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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2014 November 14 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

M. A. Barstow, *President*
in the Chair

The President. Hello and welcome. I would like to notify you that the National Astronomy Meeting 2015 will be held at Venue Cymru in Llandudno, from 2015 July 5–9. The reason we've gone to Wales again is the difficulty in finding a suitable location within a university. I would like to encourage strongly those of you at universities to think about hosting NAM in the future, as it would be nice to move around the UK, especially to some more-remote areas.

On to the programme: I am pleased to tell you that we will be hearing from two thesis prize winners, as well as having the Harold Jeffreys Lecture. The first of them is the Keith Runcorn thesis prize winner, Dr. Richard Walters from the University of Leeds: 'Continental deformation and seismic hazard across the Middle East: a satellite's view'. Before he starts, I am going to embarrass him by handing him the certificate. Congratulations. [Applause.]

Dr. R. Walters. First of all, thank you very much for this award and for the invitation to speak today; it's a real honour to be here. I'm going to talk to you about how satellite-based measurements can help improve our understanding of seismic hazard and tectonic deformation.

Whilst oceanic plates behave in accordance with plate-tectonic theory, with a lack of internal deformation and focussed strain and seismicity at narrow plate boundaries, this theory doesn't hold at the boundaries between continental plates, where deformation and seismicity is diffuse and can span thousands of kilometres, approaching the size of plates themselves. It's important to study these diffuse regions of continental deformation for two reasons. First, because the mechanics of how the continents deform isn't very well understood, and second because the spatial distribution of seismic hazard is in general much less well known in the continents, which leads to a disproportionately high number of fatalities due to earthquakes. My PhD focussed on using satellite-based geodetic data to address both these points: to understand how continental lithosphere deforms and to characterize seismic hazard, with the central

Alpine–Himalayan mountain belt (Iran and Turkey) as the geographic focus. This region experiences relatively fast rates of continental deformation and has large urban populations at risk from high seismic hazard.

My research mainly utilizes two types of geodetic data: repeated GPS and InSAR (satellite radar) measurements which can be used to measure millimetric-to-centimetric-scale tectonic movements of the Earth's crust. InSAR can provide maps of this movement at high spatial resolution, *i.e.*, tens of metres, over regions hundreds of kilometres across. InSAR is often used to measure the permanent coseismic ground deformation that takes place due to earthquakes, but can also be used to measure the slow elastic warping of the crust that takes place in the years-to-centuries between earthquakes, and this 'interseismic deformation' has been the focus of my work. Essentially we can use this as a proxy for seismic hazard, as this elastic energy builds up and is released in earthquakes.

I used InSAR data in this way over eastern Turkey to measure the interseismic deformation across a large 500-km by 400-km area — about the same area as England and Wales. It was necessary for me to combine a huge amount of data in order to measure this tectonic warping, which takes place at a rate of only 1–2 cm per year, as this signal is typically much smaller than the noise inherent in the data. I used these data to calculate slip rates across the two major faults in this area — the East and North Anatolian Faults — but also combined these data with existing GPS velocities to measure 3-D deformation there for the first time. I found that there is no vertical motion across the Anatolian Plateau, and also demonstrated that it is possible to estimate 2-D tectonic strain from InSAR alone, even without complementary GPS measurements. However, I also found that whilst strain is localized on the two major faults in eastern Turkey, the region between them, Anatolia, shows significant deviations from the rigid and 'block-like' behaviour that has previously been proposed for the area.

My research also involves the use of numerical models to try to understand the mechanics of continental deformation on a country-wide scale. East of Turkey, Iran shows a complex pattern of deformation from the distribution of seismicity and mapped faults, and one of the key features of this pattern is that central Iran has a lack of earthquakes and is not deforming. Previous attempts to model the Iranian deformation field suggested that central Iran had to be a strong, rigid block in order to explain its lack of deformation. However, in the last ten years or so since these studies, a huge wealth of GPS data has been collected from across Iran by many researchers. These crustal velocities can now be used to test such models for the first time.

I model the Iranian lithosphere as a thin viscous sheet, which deforms as a creeping viscous fluid in response both to lateral boundary forces and to internal buoyancy forces, which arise from spatial variations in gravitational potential energy. I chose appropriate boundary conditions from plate motions of Eurasia to the north, and Arabia to the south, and let geoid height control the distribution of gravitational potential energy across Iran. I varied only two parameters to best-fit the GPS velocity field across Iran. These parameters are the power-law exponent of the viscous fluid and the Argand number, a non-dimensional quantity which determines the relative importance of buoyancy forces and stresses required to deform the lithosphere. I found that by including buoyancy forces I could produce a remarkable fit to the crustal velocity field across Iran, including reproducing a non-deforming central Iran. So this work shows that central Iran does not have to be stronger than its surroundings, and instead its lack of deformation can be explained due to the interaction of buoyancy and boundary forces. I also find that this model replicates observed patterns of the different types of seismicity across Iran.

I have also developed a novel approach to predict fault slip rates from this model. By assuming that the motion of the viscous lower lithosphere drives the motion of overlying crustal blocks, I can calculate seismic hazard across a simplified set of faults, using my model velocity field as input. Even with such a grossly simplified fault map, there is a very good fit between my estimated slip rates and those observed from thousand-to-million-year geological estimates. So I suggest that this may be a useful method to create a dynamic, self-consistent model that can estimate seismic hazard even in regions without dense GPS data.

Finally I want to look to the future of tectonic geodesy. In April this year the European Space Agency launched *Sentinel-1A*, the first of two new InSAR satellites. This 20-year operational programme will provide a radical increase in radar data quality and quantity and lead to a huge improvement in our ability to measure slow rates of tectonic deformation on a global scale. So I'd suggest that the combination of high-resolution crustal-velocity measurements on a global scale, and numerical modelling of these data, will revolutionize our understanding both of continental seismic hazard and of the mechanics of continental deformation.

The President. We have time for a few questions.

Ms. Tracey Olsen. I know there was a very severe earthquake in April, AD 33, that completely flattened Lycia. It also blocked out the Sun; do you know about that?

Dr. Walters. I don't know about that particular earthquake.

Ms. Olsen. If you have an earthquake, then could that be associated with volcanic activity?

Dr. Walters. Earthquakes can trigger volcanic activity. That has been known to happen. You wouldn't expect any blocking out of the Sun otherwise.

Professor S. Balbus. Just at a purely kinematic level, you have maps of the divergence and vorticity of these velocity fields. You looked at a dynamical model but it might be interesting to look at inclinations of rotational stresses and compressions.

Dr. Walters. Yes, that would be well worth having a look at. The block model is essentially fitting simple rotations to the dataset.

Professor Balbus. When you say 'simple rotations', do you mean 'not differential' rotations?

Dr. Walters. I mean bulk rotations of one of those whole blocks relative to the reference frame for the GPS data.

Mr. J. C. Taylor. You're averaging in your dynamic model over the vertical. In other words, there's no z in your equations. Is that a serious shortcoming? Is there anything you can do about it?

Dr. Walters. In most cases, it has been shown that this is a pretty good approximation. Full 3-D models have been run for validation, and in regions where the length-scale of deformation is much greater than lithospheric thickness, 2-D models are a good approximation.

Mr. Taylor. Presumably there is a problem getting the data in the first place, in terms of vertical changes?

Dr. Walters. Yes. Where you get significant shear at depths, like at a subduction zone, that's when these shears are non-negligible.

Mr. M. F. Osmaston. How can you — or can you — distinguish between rheological deformation and stress build-up during the interseismic period?

Dr. Walters. I showed a cartoon of the coseismic and interseismic, but after an earthquake the sudden slip on the fault also introduces a postseismic, transient effect. But if we're measuring deformation long enough after the last earthquake we expect to measure just the stress build-up.

The President. I think we need to move on so let's thank our speaker again. [Applause.] The Michael Penston Prize is awarded to Dr. Joseph Elliston for his thesis completed at Queen Mary, University of London, on 'Observational predictions of generalized inflationary scenarios'. I have a certificate for you as well — congratulations. [Applause.]

Dr. J. Elliston. Inflation is an epoch of rapidly accelerating expansion in the very early history of the Universe. The ultimate goal of inflationary cosmology is to discover the correct model that describes this early rapid expansion while accounting for the Universe we observe today. In particular, any viable model of inflation must account for the subtle variations in density that we observe in the Cosmic Microwave Background (CMB).

So the route forward appears obvious: we calculate what the CMB would look like for every possible model of inflation, toss away all of those models that are incompatible with the data, and as the data gets more precise we'll eventually single out the correct model. If only it were that simple! Unfortunately, present analytical tools are only able to calculate how the CMB would look for simple models of inflation. The need for simplicity is problematic because the result of many years of research into high-energy theories such as String Theory is that inflation should be anything but simple — in fact we should expect inflation to be driven by some complicated model involving multiple degrees of freedom. A key task today is therefore to improve our analytical tools so that we can tackle more-complex models of inflation and show what they predict the CMB would look like. This talk is about my work with David Seery and Reza Tavakol where we make an important step in this direction.

We begin by taking the *simplest* model of inflation that involves multiple scalar fields. This model is sufficiently simple that its CMB predictions can already be calculated, but the price that we pay for this simplicity is that the extra richness demanded by current high-energy theories is absent. We take a step in that direction by adding an additional level of complexity to the *simplest* model which is motivated by high-energy-candidate theories, and then derive the new analytical tools needed to work out what this *next-but-simplest* model would predict for the CMB.

To describe the difference between the *simplest* and the *next-but-simplest* models of inflation, I employ an analogy from classical mechanics. In this analogy let us replace the inflationary fields with particles of unit mass, and label the velocity of the i-th particle as v_i. In this picture the *simplest* model is equivalent to standard classical mechanics, where the kinetic energy of the whole system is equal to the sum of kinetic energies of the individual particles, $\frac{1}{2}\Sigma_i v_i^2$. The *next-but-simplest* model, by contrast, has a more complex form for the kinetic energy, which not only includes the product of each particle's velocity with itself, but also products of each particle's velocity with every other particle's velocity. This leads to a kinetic energy of the rather less-familiar form $\frac{1}{2}\Sigma_{ij} G_{ij} v_i v_j$, where G_{ij} is some matrix that determines how much of each velocity is multiplied into each other velocity. It is the presence of this non-diagonal matrix G_{ij} that requires us to develop new tools to understand this type of model.

As an important aside, this *next-but-simplest* model is also of interest in current studies of modified theories of gravity, regarding attempts at understanding the observed late acceleration of the Universe. Research in this area involves looking for possible deviations from Einstein's General Theory of Relativity, and some of the best-studied alternatives are the so-called $f(R)$ theories. It can be shown that these $f(R)$ theories are equivalent to the *next-but-simplest* model that we are considering. Therefore, the tools that we have developed may have important applications for modified-gravity theories.

Working out how the CMB would appear for the *next-but-simplest* model requires us to perturb this model. Even the most basic perturbation calculation requires us to take second-order derivatives of the kinetic energy, and because this includes the matrix G_{ij} then one gets a slew of terms in the resulting formulae involving derivatives of G_{ij} that were not present when working with the *simplest* model. The principal problem with these extra terms is that they make it increasingly difficult to understand the physical meaning of the mathematical results.

To avoid this problem we adopt a new perturbation scheme devised by Gong and Tanaka which works by exploiting a clever geometrical trick. Instead of thinking of G_{ij} as simply a matrix, one can imagine a *field-space* in which the scalar fields live, and assign G_{ij} to be the metric on this *field-space*. This therefore means that there are two different metrics involved in our calculations: the metric of the physical space–time and metric of this *field-space* G_{ij}. The benefit of making this abstraction is that one can perturb in a different way, taking covariant (rather than ordinary) derivatives of the kinetic energy such that no terms involving derivatives of the metric G_{ij} are ever produced. Instead, one is left with short equations that can be intuitively interrogated. For example, these results show that the evolution of perturbations of the scalar fields undergo geodesics deviation, which is the familiar physical mechanism by which initially parallel rays of light can be made to deviate by the curvature of the space that they are travelling through.

The result of our work is a complete set of tools which are intuitive and allow the calculation of perturbation predictions for these *next-but-simplest* models. In contrast to the *simplest* models, we have shown that these models can result in new features in the higher-order statistics of the CMB. They can also result in new signatures in the time-evolution of these perturbations. These results will enable future galaxy surveys or CMB data to be used more precisely to pin down the underlying model behind inflation.

The President. We have time for a few questions.

Professor I. Steele. Is the use of the manifold simply a computational device or does it have any physical, intuitive interpretation?

Dr. Elliston. It's a computational aid in that you arrive at the same equation at the end. The three-point-function formula that I showed on the last slide was admittedly very complicated, but it certainly wouldn't have fit on one slide if I'd used the conventional technique. It wouldn't have been clear that various terms cancel so easily. It's absolutely legitimate to use conventional perturbation theory and not introduce the concept of the field-space manifold, but if you do take that conceptual step, it makes your life easier as you have a lot less stuff to write down. So we can still solve the same problem, but we can solve it a lot more easily using this technology.

