*, about £9 and £12·50 for the year, respectively. Readers may wonder why a cheaper form of bulk postage is not used; the answer is simply that it *was* used in the past and frequently copies went missing! In two instances, the entire shipment to the USA disappeared. The Royal Mail (and associated organizations in the receiving countries) has been found reliable and speedy, as we hope readers will have noticed in recent years.

Here and There

MAXIMUM EASTERN ELONGATION

Venus, the brightest object in the night sky except for the full Moon, rises about 5pm. — *The Daily Telegraph*, April Night Sky, 2011.

NOT CAREFUL ENOUGH

Thanks to — for a carefull reading of the manuscript. — *AN*, **332**, 215, 2011.