Rev. G. Barber. For the last line in your last slide, how do these theories compare to observations? How well do they confront the observations and are there actually observations to confront?

Dr. Elliston. That's a very good question. The principal observations come from the CMB. If you take an inflationary model, then there are lots of parameters to try to fit and there aren't that many in the data, so it's pretty difficult to pin these model parameters down. The non-Gaussianity constraints only provide null constraints, so you can rule out parameter space but you can't use it the other way around.

The President. Let's thank our speaker again. [Applause.]

Our last, but not least, talk today is the Harold Jeffreys Lecture, which will be given by Professor Alex Halliday of Oxford, on 'The origin of the Earth and Moon'. Professor Halliday is a geochemist who specializes in the determination of isotope abundances in terrestrial and planetary materials, including samples from the Moon, Mars, and asteroids. He is a world leader in his field, and has been instrumental in the development of new analytical techniques to investigate isotope systems that were previously poorly understood.

His research topics range from the timing and nature of the origin of the Earth–Moon system, to the sources and evolution of different geochemical reserves in Solar System bodies. Such efforts have important implications for the history of the Earth, understanding for conditions for the onset of life in the Solar System, and have implications for the likelihood of finding Earth-like planets amongst the numerous exoplanetary systems.

On top of these outstanding research contributions, Professor Halliday is an excellent public speaker who communicates his science very well to both the astronomy and geophysics communities. He was recently the main organizer of the 2013 'Origin of the Moon' meeting at the Royal Society, which generated widespread media interest about understanding our planet's origins and early evolution. Professor Halliday always presents his cutting-edge science in a very accessible and entertaining manner. For these reasons Professor Halliday is awarded the 2014 Harold Jeffreys Lectureship. [Applause.]

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

The President. Sadly we've gone past 6 o'clock but it was such a good talk I think we should have at least one question.

Dr. G. Q. G. Stanley. When you showed the model of Theia impacting Earth, Theia seemed to be totally destroyed. If we had a wet Earth at that stage, would that modify the Moon's formation and would we find a signature in the Moon of that?

Professor Halliday. Firstly, a large amount of material would have been vaporized, so there would be a silicon atmosphere as well as water. There are now data being produced by secondary-ion mass-spectrometry of glass beads from the lunar surface which appear to show evidence of relatively high water content. It looks like the water in them is not so different from that of the water in the Earth's mantle today. There is a whole story around water that's completely unexplained. The deuterium-to-hydrogen ratio of that water is slightly heavy relative to the Earth's as if there's been a large amount of water lost — nothing like as different as cometary water is. The loss is consistent with some kind of hydrogen-ion escape process from the atmosphere when that happened. I'm not sure if that actually answers your question?

Dr. Stanley. It does, because you are saying there is a signature in the Moon.

Professor Halliday. There is, but it's poorly understood and quite controversial.

The President. Let's thank our speakers again for such a stimulating session. [Applause.] There is now a drinks reception in the RAS library. The next A&G meeting of the society will be on 2014 December 12.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2015 January 9 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

A. M. CRUISE, *Treasurer*
in the Chair

The Chairman. Friends and colleagues, a Happy New Year to you all. I start with what is an important part of the RAS activities, the announcement of the 2015 awards. The awards begin with the Gold Medals: the astronomy medal is presented to Professor Michel Mayor of the Observatoire de Genève and the geophysics award is presented to Professor Mike Lockwood of Reading University. The Chapman Medal is awarded to Professor Alan Hood of St. Andrews University. The Eddington Medal is presented to Professor Rashid Sunyaev of the Max Planck Institute for Astrophysics. The Herschel Medal goes to Professor Stephen Eales of Cardiff University. The Price Medal is presented to Professor John Brodholt of UCL. The Jackson-Gwilt Medal is this year being awarded in the area of history of astronomy, and I think it will come as no surprise to anyone that it has been awarded to Dr. Allan Chapman. The Patrick Moore Medal goes to Sarah Llewellyn-Davies of the Castell Alun High School. The Fowler Awards go to Dr. Haley Gomez (A) of Cardiff University and Dr. Catherine Rychert (G) of Southampton University. The Winton Capital Awards go to Dr. Michal Michalowski (A) of Edinburgh University and Dr. Richard Morton (G) of Northumbria University. The Group Achievement Awards go to the *e-MERLIN* team (A), led by Professor Simon Garrington of Manchester University; and the *Hinode Extreme Ultraviolet Imaging Spectrometer* team (G), led by Professor Louise Harra of UCL. The RAS Service Award goes to Professor Lidia van Driel-Gesztelyi of UCL. Honorary Fellowships go to Professor Fiona Harrison of the California Institute of Technology, Dr. Alan Stern of Southwest Research Institute, Dr. Asahiko Taira of the Japan Agency for Marine–Earth Science and Technology, and Dr. Janet Luhmann of Berkeley. The George Darwin Lecturer is to be Professor Katherine Blundell of the University of Oxford; the Harold Jeffreys Lecturer will be Professor Tony Watts of the University of Oxford; and the James Dungey Lecturer will be Dr. Helen Mason of Cambridge University.

We move on straight away to Professor Tom Ray, of the Dublin Institute for Advanced Studies, who will be speaking on 'The Einstein lens and a tale of two eclipses'.

Professor T. Ray. Ladies and gentlemen, thank you very much for this opportunity to tell you how equipment from Howard Grubb, the well-known Irish telescope maker, contributed to verifying the General Theory of Relativity. Grubb, as some of you will know, manufactured telescopes for around the world, including the *Great Melbourne Telescope*, several instruments for the Royal Observatory at Greenwich, and the *Great Vienna Refractor*. In its heyday, Grubb employed some 400 people at the firm's works in Rathmines, Dublin.

In 1900 Grubb, and Charles Jasper Joly, then at Dunsink Observatory, led an exhibition to Plascenia, Spain, to view the total solar eclipse of May 28. Their intention was to photograph the corona and to record it spectroscopically. With the expedition in mind, Grubb made specially two coelostats and a long-focal-length 4-inch lens. This equipment, which was paid for by the Royal Irish Academy and the Royal Dublin Society, produced some excellent images for the time. Moreover, by combining photographs from Plascenia with those from other locations on the eclipse path, including Georgia and North Carolina in the US, we can see that the Sun underwent a Coronal Mass Ejection (CME) around 13:45 UT on that day. This is probably the first photographically recorded CME.

If we then fast-forward 17 years, Frank Dyson, then Astronomer Royal, proposed in a short paper to the editors of *Monthly Notices* the idea of using the forthcoming eclipse on 1919 May 29 to test Einstein's General Theory of Relativity. As many of you will know, Arthur Stanley Eddington led one of the eclipse expeditions, to the island of Príncipe off the West Coast of Africa. Andrew Crommelin, from the Royal Observatory at Greenwich, led the other expedition to Sobral in Brazil. Crommelin, originally from County Antrim, brought with him one of the coelostats and the 4-inch lens owned by the Royal Irish Academy as backup equipment. Intermittent cloud plagued Eddington's expedition although Eddington did manage to get some interesting photographs of a prominence emerging from the Sun and a few faint stellar images. Crommelin was much luckier as regards the weather although his primary equipment for observing the eclipse did not function properly on the day, probably as a result of expansion caused by heat. The Grubb 4-inch lens and 8-inch coelostat, however, proved up to the task and gave results that in the words of Dyson, Eddington and Davidson, had to be assigned the highest weight. The so-called 'Einstein Effect' had been verified, and of course the rest of the story is well-known to you.

I am very happy to tell you today that after working through archives in the Royal Astronomical Society (and here I must thank the Society's Librarian Jennifer Higham for her help), Royal Society, and Royal Irish Academy, I have uncovered the Grubb coelostat and 4-inch lens used in Sobral, despite the fact that they have been lost for almost 70 years. The equipment has recently been restored and will go on exhibition in time for the centenary of General Relativity in 2016. There is, however, a final very interesting twist to this story.

As many of you will know, the importance of the eclipse of 1919 May, and stressed by Dyson and Eddington, was that the Sun was in the Hyades and hence surrounded by a relatively large number of bright stars for measurement purposes. You may have noticed that the date for the eclipse when the Royal Irish Academy equipment was first used was May 28, *versus* May 29 for its subsequent use for the deflection-of-light expedition in Brazil. If, however, one factors in the leap-year effect, whereby, for example, the solstices move forward by slightly over a quarter day per annum, and the fact that one eclipse (Plascenia) occurred just after 16:00 UT while the other (Sobral) occurred around 12:00 UT, by amazing coincidence the Sun was in almost exactly the same part of the sky for both eclipses within a couple of arcminutes! Thus the same equipment had been used to photograph essentially the same eclipse on both occasions. Eclipse photographs from 1900, for example, show the same stars as in plates from 1919. This is illustrated by some plates from the Chabot Observatory near San Francisco.

This begs the obvious question: could Dyson and others have used historical photographs, such as those from 1900, to measure the gravitational deflection of light and not bother with the 1919 expeditions? In fact Erwin Freundlich, a friend of Einstein, wrote to Dyson expressing this idea around the time of the outbreak of the Great War and asked him if the Royal Observatory at Greenwich had suitable material. But this was before the full General Theory of Relativity had been developed and Freundlich was hoping to measure the so-called Newtonian deflection (*i.e.*, half the value of what Einstein subsequently predicted as the full deflection). Dyson in fact tried to make measurements on some old (1905) eclipse plates from Sfax in Tunisia as noted in the same *Monthly Notices* paper suggesting the use of the 1919 May 29 eclipse. The situation was, however, hopeless as he could only see two stars on the plate, although this did not prevent him from declaring the method sound! Interestingly he never mentions the 1900 eclipse plates in his possession (one of a set of two that he could have worked with, excluding the Sfax plates). While we can never be 100% certain, in a paper in preparation I describe how he may have conveniently 'forgotten' the 1900 plates, which he himself obtained in Ovar in Portugal, to ensure Eddington avoided being sent off to war.

It is well known that Eddington was a conscientious objector. It has also been claimed, for example by Chandrasekhar, that the primary reason why Eddington was chosen to lead the eclipse expedition was to avoid conscription. When hauled up in front of a military tribunal in 1918, Eddington claimed that it was crucial that he test Einstein's Theory and that there would not be another suitable eclipse in the Hyades, like that of 1919 May 29, for hundreds of years. While this is correct, he failed to mention the one 18 years before. Moreover Dyson, who wrote to the military tribunal asking for Eddington to be excused conscription, never mentioned the set of 1900 eclipse plates in his possession. Eddington was excused military service on the grounds of the importance of his work.

The Chairman. Any questions?

Dr. R. C. Smith. It seems like there is an obvious question: has anybody looked to see if the 1900 plates can be used to detect the deflection?

Professor Ray. The answer is 'yes': I have and I am in the process of making the measurements now. I, however, have the advantage of modern software, *etc.*, and I have no intention of using the old machinery they used! It is also worth pointing out that there were accurate proper motions for the Hyades by 1918 so comparison plates were possible.

Professor S. Miller. I'm glad you showed the bit from *The Times*, which was fantastic in 1919. On November 21, Einstein put a signed article in *The Times*. There is a lovely bit where he said, "Now that my theories have been proved correct, in England I'm known as a Swiss Jew and in Germany as a German man of science. If in future, my theories are proved incorrect, by application of the theory of relativity, in England I will be known as a German man of science and in Germany as a Swiss Jew." [Laughter.]

A Fellow. I just wanted to advertise the 2017 eclipse, which can be seen across continental US and has Regulus in the field.

Professor Ray. There have actually been very few subsequent eclipse measurements of the gravitational deflection of light. There were some made in Australia confirming Eddington's result. In fact, the best measurements are done at radio wavelengths using VLBI.

A Fellow. Maybe I missed it, what was the name of the man who led the expedition to Sobral?

Professor Ray. Andrew Crommelin.

The Fellow. Is his name on the paper?

Professor Ray. No.

The Fellow. Why is that?

Professor Ray. I don't know. I had thought about it, but I'm worried I'm getting too much into conspiracy theories! [Laughter.]

Mr. H. Regnart. A little historical point: as a Northumbrian with Irish ancestry, people might be interested to know that Sir Charles Parsons was a son of Lord Rosse. Having set up his steam-turbine business in Tyneside, in due course he brought Grubb's business to Tyneside where it ran successfully for many years until unfortunately the orders ran out. He hadn't commercialized into smaller-scale mass-market equipment which might have kept it going.

Professor Ray. What killed Grubb in Dublin, and why the firm had to be bought over by Sir Charles Parsons, was that prior to the Great War, Grubb had made commitments through several major contracts. There was incredible inflation during the Great War which led to his being unable to complete those contracts when the war was over. As an aside, and something that I didn't mention, Howard Grubb was also the inventor of the submarine periscope. It's hard to think that until his invention, the first submarines were blind when underwater!

Dr. L. Cox. I haven't looked at this for some time but I have a memory that during the 1919 expedition, they took plates away from the eclipse so they could they could get the star field without the Sun. This was to help cope with the scale factor. I wonder whether it would be possible to have got a sufficient measurement without the ability to take out scale factors?

Professor Ray. They had all the same equipment so they could have determined the scale factors by just photographing the same star fields. A more important issue is knowing the proper motions of your target stars but, as I said, these were well known by then.

Professor D. Lynden-Bell. I just wanted to make the remark that Andrew Murray re-measured the plates at the RGO and the original measurements were substantiated very well.

The Chairman. Thank you very much to our speaker again. [Applause.] We come now to the George Darwin Lecture. I shall read the citation.

Professor James Dunlop of the University of Edinburgh has played a leading role in transforming our understanding of how galaxies form. He has pioneered new fields of study and then established them as mature areas of research, often by leading major new observational programmes. The first systematic study of quasar host galaxies was carried out by Professor Dunlop, and he went on to discover that their basic properties are indistinguishable from their inactive counterparts.

He has demonstrated that the most massive radio galaxies and black holes formed before most of their lower-mass counterparts, an effect known as 'downsizing'. Through his leadership on age-dating galaxies, he provided the first evidence that massive galaxies formed at redshifts greater than 5. He took sub-millimetre astronomy from its infancy, through developments in instrumentation with the *SCUBA* camera on the *James Clerk Maxwell Telescope* in Hawaii, to establish the basic properties of star-forming galaxies shrouded in cosmic dust, and has played key roles in studying the formation of the very first galaxies. For these reasons Professor Dunlop is awarded the 2014 George Darwin Lecture. [Applause.] His talk is entitled, 'The cosmic history of star formation'.

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

The Chairman. An excellent talk — any questions?

Rev. G. Barber. Is there any handle on the masses of these very-high-redshift galaxies and the connection with star formation and black-hole formation?

Professor Dunlop. Yes: there's one quasar known at redshift 7, so there are massive objects at high redshift but they're rare. Of the galaxies that you see in *HST* images at redshift 7, the most massive ones are approximately $10^9\ M_\odot$; that's not a fundamental mass limit, that's just from a small area of sky. I didn't talk about *UltraVISTA*, which is mapping a couple of square degrees now at redshift 7, and has rarer, brighter objects. There are also galaxies at redshift 10 and a few at redshift 7 that have masses of a few times $10^{10}\ M_\odot$.

Once you go to the quasar surveys like *Sloan*, which have covered large areas, you can see even rarer objects. We know, for example, that the brightest quasars at redshift 7 have a 10^9-$M_\odot$ black hole — not many of them do but the mass function does extend out to very massive objects. One of the challenges is how do you get any 10^9-$M_\odot$ black holes by redshift 7 (less than a billion years since the Big Bang)? If you can seed with a few-hundred-solar-mass black holes at redshift 10 to 15, you can just make it with Eddington-limited accretion.

There are other arguments that perhaps black holes got off to a kick start. Maybe one star-forming galaxy heats up a neighbouring halo, preventing it fragmenting into stars. There are deep X-ray images of, for example, the Ultra Deep Field but there are no X-ray detections of any of these redshift-7 or -8 galaxies. All that's saying is there's not a black hole more massive than about $10^7\ M_\odot$. X-ray astronomy has got a big job to do catching up to see AGN at these epochs. Despite lots of effort, nobody has found another quasar over redshift 7; it may be that there are no quasars of that mass out at redshift 8 or 9. For all the stellar-mass estimates at the moment you just see the UV, and for a few objects you get some handle on the optical from *Spitzer* and you fit a function. They're only good to a factor of a few. With *JWST*, when we can see the near infrared, we will get stellar-mass estimates that are good to 10%.

Professor Ray. Will *MIRI* on *JWST* be useful?

Professor Dunlop. To remind us, there are three instruments on *JWST*: there's *NIRCam*, which goes from the near infrared out to about 5 μm; there's *NIRSpec*, which also goes to about 5 μm; and *MIRI*, which does the longer wavelengths. It should be very exciting because you could do Hα at very high redshifts. At the moment it looks like the spectroscopy on *MIRI* might be limited for what I'm talking about to very bright high-redshift galaxies. What we will be able to do is spectroscopy with *NIRSpec* and photometry above the Balmer break with *MIRI* to get really good ages and break the notorious dust–metallicity degeneracies. I think in the high-redshift régime, *MIRI* will do what *Spitzer* does for us now at much higher redshift. With *JWST* pushing the imaging to ridiculously faint levels, I think the really good spectroscopy will come from *NIRSpec* and the photometry from *MIRI*.

The Chairman. Let's thank our speaker again. [Applause.] I remind you of the drinks reception in the RAS library. I give notice that the next monthly A&G meeting of the Society will be on Friday, 2015 February 13.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2015 February 13 at 16^{h} 00^{m}
in the Geological Society Lecture Theatre, Burlington House

M. A. BARSTOW, *President*
in the Chair

The President. Hello and welcome. We are currently in the process of revising the byelaws of the Society. As the byelaws currently exist, they don't really serve the purposes of the Society in the 21st Century. The Society as an organization is defined by its Royal Charter at the highest level, but the byelaws deal with the operational matters. We've lagged badly behind in this regard, so we have reviewed the byelaws, kept the ones we still need and modified them to include appropriate language. Everybody should have received them electronically. If you don't have an email address we will send you a hard copy. There are also copies at the front of the lecture theatre that you can pick up this afternoon. There is no excuse not to spend a happy few hours reading your way through them [laughter]. We would like to get feedback before we conclude the process at the AGM in May. By publishing the proposed changes and giving everybody the opportunity to read through them, we aim to get your feedback in time to make any changes before they are tabled at the AGM. The deadline for this feedback is 2015 March 8.

After wrestling with some technical issues, we have now made our meetings available online. So if you wish to catch up with our excellent talks, you will be able to get them on the RAS website.

Finally, on to the programme. I am delighted to welcome our group-achievement award winner for the *Herschel–SPIRE* instrument, led by Professor Matt Griffin: 'The *Herschel–SPIRE* instrument and its scientific legacy'.

Professor M. Griffin. About half of the electromagnetic energy that has been produced by astrophysical processes since the Big Bang is in the far infrared and submillimetre region of the spectrum (wavelengths between around 50 μm to 1 mm). This is because much of the radiation produced at shorter wavelengths from luminous young stars, and from accretion of material onto protostars and onto black holes at the centres of galaxies, is absorbed by surrounding clouds of gas and dust. Stars can form inside such clouds, and from the early stages until the emergence of a fully-formed star, no visible light emerges. But the embedded protostars heat the surrounding cloud material to temperatures of a few tens of degrees above absolute zero, and the absorbed energy is re-radiated at longer wavelengths. Thermal emission from the dust results in a broad continuum, and radiation from the gas is in the form of spectral lines characteristic of the chemical composition and the physical conditions. This 'reprocessing' of stellar and accretion energy means that most of the energy output of galaxies during their most active periods of star formation has been in the far infrared. For the same reason, in studying obscured star formation going on today in our own or nearby galaxies, we need to observe at these wavelengths. Towards the end of their lives, stars also eject large quantities of material into interstellar space, including heavy elements that have been synthesized in the star. So to investigate the final stages of stellar evolution and the chemical evolution of the galaxy and the Universe, we also need to observe in the far infrared and submillimetre.

The European Space Agency's *Herschel Space Observatory* was designed to open up the far infrared and submillimetre spectral region for sensitive and systematic study. Launched in 2009 May, it operated until 2013 April, observing between 55 and 670 μm. *Herschel* carried a passively-cooled 3·5-m diameter telescope and three scientific instruments, *HIFI*, *PACS*, and *SPIRE*. All three instruments needed cryogenic temperatures for their operation, and their focal-plane units were cooled by a 2200-litre liquid-helium cryostat, which provided a lifetime of nearly four years before the helium ran out. Cryogenic satellites such as *Herschel* are best placed far from Earth, which would otherwise pose severe thermal problems, acting as a huge thermal radiator next to the satellite. *Herschel* was therefore placed in orbit around L2, the second Lagrange point of the Sun–Earth system, approximately 1·5 million km further out from the Sun than Earth, with the Sun, Earth, and L2 in a straight line. A satellite near L2 orbits the Sun at the same angular rate as does Earth, always keeping the same position with respect to Earth and Sun, thus providing a highly stable thermal environment.

The spacecraft and instruments were designed and built by very large teams across Europe and beyond. Few single countries have access to all the technical expertise and financial resources needed for such large and complex space projects. Naturally, there are many technical and managerial challenges with large and multifaceted international collaborations, but it is to the credit of the international scientific community that it has developed the culture and methodology to meet such challenges and collaborate effectively to bring about the advanced scientific capabilities afforded by large-scale facilities for astronomy and fundamental physics. For example, the *SPIRE* instrument, which was led by the UK, was provided by six institutes in the UK (Cardiff University, STFC–RAL, Imperial College, UCL–MSSL, UKATC, University of Sussex) and twelve others in Canada, China, France, Italy, Spain, Sweden, and the USA. *SPIRE* was funded by PPARC/STFC and UKSA in the UK, and by the counterpart space agencies and institutes in the other participating countries. Detailed design and construction started in 1998, and over the years since then hundreds of scientists, engineers, and managers have worked on the project. It is a great privilege and honour to be able to speak on their behalf and to report on the fruits of their talents and hard work.

After nearly a decade of development, the *SPIRE* instrument was finally delivered for integration into the *Herschel* spacecraft in 2007. Like the other *Herschel* instrument consortia, the *SPIRE* team also provided its Instrument Control Centre, responsible for data-processing software, calibration, and instrument-operations support. From the start of the project, in parallel with hardware development, substantial and sustained efforts were devoted to developing the data pipelines to ensure the best possible data quality early in the mission.

SPIRE had a three-band camera, operating simultaneously at 250, 350, and 500 μm (complementing the *PACS* camera which covered 70, 100, and 160 μm), and an imaging Fourier-transform spectrometer covering 194–670 μm (also complementing the *PACS* spectrometer which had a wavelength range of 55–210 μm). An internal ^{3}He refrigerator cooled *SPIRE*'s arrays of bolometric detectors to their operating temperature of 0·3 K. Instrument control and data handling were provided by warm analogue and digital electronics units located on the spacecraft service module.

The range of astrophysics research that *Herschel* and *SPIRE* have enabled is far too extensive to review here, but a few examples are given as illustrations. The multi-band images made by *SPIRE* and its sister instrument *PACS* probe the interstellar material from which stars can form. Such material does not show up in shorter-wavelength optical/IR images, which are sensitive to stars that have already formed. Molecular clouds in the Milky Way, which are apparent in optical/IR images only as dark regions blocking out the light from background stars, show up in emission at *Herschel* wavelengths. Elliptical galaxies, many of which have very little interstellar material and are no longer actively forming stars, are often very faint in *Herschel* images whilst spiral galaxies, which may be less bright in the optical, are strong far-infrared/submillimetre emitters. Together, therefore, the combination of optical/IR and FIR/submillimetre observations provides us with a full picture of the ecology and life-cycle of star formation and evolution.

Large-scale mapping by *SPIRE* and *PACS* of nearby molecular clouds and of the Galactic plane has revealed the ubiquitous filamentary structure that is now clearly understood to mediate star formation, and shown that these filaments tend to have a universal size scale of around a tenth of a parsec, although their densities can vary over several orders of magnitude. This implies that the mechanism for the initial formation of the filaments is not gravitational but determined by gas physics, perhaps the dissipation of magnetohydrodynamic turbulence. The *PACS* and *SPIRE* images also allow the detection and characterization of large numbers of pre-stellar condensations and protostars, which form through gravitational collapse when the mass per unit length of the parent filaments exceeds a threshold value of around 15 solar masses per parsec. This has allowed the statistical and physical properties and the evolution of these stellar precursors to be understood in unprecedented detail, revealing, for instance, a clear link between the initial mass function (IMF) of stars and the IMF of the clumps from which they form.

SPIRE has also revolutionized our understanding of the extragalactic sky. Its ability to map large areas with great sensitivity, and the fact that in the submillimetre range star-forming galaxies can readily be detected out to high redshift, has resulted in the multi-band detection of very large numbers of such galaxies. Statistical analysis of the properties of large samples, combined with data from catalogues at other wavelengths, is allowing astronomers to reconstruct the star-formation history of the Universe and to understand the origin and evolution of the galaxies that populate the cosmos today. The large areas covered by the *SPIRE* surveys also meant that a significant number of rare strongly-lensed sources were detected. These are very distant galaxies whose emission is amplified, typically by a factor of 10–30, by the serendipitous location of a massive elliptical (submillimetre-faint) galaxy exactly along the line of sight. This means that galaxies could be studied by *Herschel*, and by follow-up with other observatories, out to even higher redshifts than would otherwise be possible. One way in which *Herschel* has changed our understanding of galaxy evolution is by revealing that the most important modality of star formation in high-redshift galaxies is not merger-induced starbursts as in the local Universe, but driven by much larger reservoirs of available gas.

The *SPIRE* Fourier-transform spectrometer has also been extensively used as a spectral-survey instrument, given its ability to measure the continuum and spectral features over its whole 194–672-μm band in a single observation. This includes many atomic and molecular lines which characterize gas composition, density, and temperature in Galactic molecular clouds and in nearby and lens-amplified high-redshift galaxies.

So far, some 1300 refereed papers have been produced using *Herschel* observations, over 780 of which use *SPIRE* data, and many more are to be expected in the future given the huge amount of data now publicly available in ESA's *Herschel* Science Archive. To ensure that the *Herschel* observations will be as easy as possible for present and future astronomers to access and to use, a post-operations programme is currently under way, with the objectives of generating the final and definitive data products, calibration, and documentation.

Herschel's combination of angular resolution, sensitivity, and wavelength coverage has brought about a new understanding of the obscured Universe. The over-arching scientific legacy of *SPIRE* and *Herschel* is likely to be seen as the development of an integrated and unified picture of star formation and galaxy evolution over cosmic time and distance — with close-up study in our own galaxy where *Herschel* has good enough angular resolution to study individual molecular clouds and their internal structure, examination of the global structure of nearby galaxies such as Andromeda, and the establishment of a statistical picture of star formation and evolution of galaxies in the high-redshift Universe.

The President. Any questions?

Professor D. Lynden-Bell. You showed something in the position of NGC 205, a satellite galaxy of Andromeda, but it was at a slightly different angle. When it's looked at in the far-IR it has a slightly different angle than in the visible.

Professor Griffin. What that means I'm not sure but these have been overlaid manually. It is very much fainter in the far-IR than in the visible.

A Fellow. A technical question. You mentioned that the lifetime of the mission was just a few days short of four years. I'm wondering how that compared to your desired time?

Professor Griffin. It met the requirement comfortably. The instrument teams and observers were hoping for bit of a bonus but you can never tell. It turned out the margin on the lifetime wasn't as great as we'd have liked, but it did exceed the design lifetime so it was completely compatible with what we were entitled to expect.

The Fellow. They always do and I was wondering by how much?

Professor Griffin. We were hoping for another six months.

The President. Let's thank Matt again. [Applause.] Our next speaker is Abigail Calzada-Diaz: 'Constraining source localities of lunar meteorites using remote-sensing datasets'.

Ms. Abigail Calzada-Diaz. The Moon is a valuable body to understand better the early history of the Solar System as it preserves a geological record of the processes that have shaped the formation and evolution of the Earth–Moon system. Also, it is the only planetary body from which samples have been returned from known locations. These samples from the Apollo and Luna missions have shed light on the age and composition of the Moon. However, these sample-return sites are restricted to nine localities on the near side of the Moon and do not give us sufficient global knowledge to understand fully its origin and evolution.

Fortunately, we have an additional source of lunar material: lunar meteorites. These samples have been ejected from the surface as result of impact events. Petrological and geochemical analysis performed on these meteorites, combined with orbital remote-sensing measurements, can reveal many new details about the composition and geological evolution of the Moon.

Our investigation aims to determine the local geological context of these samples by utilizing geochemistry data from the NASA's *Lunar Prospector Gamma-Ray Spectrometer* (*LP–GRS*). This instrument resolved spatially the abundance of major rock-forming elements (O, Si, Ti, Al, Fe, Mg, Ca), as well as radioactive incompatible elements (U, Th, and K), within the upper few tens of centimetres of the surface, producing global abundance maps of the composition of the lunar regolith. For our purposes we chose the FeO, TiO_2, and Th data-sets. Fe allows us to distinguish between mare basalts and highland lithologies, Ti discriminates between different types of basalts, and Th differentiates Procellarum KREEP Terrain (PKT). Finally, we used the 2 × 2-degree-per-pixel (*i.e.*, 60 × 60-km-per-pixel) data-sets because of a combination of an adequate compositional accuracy and an acceptable spatial resolution.

We have developed a new software application in the PYTHON programming language that matches input elemental compositions from a *LP–GRS* data-set taking into account the instrumental uncertainties as well as the analytical standard deviations of averaged measurements. The PYTHON application is compatible with ArcGIS and produces a 'shapefile' layer that allows for convenient visualization of the results.

We first validated our approach by comparing the composition of regolith breccias returned by the Apollo and Luna missions with the elemental abundances of FeO, TiO_2, and Th reported by *LP–GRS*. Results for most of the samples (*Apollo 11*, *12*, *15*, *16*, *17*, and *Luna 16* and *24*) show that our method correctly matches the landing-site average-regolith-sample compositions with the corresponding *LP–GRS* pixels. Unsurprisingly, we found that other areas of the lunar surface have similar compositions. In the case of *Luna 16*, the closest matches are too far out to be considered a valid result. These discrepancies are related to the spatial resolution of the data-set and, therefore, heterogeneities in the area will affect the results. In the case of *Apollo 14*, the average Th compositions are larger than those reported in the *LP–GRS* data-set, so we did not obtain any valid results. Again, probably the discrepancy is most likely due to the coarse spatial resolution of the *LP–GRS* compared with the small scale of the sampling area (from cm up to 1 km).

Once validated, we applied our approach in a study of lunar meteorites. To do that we first created a data-base with compositions of 85 meteorites (including paired stones) based on previous works, and calculated the analytical standard deviation of averaged measurements. We obtained sensible good results in basaltic and mixed (containing basaltic and feldspathic components) regolith breccias.

Our approach is not sensitive to the differences among the highland feldspathic suites. Distinguished highland lithologies are key to understanding the mechanism leading to the formation and evolution of the lunar crust. This uncertainty may be further addressed with the inclusion of the dataset for the element Mg in our software.

As for the *Apollo 14* case, the Th content in some samples is extremely high and we do not obtain any match with the *LP–GRS* data. The upper limit of the Th measurements in the remote-sensing data-set is 11·6 ppm, considerably smaller than the 15 ppm measured in *Apollo 14* regolith breccia, and far below the 32·7 ppm measured on an impact-melt breccia meteorite (SaU 169).

Finally, our technique is easily adaptable for use with other elements and may be expanded to include additional remote-sensing datasets. In future work we will explore the use of mineralogical data to constrain further matches for different types of lunar material.

The President. Time for a few questions?

Professor Kathy Whaler. Does this technique have the potential to be applied to Mars in the future?

Ms. Calzada-Diaz. We were thinking about that. There are problems though. The Moon has no atmosphere and the processes are quite simple. Mars is more complex. We can think about it but as of now we are not sure how applicable it would be to Mars.

Dr. R. M. Catchpole. How do you select your lunar meteorites? How do you know they don't come from somewhere like Vesta, for example?

Ms. Calzada-Diaz. There are ways to determine that they are definitely from the Moon. The composition of the meteorites show a lack of volatiles that suggest they are from the Moon. Most have other similarities with samples from lunar missions. So we know that they are doubtless from the Moon.

The President. If there are no further questions, let us thank our speaker again. [Applause.] The next talk is from Samaya Nissanke: 'Black-hole births in real time'.

Dr. Samaya Nissanke. Gravity drives the evolution of the Universe at all scales but we still have many unanswered questions about how astrophysics works in the strong-gravity-field régime. We are now at a point where we can observe black holes being born in real time because of recent developments in gravitational-wave detectors such as *LIGO*, and tremendous advances in time-domain electromagnetic astronomy. These two observational tools combined will allow us to witness and probe the physics driving the birth of black holes.

We can answer this central question by combining the three main pillars of modern astrophysics: General Relativity, computational astronomy, and transient astronomy. By answering our central question on how black holes form we can push forward the frontiers of several diverse fields in astronomy, such as stellar evolution, by pinning down the fate of compact objects. We can also use these events as cosmological probes to study the expansion of the Universe and to understand cosmic enrichment.

In this talk, we are focussing entirely on the last stage of the development of compact binary systems — those involving neutron stars and black holes, which are the most extreme-gravity objects that we know of in the Universe. Neutron stars (NS) have masses in the range 1·4 to 2 $M_{\odot}$, radii of around 12 km and are extremely dense. They have huge magnetic fields and their centres are governed by exotic equations of state. On the other hand, black holes (BH) have masses ranging from 2 to 10^9 $M_{\odot}$ but they are very simple objects, being characterized entirely by mass and spin. From astrophysics we know that most binaries end up as NS–NS or NS–BH systems, but we still do not know if they merge, accrete, form relativistic jets, or explode. We don't even know if they ultimately form magnetars or black holes. What we do know is that this process causes some matter to be thrown out of the system, and it is this delayed matter outflow that will create electromagnetic emission that we plan to detect. From radio observations we know that the components in tight NS binaries exist and are spiralling in towards each other, losing gravitational radiation as they do so and carrying energy and angular momentum away from the system. In the last couple of orbits the gravitational energy which is expended is about 10^{57} ergs per second, which is 10^{24} times the output of the Sun or about 10 times the total brightness of all the galaxies that we can see.

Gravitational waves are the perturbations in the fabric of space–time and are generated by accelerating quadrupole moments. They are weak but they are also coherent. The measurable quantity in gravitational-wave astrophysics is called the dimensionless gravitational-wave strain, and it is inversely proportional to the distance. For NS binaries the frequency of the gravitational waves at zeroth order is twice the orbital frequency of the system. This makes them amongst the most numerous sources of gravitational radiation that we expect to detect with *Advanced LIGO*, which starts up this summer. The gravitational-wave strain is expected to be 10^{-21}, so in the 4-km arm we expect the length to change by 10^{-18} metre, or approximately one-thousandth the diameter of a hydrogen atomic nucleus.

Come 2020 we expect a world-wide net of kilometre-length gravitational interferometers for which NS–NS mergers are the guaranteed kHz gravitational-wave sources. We know that they exist because we have been watching the evolution of the Hulse–Taylor pulsar orbit for several decades and we have confirmed that the prediction by General Relativity of the loss of energy and angular momentum in the system is good to 0·4%.

One of the big challenges is that we only have a small sample of known NS–NS systems in the Galaxy and we have yet to find a NS–BH system. Finding a NS–BH system is one of the holy grails of radio astronomy as the likelihood is thought to be about one per 100 million years per Milky Way-equivalent galaxy. With *LIGO* we expect to detect tens of systems before they merge, and with the new time-domain surveys we expect to detect their electromagnetic emissions.

In order to reach the limit of 10^{-18} metre with *Advanced LIGO* we need to understand the instrumental noise very well. Secondly, we need to know what to look for. What are the predictions of this gravitational-wave strain? For inspiralling sources, by looking at the 'chirp' with time we will learn a lot about the source. Using the weak-field-perturbation theory of Einstein's field equations, the consequences of gravitational-wave production have been modelled over the last two decades and we are using these templates to try and detect gravitational waves. Depending on the mass of the NS or BH and the signal-to-noise ratio of the source, accuracies in mass of a few per cent can be obtained, and spins can be determined to a few tens of per cent. For the geometric properties such as orbital inclination, distance, *etc.*, strong degeneracy exists amongst the parameters. To resolve the degeneracy we need the additional information from electromagnetic emission from counterparts. We identify four such counterparts — first is a prompt radio emission which happens just before merger due to some relativistic plasma outflow; second is a short gamma-ray burst due to some accretion from a centifugally-supported central disc which is powering some relativistic jet; third, at longer timescales, we expect to see optical counterparts called 'kilonovas' powered by r-process nucleosynthesis in the ejecta; and finally there are slowly-developing radio remnants (supernova-like) which appear much later at time-scales of a few years. These counterparts thus represent a time-scale range from a few seconds to a few years.

We have a list of 40 gamma-ray bursts whose host galaxies have been identified but we still do not know if they are as a result of NS binary systems. A few years ago the first kilonova candidate coincident with a gamma-ray burst was seen by Nial Tanvir with a UK-led team.

From the electromagnetic signature we get a whole list of information to tell us what nucleosynthesis is happening and we can also get key information about the environment. This is why we want to combine the gravitational-wave 'chirp' and the electromagnetic signature.

For follow-up, we ask how many events we have now, how far can we detect them, and how well can we localize them. When *Advanced LIGO* comes into operation this summer it will have an increased sensitivity 1000 times higher than the current set-up. This implies that we can see a rate 1000 times larger than at present and when we reach design sensitivity we expect to see a mean rate of 20 events a year and out as far as 200 Mpc. Gravitational-wave detectors have very poor angular resolution, so to locate a source on the sky we need two or three of them working together for triangulation. We expect that the linear angular resolution will be about 10 degrees (an area of 100 square degrees). Within this area, of course, there will be many other sources including supernovae, dwarf novae, asteroids, and so on.

Radboud University is building a dedicated wide-field optical telescope called *BlackGEM* to follow up NS-binary mergers. It will be in La Silla and consist of four telescopes which can cover a total area of eleven square degrees in five colours. This array will carry out a major survey, trying to separate transients and variables. It is expected to cover the whole southern sky in its first year of operation. The next step will be to observe radio remnants with *MEERKAT*.

The President. Sounds like exciting times. Questions?

Rev. G. Barber. Thank you for a fascinating lecture. A question about the original *LIGO*: were we expecting any detections from the original instrument?

Dr. Nissanke. Not really. The main challenge we face with gravitational-wave interferometers is that we have these huge uncertainties in the merger rates. So it was unlikely. If we're talking about the mean rate being 20 now we have to reduce that by a factor of 1000. In the six years or so it was running there was a chance they would have seen something but they would have been lucky. The merger rates are only calibrated by 13 known systems of binary pulsars.

Rev. Barber. The actual advance is a factor of ten in terms of the distance?

Dr. Nissanke. The sensitivity is a factor of ten increased, because we are not measuring the flux but the strain amplitude of the wave.

Professor Griffin. I didn't see anything about binary-black-hole mergers. Do you expect to see any and if so would they have any electromagnetic signature due to destruction of local material or some second-order effects?

Dr. Nissanke. I only talked about the guaranteed sources. I focussed on those because we know at least one of the components has matter, so we expect some matter outflow. For supermassive black holes we expect some electromagnetic counterparts but they happen at a very different frequency range, mHz rather than kHz. For those you would need space-based gravitational-wave interferometers. For stellar-mass black holes, it's doubtful that there would be any electromagnetic counterpart but they are a strong anticipated source. In that frequency range you detect the final orbits before they merge. We are in the strong-gravity-field régime for binary black holes so you can test GR fundamentally.

The President. Thank you very much. [Applause.] Our final speaker is Allan Chapman: '*Micrographia* on the Moon: Robert Hooke and the origins of lunar geology in 1665'.

Dr. A. Chapman. It may seem curious that a book entitled *Micrographia* should possess any connection with astronomy, but indeed, it does. For Robert Hooke's great treatise, published in 1665 January, initiated several lines of primary research which are still very relevant to modern astronomy, planetary science, optics, and technology.

Let us begin with optics. Up to Hooke's time, academic opinion about the nature of light and colour (as opposed to its refractive and reflective properties) was still essentially classical. Light was pure and white, emanating as it did from the heavens, and only broke down into colours upon making contact with corrupting terrestrial materials, such as air, water, or glass. However, on the basis of meticulous astronomical observations and laboratory experiments, Hooke proposed in *Micrographia* Observation 9, that light consisted of two primary colours, red and blue.

These were produced when the opposite extremities of a 'pulse' or wave of light entered the eye. All the other colours of the spectrum came about through a mixture of the red and blue primaries in God's 'pallat', in much the same way as a painter could derive all of his tints from a few primary colours.

Hooke's fascination with telescopic optics and the design of machines for figuring lenses also comes across in *Micrographia*. Since 1612 or so, astronomers had been amazed at how each improvement in telescopic power had revealed an infinity of new stars. Hooke tells us, in *Micrographia* Observation 59, that whereas Galileo could see 36 stars in the Pleiades, he himself, by 1664, using a telescope of 36-feet focal length with an object glass 3·5 inches in diameter, could see more stars than he could count.

Hooke, in addition, drew attention to what we now call telescopic 'resolving power', which depended on object-glass diameter, as opposed to simple magnification. Indeed, in this respect he was also a significant innovator. He claimed that his 36-foot telescope revealed five stars in the Orion's Sword, or Nebula, whereas less than a decade before, Christiaan Huygens' smaller telescope had revealed only three. What was the Nebula, and how did the individual stars relate to the glowing material? That question remained unanswered 150 years later, and was still being asked by Lord Rosse in 1850.

Yet the most astronomically significant Observation in *Micrographia*, covering five pages of small print, is of 'The Moon'. For here, in Observation 60, Hooke initiated several lines of research which still have their resonance with us today. He began by making a 'high-magnification' (I reckon about 170 diameters) observation of a single lunar feature, the crater Hipparchus, his accompanying published plate of the formation covering some 90 arc seconds of the lunar surface.

How were Hipparchus and other lunar craters formed? Were they the result of a projectile from space impacting the Moon with great force? Or were they the product of what we now call lunar vulcanism? Hooke tended to favour the latter hypothesis, and drew parallels between lunar craters and terrestrial volcanoes, such as Etna, Hecla, and the 'Vulcans' of Tenerife and the Americas.

And it is at this point in *Micrographia* that Hooke becomes the first 'laboratory astronomer', from his attempts to replicate crater formations experimentally. Dropping spherical lead bullets into soft glutinous pipe clay, he obtained astonishingly crater-like depressions, complete with central mountains. Next, he blew air into the pipe clay and noticed that the emerging bubbles also resembled craters. But most dramatic of all was the bubbling mass of boiling alabaster, which produced craters remarkably similar to those on the Moon. Hooke concluded that the Moon possessed, or had once possessed, a fiery lava interior, just like Earth. He tells us that he used a candle to cast shadows from different heights upon his artificial moonscape, to replicate the appearance of a feature at different stages of the lunation.

And before Newton had become involved with the gravity problem in 1663–4, Hooke argued that not only Earth, but the Moon (and by extension, other astronomical bodies) must possess gravitational attraction, for the spherical nature of our rocky satellite suggested that the matter of which it was composed had become uniformly packed around a central gravitating point. Indeed, Hooke's work on gravity, and his *circa* 1662–3 'gravity-measuring' experiments, were part of a European tradition extending back through Huygens, Jeremiah Horrocks, and on to Kepler.

Robert Hooke's experimental approach to astronomy would later be extended to comets in 1677, when he successfully reproduced comet-like nuclei, tails, and streamers with metals immersed in acids. But it was in *Micrographia* that Hooke displayed an astronomical perceptiveness and a brilliance which are quite breath-taking. While his fellow-astronomers were primarily occupied in measuring celestial angles, Robert Hooke invented a physical, experimental approach to physics and astronomy which was of a piece with his wider geophysical, geological, and meteorological interests. For Hooke was coming increasingly to see both the heavens and Earth in dynamic and developmental terms. And lying at the heart of this vision was a sustained inquiry into what he, groping for the language, saw as 'force', 'attraction', or 'weight', and which a later age would call energy. It was this 'force' that, in his way of thinking, connected light, gravity, colour, motion, heat, and pressure together, as the unifying agencies of nature. And all of them, moreover, were to be rendered measurable and comprehensible by means of accurate instruments and increasingly refined experimental and observational procedures.

The President. Superbly entertaining as always, but we have run out of time for questions so you'll have to catch Allan at the drinks reception in the RAS Library immediately following the meeting. I give notice that the next monthly A&G meeting of the Society will be on Friday 2015 March 13.

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 243: HD 20577, HD 23257, HD 38232, AND HD 130669

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Orbits are given for four spectroscopic binaries of very various characters. HD 20577 has proved to be a triple system; there is only one star whose radial velocity can be measured, but the velocity varies in two periods — one of 1021 days, which seems to be determined with an uncertainty of less than two days, and a long period that appears to be something like 70 years but is not at all well determined. Even with two orbits fitted, the velocity residuals are unusually large (0·6 km s^{-1}) and may be indicating further complications that are not understood here. HD 23257, a star near the Pleiades that is nearly bright enough to qualify for the *Bright Star Catalogue*, has an orbit of moderate eccentricity and a period inconveniently close to two years. Just when this paper was being prepared, an impression that had been gained that the system was incipiently double-lined was confirmed explicitly — it was then at a nodal passage, and a weak secondary

dip was observable in the radial-velocity trace. The orbit of HD 38232 is also of moderate eccentricity, and has a period of about 6·3 years. HD 130669 is double-lined, and has been known for almost 100 years as a very close visual binary. There has been an ambiguity in its orbital period, with values close to 10 years and 20 years being promulgated; the radial velocities show the shorter period to be the correct one.

Introduction

The stars treated in this paper form a rather motley collection that has been assembled from various sources. HD 20577 comes from an early Cambridge programme of stars that were specially selected for ease of measurement, as a sort-of 'fall-back' programme for nights when observing conditions were poor. HD 23257 was recognized as an astrometric binary from *Hipparcos* data. The radial velocity of HD 38232 was found to be variable in a survey made with the Geneva Observatory's *Coravel* spectrometers. HD 130669 has long been known as a very close visual binary, and the radial velocities confirm the resolution of an ambiguity that long existed between orbital periods of 10 and 20 years in favour of the shorter one.

HD 20577

HD 20577 is a $7^{m}\cdot 7$ late-type star, to be found about 3½° south of, and slightly preceding, α Persei — nearly half-way from that star towards Algol. The only spectral classification available for it, despite its being quite bright, appears to be the K2 listed in the *Henry Draper Catalogue*[1]. In fact the star seems not to have inspired much interest from astronomers at all, since the only paper that is retrieved for it by *Simbad* is one by Fehrenbach *et al.*[2] giving a radial velocity of -4 ± 5 km s^{-1} as the mean of two measures obtained by their idiosyncratic objective-prism technique. There does not seem even to be any ground-based photometry of HD 20577, but *Simbad* interprets *Tycho 2* photometry as being equivalent to $V = 7^{m}\cdot 68$, $(B - V) = 1^{m}\cdot 52$.

The star featured in an early Cambridge radial-velocity listing called the 'thick-night programme' that consisted of stars that were easy to observe by virtue of their brightness (brighter than 8^{m}), favourable spectral type (K2), and near-zenithal declination (+40 to +50 degrees) and so might be measureable in poor observing conditions. Orbits were given in this series of papers for five of them before the existence of the underlying programme was ever admitted; the first mention of it was made in Paper 174[3] (regarding HD 221422; and the five stars already treated were there identified). The programme was eventually described in Paper 205[4] (HD 9519) and its nature recalled in some detail in Paper 226[5] (HD 11571). It was explained there that only a limited number of the stars on that programme (ones between 22^{h} and 5^{h} R.A.) were ever observed; in fact the only one discovered to be a binary that remains unpublished after this is HD 26446, which has an orbit with a period of about 21 years and a small amplitude, and needs to be followed for several more years to obtain satisfactory phase coverage.

The first Cambridge radial-velocity observation of HD 20577 was made as long ago as 1968, with the original spectrometer[6] with which the cross-correlation procedure was developed. It was four years before the next observation was made, and found to be significantly discordant; when several further observations did not throw up another such discordance the writer rather lost interest in the object, but later instituted a programme of annual observations after he acquired the *Coravel* spectrometer that replaced the original one. A definite change was at length seen again in 2008, since when the star has been observed reasonably regularly. Altogether there are now 47 measurements of it — eight with the original spectrometer, one with the Haute-Provence *Coravel*, and 38 with the Cambridge one. They are set out in Table I and demonstrate an orbit of small amplitude (only 2 km s^{-1}), with a period that seems to be quite accurately determined now at 1021·3 ± 1·7 days. In addition, however, there is definite evidence of a variation of the γ-velocity with a comparable amplitude but a much longer period. Evidently the observed star has two companions, in an hierarchical system of orbits. The amplitudes in both orbits are so small that the companions would be detectable only if their brightnesses were tolerably comparable with that of the principal star, since the spectra must be wholly blended together all the time, unless indeed the companions are of such small mass that their spectra would be swamped by that of the primary in any case. No extra component has been recognized in the radial-velocity traces, which exhibit 'dips' of unusual magnificence, having equivalent widths (defined as for spectra but here in units of radial velocity rather than wavelength) of more than 7 km s^{-1}. They have a depth of about 42% of the 'continuum' and are distinctly broadened, as if by a rotational velocity which is quantified at a mean of 5·2 ± 0·2 km s^{-1}. The length of the outer orbit is longer than the present duration (about 17 000 days or 46 years) of the observations, and is not well defined by the data. In the solution that is proposed below an outer period of 25 000 days has been imposed; the minimum length appears to be about 20 000 days, but the true value could be arbitrarily longer. The orbits are illustrated in Figs. 1 and 2.

Element		*Outer orbit*	*Inner orbit*
P	(days)	25000 (fixed)	1021·3 ± 1·7
T	(MJD)	51810 ± 2193	53950 ± 45
Γ	(km s^{-1})	−9·02 ± 0·20	
K	(km s^{-1})	2·30 ± 0·26	2·17 ± 0·18
e		0·20 ± 0·11	0·24 ± 0·07
ω	(degrees)	1 ± 36	332 ± 18
$a_1 \sin i$	(Gm)	774 ± 88	29·6 ± 2·5
$f(m)$	($M_\odot$)	0·030 ± 0·010	0·00099 ± 0·00025

R.m.s. residual (weight 1) = 0·60 km s^{-1}

Although there is no doubt of the existence of the inner orbit with a period of just over a thousand days, and of an outer one with a period twenty or more times as long, the actual determination of the orbits is admitted to be rather unsatisfactory. The r.m.s. residual of 0·60 km s^{-1} is two or three times larger than is often found for orbits determined with the *Coravel*; indeed, in this case the residuals of the *Coravel* observations are virtually the same as those that stem from observations that were made with the original spectrometer[6] and were read by eye from a paper chart, and all the measurements have been given equal weight in the solution of the orbit.

There is no obvious reason for the raggedness of the *Coravel* measurements; it does not arise from just one or two outliers that might indicate mistakes of

TABLE I

Radial-velocity observations of HD 20577

Except as noted, the observations were made with the Cambridge Coravel
(Slight inconsistencies between the velocity columns arise merely from rounding errors)

Date (UT)	MJD	Velocity	Phase Outer	Phase Inner	Computed Vel. Outer	Computed Vel. Inner	(O – C)
		km s⁻¹			km s⁻¹	km s⁻¹	km s⁻¹
1968 Dec. 15·91*	40205·92	−13·2	0·536	0·543	−10·8	−1·6	−0·8
1972 Nov. 28·98*	41649·99	−8·2	0·594	1·957	−10·7	+1·7	+0·7
1973 Sept. 27·13*	41952·15	−10·7	0·606	2·252	−10·6	+0·5	−0·6
Oct. 18·13*	973·15	−10·4	·607	·273	−10·6	+0·2	−0·1
Nov. 12·00*	998·02	−10·8	·608	·297	−10·6	0·0	−0·2
1975 Aug. 28·09*	42652·11	−9·5	0·634	2·938	−10·4	+1·4	−0·4
1979 Sept. 28·97*	44145·00	−9·7	0·693	4·399	−10·0	−0·9	+1·2
1983 Nov. 23·99*	45662·02	−9·0	0·754	5·885	−9·3	+0·4	0·0
1993 Feb. 17·91†	49035·93	−6·6	0·889	9·188	−7·3	+1·3	−0·6
2002 Mar. 28·85	52361·84	−6·1	1·022	12·445	−6·3	−1·2	+1·4
2003 Jan. 20·90	52659·89	−8·8	1·034	12·737	−6·4	−1·4	−1·0
2005 Jan. 16·82	53386·81	−7·0	1·063	13·448	−6·7	−1·2	+0·9
2006 Mar. 3·83	53797·81	−8·1	1·080	13·851	−6·9	−0·2	−1·0
2007 Mar. 26·84	54185·82	−5·8	1·095	14·231	−7·1	+0·7	+0·6
2008 Feb. 27·78	54523·76	−9·7	1·109	14·562	−7·4	−1·6	−0·7
Mar. 30·83	555·81	−9·6	·110	·593	−7·4	−1·7	−0·5
Sept. 20·15	729·13	−8·7	·117	·763	−7·5	−1·2	0·0
Oct. 22·08	761·06	−8·7	·118	·794	−7·5	−0·9	−0·3
2009 Mar. 5·86	54895·84	−7·1	1·123	14·926	−7·6	+1·2	−0·7
Sept. 10·18	55084·16	−5·1	·131	15·110	−7·7	+2·3	+0·4
Oct. 9·14	113·11	−5·9	·132	·139	−7·7	+1·9	−0·1
Nov. 24·05	159·02	−6·0	·134	·184	−7·8	+1·4	+0·4
Dec. 20·92	185·89	−5·9	·135	·210	−7·8	+1·0	+0·9
2010 Feb. 17·84	55244·81	−7·3	1·137	15·268	−7·8	+0·3	+0·2
Oct. 7·13	476·10	−9·9	·147	·494	−8·0	−1·4	−0·5
Nov. 15·01	514·98	−9·9	·148	·532	−8·0	−1·6	−0·3
2011 Jan. 14·93	55575·90	−10·0	1·151	15·592	−8·0	−1·7	−0·3
Apr. 7·83	658·80	−9·4	·154	·673	−8·1	−1·7	+0·4
Oct. 20·09	854·06	−7·9	·162	·864	−8·2	0·0	+0·3
Nov. 18·03	883·00	−7·1	·163	·892	−8·2	+0·5	+0·6
Dec. 14·94	909·91	−6·8	·164	·919	−8·3	+1·0	+0·5
2012 Jan. 10·93	55936·90	−6·9	1·165	15·945	−8·3	+1·5	−0·1
Feb. 3·81	960·78	−7·1	·166	·969	−8·3	+1·9	−0·7
Mar. 1·83	987·80	−6·4	·167	·995	−8·3	+2·3	−0·4
Sept. 7·19	56177·16	−7·0	·175	16·181	−8·4	+1·4	0·0
Nov. 3·12	234·09	−8·1	·177	·236	−8·5	+0·7	−0·3
Dec. 1·99	262·96	−8·6	·178	·265	−8·5	+0·3	−0·5

TABLE I (*concluded*)

Date (UT)	*MJD*	*Velocity* km s^{-1}	*Phase* *Outer*	*Phase* *Inner*	*Computed Vel.* *Outer* km s^{-1}	*Computed Vel.* *Inner* km s^{-1}	*(O – C)* km s^{-1}
2013 Feb. 1·89	56324·86	–9·4	1·181	16·325	–8·5	–0·3	–0·6
Mar. 2·82	353·79	–9·7	·182	·353	–8·5	–0·5	–0·6
Apr. 2·82	384·79	–10·2	·183	·384	–8·6	–0·8	–0·9
Sept. 3·18	538·15	–10·4	·189	·534	–8·7	–1·6	–0·2
Oct. 7·09	572·06	–10·4	·190	·567	–8·7	–1·7	–0·1
Dec. 1·03	627·00	–10·1	·193	·621	–8·7	–1·7	+0·3
2014 Jan. 20·81	56677·78	–9·5	1·195	16·671	–8·7	–1·7	+0·9
Feb. 11·87	699·84	–9·6	·196	·692	–8·8	–1·6	+0·8
Mar. 12·79	728·76	–9·7	·197	·721	–8·8	–1·5	+0·6
Nov. 20·99	981·96	–6·2	·207	·969	–8·9	+1·9	+0·8
Dec. 8·93	999·90	–6·3	·208	·986	–8·9	+2·2	+0·4
2015 Jan. 8·85	57030·82	–6·3	1·209	17·016	–9·0	+2·5	+0·1
Feb. 17·82	070·79	–7·0	·210	·055	–9·0	+2·6	–0·7

*Observed with original spectrometer
†Observed with Haute-Provence *Coravel*

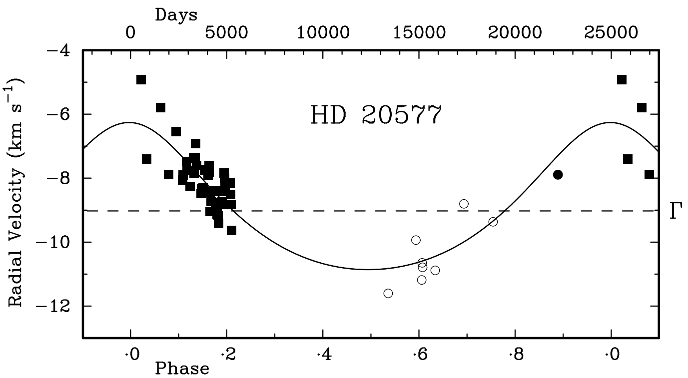

FIG. 1

A possible approximation to the 'outer' (long-period) orbit of HD 20577. The γ-velocity of the (single-lined) 1000-day inner binary varies in the fashion documented here. The orbital period has been arbitrarily fixed at 25 000 days; it might be as 'short' as 20 000 days, but it might be longer to any degree. The writer has been observing this object for nearly half a century and obviously cannot hope personally to continue much longer. Evidently the object will need to be observed occasionally in a systematic fashion for a long time yet.

The open circles represent measurements made with the prototype radial-velocity spectrometer that was developed by the writer[6] and brought into operation in 1966. The single filled circle shows an observation made with the Haute-Provence *Coravel*, while the filled squares plot measurements made with the Cambridge *Coravel*.

some sort, so it might be caused by slight instability in the star itself, perhaps of the nature of starspots. We know, however, that there are at least two companion stars which may contribute to, and tend to confuse, the observed dips, and may have their own idiosyncrasies about which we can know nothing. It would take

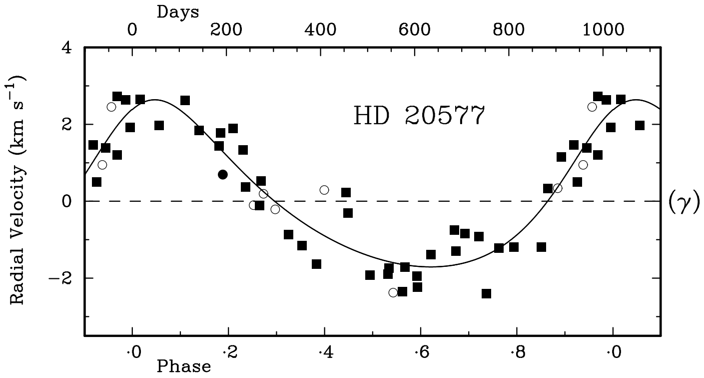

FIG. 2

The 'short-period' inner orbit of HD 20577, which has a period of 1021 days. Neither of the companions is detectable in radial-velocity traces, but the velocities of the one star that can be measured are unusually ragged. The raggedness may conceal other components or arise from spottedness and rotation of the primary star. One can see in this figure a possible wave with a quarter of the principal period, but such a situation is not dynamically plausible and the appearance may not be significant.

much longer than the writer is willing (or indeed able) to wait, and preferably the use of instrumentation having much higher intrinsic precision, before the question of the reality of the raggedness — whether it is to be assigned principally to the star(s) or to the instrument — could be demonstrated beyond cavil. Here, one can say only that the same instrument usually gives results substantially better than these ones appear to be, whereas the very fine dips seen in the traces ought to yield results that are actually *better* than usual, other things being equal. The apparent raggedness that bothers us about the velocities is only of the order of a tenth of the apparent rotational velocity, so there is scope for the excess width of the dip to obscure slight asymmetries such as could arise from starspots or from blending with additional spectra (of which we *know* that there are at least two) that cannot be recognized explicitly.

Dips having equivalent widths as large as 7 km s^{-1} are not found among normal giant stars, but generally belong to stars of higher luminosity and of later type than the K2 that is listed in the *Henry Draper Catalogue* for HD 20577. Velocity instabilities are naturally more prevalent among stars of the extra-large sizes that go with high luminosity and/or late-K or M types. HD 20577 was unfortunately not observed by *Hipparcos*, but its very small proper motion, found by *Tycho 2* to be only 2 arc-milliseconds per annum (and only 2σ from zero, at that), is another indication of high luminosity, although not an infallible one since the transverse motion in a particular case might fortuitously be almost zero. The 2-millisecond motion, taken at face value, represents about 1 km s^{-1} transverse velocity for every 100 parsecs of the star's distance, so even at a kiloparsec (for which distance the star would need to have luminosity class II) it would still represent only 10 km s^{-1}. A novelty for which the writer would not like to be remembered would be to hazard an estimate of spectral type, not upon any information about the actual spectrum at all, but upon such evidence as the colour index, proper motion, character of radial-velocity traces, *etc.* — an assemblage that might be held to point towards a type of about K5 II.

The mass functions in the two orbits are very small — that of the inner orbit strikingly so. In trying to interpret them, we are handicapped by not knowing the mass of the primary star nor anything about the orbital inclinations (or for that matter even the primary's rotational inclination). We can say, however, that for primary masses of 2 or 4 $M_{\odot}$, which might characterize a star whose luminosity is rather brighter than that of a normal giant, the companion in the long-period orbit would need to have a mass of at least 0·5 or 0·8 $M_{\odot}$; it could easily be so much fainter than the primary as not to be detectable in the radial-velocity traces. The companion in the 1000-day orbit could be much fainter still, needing to have a minimum mass of only about 0·16 or 0·25 $M_{\odot}$, respectively, for primary masses of 2 or 4 $M_{\odot}$.

Inasmuch as we have seen that the companion in the outer orbit could be four or five times less massive than the primary star, it could correspondingly be four or five times as far from the mutual centre of gravity, so the separation of the stars would be up to five or six times the distance of the primary from that point. We see from the informal table above that the latter quantity, $a_1 \sin i$, is about 5 or so AU, so the mean linear separation of the stars could be as much as 30-odd AU, which even at a kiloparsec would subtend some 30 milliseconds of arc and be in principle within the power of interferometers, or indeed of the current largest individual telescope apertures, to resolve. But such a separation would exist only if the companion star were near the minimum mass permitted by the mass function, and therefore near the minimum luminosity; it does seem like a tall order, even today, to resolve a very unequal double star with a separation that can subtend an angle only of the order of 30 milliseconds — and then only if the separation vector happens to be nearly perpendicular to the line of sight. And the smaller the Δm between the components, the smaller will be the maximum separation, owing to the approach to equality in the masses of the components and thus to their distances from their centre of gravity.

HD 23257 (*HIP 17482*)

This is a star, to be found about 3½° north of the Pleiades, that is nearly bright enough to be in the *Bright Star Catalogue*. The view of it in a telescope is enlivened by the presence of the slightly brighter F-type star HD 23245 about 2′ south-preceding. The pair was noticed and recorded as a double star some 170 years ago by (F. G. W.) Struve[7], although the angular separation was actually just outside the 2′ ostensible limit for inclusion in Struve's Class VI or in his catalogue[7] itself. Burnham listed the pair in his great *General Catalogue of Binary Stars*[8] as BDS 1839, but not before he had, characteristically, discovered[9] a "minute" companion about 8″ distant from the primary star (HD 23245). The system appears in Aitken's double-star catalogue[10] as ADS 2735.

The *V* magnitude recorded by *Simbad* for HD 23257, $6^{m}\cdot77$, evidently comes from a paper by Bakos[11], who also gave a $(B-V)$ of $0^{m}\cdot64$; it is a matter of conjecture why *Simbad* prefers Bakos to Eggen[12], who obtained a *V* of $6^{m}\cdot87$, as well as a $(B-V)$ of $0^{m}\cdot645$ and a $(U-B)$ of $0^{m}\cdot145$. Transformation of the *Hipparcos Hp* magnitude of $6^{m}\cdot99$ to *V* in the light of the $(V-I)$ colour index of about $0^{m}\cdot7$ found in *Simbad* gives a result agreeing closely with Eggen. The numerous passes made by *Hipparcos* provide assurance that the $0^{m}\cdot1$ discrepancy between Bakos and Eggen is not due to actual variability of the star, unless indeed it is of a long-term nature that is manifested only over decades and not mere years. Olsen[13], however, gave the *V* magnitude as $6^{m}\cdot859 \pm 0^{m}\cdot004$; the same value (probably quoted) appears in the large Geneva–Copenhagen survey[14] of F and G dwarfs. Thus the only appreciable discrepancy appears to be the magnitude given by Bakos.

The spectral type of HD 23257, G0 in the *Henry Draper Catalogue*[15], was given as G5V in a paper[16] published from the David Dunlap Observatory (DDO) under the name of the then Director, Heard, although he did not make the classification personally; it was subsequently proposed to be G2IV by Harlan & Taylor[17]. The (revised[18]) *Hipparcos* parallax of 15·45 ± 1·68 milliseconds corresponds to a distance modulus of 4·25 ± 0·21 magnitudes, and thus (on the basis that the V magnitude is 6^{m}·86) to an absolute magnitude of 2^{m}·61 with the same uncertainty, putting the object about two magnitudes above the main sequence at its solar-type colour index and thus supporting very nicely the G2 IV classification[17].

The radial velocity of the star was first measured at the DDO, from which a mean value of +49·0 with a 'probable error' of 1·3 km s^{-1}, from five photographic spectrograms, probably of 33 Å mm^{-1} dispersion, was published in the same paper[16] as the spectral type, nearly fifty years ago. The spread of the velocities, which were not listed individually, is well within the range expected from observational uncertainty. One measurement, which at +48·6 km s^{-1} was very accordant with the David Dunlap mean, was given by Beavers & Eitter[19] from the early photoelectric instrument[20] at Iowa State University at Ames, Iowa. The Geneva–Copenhagen survey[14], however, while obtaining again a mean velocity near +49 km s^{-1}, found a "mean error" (thought to mean the standard deviation of the mean velocity) as large as 2·1 km s^{-1}, certainly implying that there were real variations.

In a fresh review of the massive data set accumulated by *Hipparcos*, in 2007 Goldin & Makarov[21] divined an astrometric orbit for HD 23257, with a period of 699^{+26}_{-28} days. In conjunction with the previous evidence[14] of variable velocity, that galvanized the present writer into placing the star on the observing programme of the Cambridge *Coravel* radial-velocity spectrometer. Observations were begun in 2009 and in due course did indeed demonstrate an orbit with a period very close to the astrometric one. Seemingly contradictory evidence subsequently came from Isaacson & Fischer[22], who observed HD 23257 repeatedly with the very precise 'planet-finding' system at the *Keck* 10-m telescope: they found a radial-velocity 'jitter' (a term first brought into use in such a context by Gunn & Griffin[23]) of 4·270 *metres* per second. That is supposed to be the difference, quadratically subtracted, between the observed spread of the velocities and the admitted uncertainty of measurement. How it could be so small, and determined to a millimetre per second, when we shall show below that the amplitude of variability is several *kilo*metres per second, is difficult — for the writer, impossible — to understand.

HD 23257 has now been observed for nearly three cycles of its orbit, and 36 radial velocities have been accumulated. They are listed in Table II, at the head of which the single observation from Ames is given — the only published one for which the date as well as the velocity is available. The orbital period is not far off two years, a circumstance that means that there are two gaps in phase coverage of the orbit, gaps that would persist for three or four more cycles before they could be completely closed. They are not considered to detract very seriously, however, from the determination of the orbital elements.

It was only when this paper was being drafted that HD 23257 was demonstrated to be double-lined. It happened to be then at a nodal passage in its two-year orbit — indeed, at the more favourable node near phase ·1 where its velocity is furthest from the γ-velocity — and a weak secondary dip was found immediately adjacent to, and partly blended with, the principal dip. The situation is illustrated by the trace in Fig. 3. Only near the nodes of the orbit

TABLE II

Radial-velocity observations of HD 23257

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

Date (UT)	MJD	Velocity Prim. km s⁻¹	Velocity Sec. km s⁻¹	Phase	(O – C) Prim. km s⁻¹	(O – C) Sec. km s⁻¹
1983 Oct. 29·32*	45636·32	+49·4	—	0·502	−1·7	—
2009 Oct. 25·11	55129·11	45·7	—	14·261	+0·3	—
Dec. 22·96	187·96	47·9	—	·347	+0·3	—
2010 Jan. 3·89	55199·89	48·2	—	14·364	+0·2	—
Feb. 17·84	244·84	49·1	—	·429	−0·4	—
Nov. 15·02	515·02	56·2	+38·4	·821	+0·1	−2·6
Dec. 19·00	549·00	56·5	43·9	·870	+0·7	+2·5
2011 Jan. 14·93	55575·93	53·9	—	14·909	−0·8	—
Sept. 13·19	817·19	45·9	—	15·259	+0·6	—
30·13	834·13	46·4	—	·283	+0·4	—
Oct. 20·10	854·10	47·0	—	·312	+0·3	—
Dec. 14·94	909·94	49·0	—	·393	+0·3	—
2012 Jan. 23·91	55949·91	49·8	—	15·451	−0·2	—
Feb. 18·82	975·82	50·8	—	·489	0·0	—
Mar. 7·83	993·83	50·9	—	·515	−0·4	—
Aug. 16·15	56155·15	55·8	—	·749	+0·3	—
Sept. 4·18	174·18	55·5	36·9	·776	−0·3	−4·5
Nov. 18·04	249·04	55·2	39·1	·885	−0·3	−2·7
Dec. 5·99	266·99	53·9	—	·911	−0·7	—
2013 Jan. 2·02	56294·02	51·6	—	15·950	−0·4	—
9·90	301·90	50·8	—	·961	−0·1	—
31·80	323·80	48·0	—	·993	+0·4	—
Feb. 27·81	350·81	44·2	—	16·032	+0·3	—
Apr. 2·81	384·81	41·7	60·5	·082	−0·1	−0·1
Sept. 3·19	538·19	47·1	—	·304	+0·6	—
Dec. 22·92	648·92	50·3	—	·464	0·0	—
2014 Feb. 11·88	56699·88	51·2	—	16·538	−0·6	—
26·82	714·82	51·7	—	·560	−0·5	—
Mar. 12·79	728·79	52·0	—	·580	−0·6	—
Sept. 25·18	925·18	56·2	43·7	·865	+0·3	+2·4
Nov. 1·07	962·07	53·3	—	·918	−0·9	—
Dec. 5·98	996·98	49·9	—	·969	−0·3	—
8·96	999·96	50·0	—	·973	+0·3	—
29·90	57020·90	46·7	—	17·004	+0·2	—
2015 Jan. 6·88	57028·88	46·1	—	17·015	+0·8	—
Feb. 17·82	070·82	41·8	62·3	·076	−0·1	+1·8
Mar. 10·83	091·83	+41·6	+59·6	·106	−0·2	−1·1

*Velocity published from Ames[19]

is the secondary a somewhat visible feature of the traces; most of the time it is heavily blended with, and masked by, the primary dip. Of course Fig. 3 was deliberately scanned over a velocity range that included all of the secondary dip and a good deal of the 'continuum' beyond, but before the recognition of the secondary the scan range was much less generous and was naturally centred on the obvious dip. In such a trace, even if the secondary is partly visible beside the

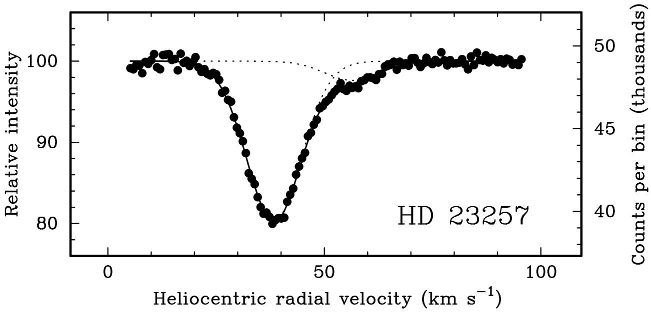

FIG. 3

Radial-velocity trace of HD 23257, obtained with the Cambridge *Coravel* on 2015 March 10 and showing the weak secondary dip, which had been anticipated from the nature of the orbit plot but had not been explicitly documented previously.

main dip, it is easily mistaken for a mere slope on the continuum between the two sides of the dip, as sometimes happens in any case, without significance, for merely instrumental reasons, and as such the slope would be taken out in a routine process of levelling the continuum.

The existence of the secondary had been suspected from the appearance of the 'orbit' plot, of the observed velocities against phase, in which the velocities at the nodes seemed to be slightly 'too extreme' in relation to those nearer the γ-velocity. The effect is more conspicuous now in the orbit plotted in Fig. 4, where the points near the nodes have been heavily weighted. The slope of the radial-velocity curve is seen to be slightly too shallow near the γ-velocity, where the two dips are wholly blended together, but near the nodes the increasing velocity separation makes the blending less severe, and the velocities of the primary are then less affected. In an effort to obtain the best orbital elements that the available observations are capable of giving, an effort has been made to re-reduce as double-lined the traces that gave primary velocities more than 5 km s^{-1} from the γ-velocity, with the parameters of the secondary dip fixed at a strength of 0·12 times the primary and the projected rotational velocity at zero. Those parameters are suggested by the partly-resolved traces, particularly that shown in Fig. 3. The new reductions led to primary velocities slightly further (typically 0·3 km s^{-1}) from the γ-velocity than the results of single-lined reductions of the same traces; the results for the secondary are badly scattered, as is only to be expected because the secondary is at, or often partly beyond, the relevant ends of the respective traces. It will perhaps be appreciated that we are here trying to measure features that the traces were never intended or expected to show and for which they are really unsuitable.

The results are plotted in the orbit diagram of Fig. 4, where the large symbols for the primary represent the observations for which efforts have been made at double-lined reductions. *They* have been given full (unit) weight in the calculation of the orbit, whereas all the other *Coravel* measurements, which are really of blends and not of the primary alone, have been given weight ⅕. Velocities of the secondary have been globally weighted 1/200, but the one

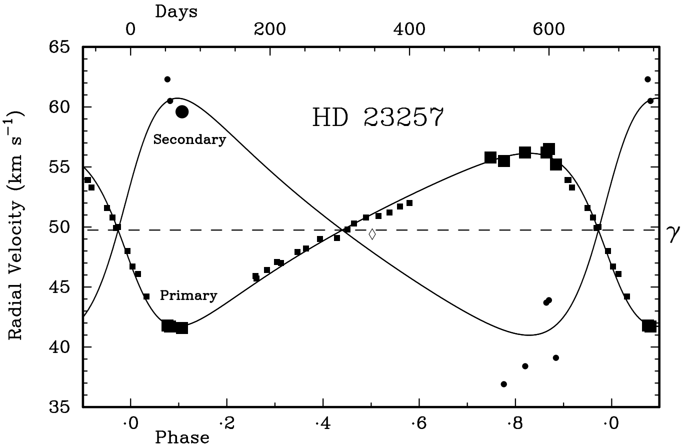

FIG. 4

The computed double-lined orbit of HD 23257, with the observed radial velocities plotted. All the measurements apart from the open diamond (a published[19] observation made in Iowa long ago) have been made with the Cambridge *Coravel*. The orbit largely depends on the relatively few observations for which an effort has been made to reduce them as double-lined. In the case of the primary, such measurements are plotted with much larger symbols than the others, whose velocities are those of blends of the primary with the weak secondary and so are 'dragged' slightly towards the γ-velocity; they have nevertheless been incorporated in the solution of the orbit with a weight of ⅕. The velocities obtained for the secondary by the efforts at double-lined reductions are inevitably badly scattered; they have been attributed a weight of only 1/200, apart from the *relatively* good one illustrated in Fig. 3, which has been given five times that weight.

observation that was properly organized to show the secondary, and appears as Fig. 3 here, has been given five times that weight. The one early observation from Ames[19] of the primary (or, to be more accurate, of the blend) has received a zero-point adjustment of +0·8 km s^{-1} before being entered in Table II. When the orbit was calculated without it, the period was found to be 691·9 ± 2·1 days, and the Ames point appeared in the phase diagram (analogous to Fig. 4) at a phase just over 0·1 cycles (70 days) later than the time ($\phi \sim$ ·44) at which the velocity curves cross the γ-velocity. Introducing it into the solution with a weighting of 1/25, as has been done in the adopted solution here, has reduced the computed orbital period by about one standard error, to 689·9 ± 1·7 days, and nearly halved the apparent phasing error of the early point, which dates from a time about 15 cycles before the mid-time of the recent data.

The way in which the velocities form sequences, on both the rising and the declining 'branches' of the primary's radial-velocity curve, that are slightly 'too shallow' owing to the blending with the weak secondary, is quite striking. The writer can think of ways in which it might be possible to disentangle the blends, or with better plausibility could at least 'clean' them of adulteration with the secondary, but all such methods could be open to objection on the grounds that, to a greater or lesser extent, one had in effect imposed the result that one was hoping to obtain. By simply down-weighting very drastically all the observations whose blending cannot securely be unravelled — which actually are the majority of all the observations of HD 23257 — one prevents the solution from straying far from correctness while minimizing the ill effects of blending. The fact that the blended observations are nearer to the γ-velocity than the ones for which an effort has been made at resolution guarantees that the former set have less leverage on the solution (particularly on the velocity amplitude), quite apart from their much smaller weighting.

The finally adopted orbital elements are as follows:

P	$= 689{\cdot}9 \pm 1{\cdot}7$ days	T	$=$ MJD 56329 ± 3
γ	$= +49{\cdot}74 \pm 0{\cdot}09$ km s^{-1}	$a_1 \sin i$	$= 63{\cdot}0 \pm 0{\cdot}9$ Gm
K_1	$= 7{\cdot}21 \pm 0{\cdot}09$ km s^{-1}	$a_2 \sin i$	$= 86 \pm 9$ Gm
K_2	$= 9{\cdot}9 \pm 1{\cdot}0$ km s^{-1}	$f(m_1)$	$= 0{\cdot}0210 \pm 0{\cdot}0009\, M_\odot$
q	$= 1{\cdot}37 \pm 0{\cdot}14$ $(= m_1/m_2)$	$f(m_2)$	$= 0{\cdot}054 \pm 0{\cdot}017\, M_\odot$
e	$= 0{\cdot}388 \pm 0{\cdot}014$	$m_1 \sin^3 i$	$= 0{\cdot}16 \pm 0{\cdot}04\, M_\odot$
ω	$= 106{\cdot}9 \pm 2{\cdot}3$ degrees	$m_2 \sin^3 i$	$= 0{\cdot}118 \pm 0{\cdot}015\, M_\odot$

R.m.s. residual (unit weight) $= 0{\cdot}23$ km s^{-1}

Since the initial evolution of a 1-$M_\odot$ star is towards higher luminosity at almost constant colour index (vertically upward in the H–R Diagram), we might well consider HD 23257 to be a star of approximately solar mass that has begun its evolution and is now about two magnitudes brighter than when it started. The table immediately above shows that the mass function yields a value of $0{\cdot}16 \pm 0{\cdot}04$ for the quantity $m_1 \sin^3 i$, from which if $m_1 = 1$ one obtains $\sin i = 0{\cdot}54 \pm 0{\cdot}05$, putting the orbital inclination in the 1-σ range 29°–37°.

Taken at face value, the mass ratio of $1{\cdot}37 \pm 0{\cdot}14$ presents a problem. When the mass ratio differs considerably from unity, as it does here, the components of a double star do not go through their giant-branch evolutions simultaneously. Although the ratio of dip areas in radial-velocity traces of the system appears to be about 0·12 to 1, when allowance is made for the primary component being about two magnitudes (a factor of 6 or so) above the main sequence, the secondary would seem to be giving a dip about ¾ as large as the primary would do if it were still on the main sequence, where it supposedly started its career at about G2 V. Expressed in stellar magnitudes, the ¾ ratio is little more than $0^{\rm m}{\cdot}3$, so the secondary could be considered to be of a type not much later than where the primary putatively started. The later type of the secondary means that the line strengths in its spectrum are likely to be somewhat greater than those of the primary, so we could suppose that to give a signature smaller by some $0^{\rm m}{\cdot}3$ in radial-velocity traces its actual luminosity might be less by nearly $0^{\rm m}{\cdot}4$ than the original luminosity of the primary — so it could be expected to have a type close to G5 V.

That produces a difficulty, because the difference in the masses of the two stars, one of type G5 V and the other somewhat evolved (but without significant mass loss) from a type very near G2 V, should be no more than about 10%, whereas the mass ratio found from the orbital elements is $1{\cdot}37 \pm 0{\cdot}14$. The discrepancy can be resolved (or at any rate put aside) by admitting for the mass ratio a value differing from the central one by two standard deviations, or else by admitting that the nature of the system has been misunderstood here. The former option may be the less disagreeable one; we could perhaps console ourselves with the thought that the radial-velocity amplitude of the secondary star is really very poorly determined, that there is only one point that could be regarded as at all reliable on that star's radial-velocity curve, and that that point actually would prefer a considerably smaller mass ratio than the one given in the table above, in which case the discrepancy that bothers us would not be glaring.

The agreement between the inclination given by the radial velocities, as being within a ±1-σ range of 29°–37°, and the astrometric[21] one, of 132^{+10}_{-9} degrees, is not very encouraging; even when we have inverted the astrometric number to 48^{+9}_{-10} degrees, *viz.*, 38°–57°, the 1-σ ranges found by the two methods do not quite overlap. On the other hand, the periods and the eccentricities found by the two methods are well within one standard error of one another, and the astrometric value of ω is too uncertain to make a useful comparison.

The visual companion star, HD 23245, type F0 in the *Henry Draper Catalogue*, is not related to HD 23257 — although it has a very similar parallax it has a completely different proper motion. Its magnitude and colour index have been given by Eggen[24] as $6^{m}\cdot77$ and $0^{m}\cdot37$, respectively. The absolute magnitude implied by the parallax[18] and V magnitude is close to $3^{m}\cdot0$; both that and the colour index suggest a type of F2 V or F3 V, but the only actual MK classification for it under the designation HD 23245 appears to be Abt's[25] F4 V. Although the parallax has an uncertainty of 1 part in 35, and therefore the luminosity of the star must be uncertain by at least 1 part in 18, a paper that I swore in Paper 242[26] (it is ref. 37 there) not to refer to again (in order avoid improving its citation statistics when really I was damning it) gives a derived distance ("56·980 pc") to better than 1 part in 50 000 and a luminosity ("5·45 $L_{\odot}$") to better than 1 part in 500. It also concerns itself with 'infrared excess' and lists the wavelength at which that excess is a maximum as 4·6 nm, *i.e.*, 46 Å, which is far from the infrared and must surely be a(nother) mistake.

The slightly brighter, F-type, star that is the subject of the paragraph immediately above (the primary or A component of the ADS 2735 pair, whose secondary is the object of greater interest in this present paper) has a substantial rotational velocity, which was quantified long ago by Slettebak as 60 km s^{-1}. His paper[27] was specifically devoted to rotational velocities, but it also included his own classifications of the stars concerned, which were F2 V for A (HD 23245) and G2 V for B (HD 23257). Slettebak also gave the rotation of B, as "≤25" km s^{-1} — that was the minimum rotation that the resolution of his spectra allowed him to determine. There have been some misunderstandings or misrepresentations both by Bernacca & Perinotto[28], who listed what they considered to be Slettebak's results in their *Catalogue I* of stellar rotational velocities, and by *Simbad* in its quotation of Bernacca & Perinotto. The latter duly attributed to Slettebak a projected rotational velocity of 60 km s^{-1} for ADS 2735 A, for which they also gave the identities HD 23245 (which Slettebak himself gave) and BD "27 556" (which he did not); they also attributed to him a rotation of ≤25 km s^{-1} for B, to which, however, they assigned the same *HD* and *BD* designations as for A, instead of HD 23257 and BD +27° 558. *Simbad* compounds the errors by attributing to HD 23245 both the 60 km s^{-1} that actually belongs to it, and "13 km s^{-1}" which must be *Simbad*'s interpretation of Bernacca & Perinotto's "≤25" for HD 23257. An effort to observe HD 23245 with the Cambridge *Coravel* on 2010 January 3 gave a radial velocity of +12·6 km s^{-1} and a projected rotational velocity of 43 km s^{-1}; the radial velocity seems not previously to have been measured at all. (The star, like HD 23257, is listed in the large Geneva–Copenhagen survey[14], accessible on line, of F and G dwarf stars, but for HD 23245 the columns relating to velocities in the table are all blank.)

HD 38232 (HIP 27172)

This star, of *HD* type F2, is to be found near the southern border of Auriga, about 4° following and slightly north of β Tauri. It is distinguished as being the nearest star of such brightness (about 7½ magnitude) to the 'Galactic anti-centre' — it is about 20′ north of the 180°-longitude point on the Galactic equator.

The *Hipparcos* Catalogue itself offers the transformation to $V = 7^{m}{\cdot}42$ from the very accurately established *Hp* magnitude of 7·5538 ± 0·0019, and it gives a $(B-V)$ value, transformed from *Tycho*, of $0^{m}{\cdot}644$. Bouigue *et al.*[29] long ago gave photometry for HD 38232 as $V = 7^{m}{\cdot}41$, $(B-V) = 0^{m}{\cdot}69$. Other values, over which there seem to have been some reservations, are those of Parsons & Montemayor[30] ($V = 7^{m}{\cdot}42$, $(B-V) = 0^{m}{\cdot}63$), and of Fernie[31] ($V = 7^{m}{\cdot}45$, $(B-V) = 0^{m}{\cdot}60$, $(U-B) = 0^{m}{\cdot}35$). Olsen[13] obtained $V = 7^{m}{\cdot}426 \pm 0^{m}{\cdot}004$ in the course of his work on Strömgren photometry; Craine & Scharlach[32] gave an unacceptably discordant V magnitude of $7^{m}{\cdot}58$ in their survey of V and I magnitudes.

The spectral type of HD 38232 was first classified on the MK system as F2 II by Nassau & Morgan[33] in the 'Case Survey' undertaken on objective-prism plates obtained with the *Burrell Schmidt**. Those authors refer to a 4° prism but neglect to mention the dispersion, which the present writer believes was 280 Å mm^{-1}. They actually told their readers that better classifications were being made from slit spectra by Bidelman, and *so* (five years later) it proved[35]. Bidelman was even more reticent than Nassau & Morgan as to the nature of his spectra: not only did he refrain from mentioning a dispersion, but even the telescope was not identified in any individual case! — he says only that "low-dispersion slit spectra have been taken at the Yerkes and Lick Observatories". His result in the case of interest was F5 II, and since then nobody seems to have dared to quarrel with Bidelman, apart from Reed[36], who listed HD 38232 as '+29°9' in his 'LS-North Catalog' [LS standing for 'Luminous Stars']. He gave its type as F2 Ib, obtained with an instrument called 'OP 580g' which is believed to mean an objective prism giving 580 Å mm^{-1} at Hγ on the *Burrell Schmidt*, but the significance of the 'g' has escaped the present author.

Sharon *et al.*[37] actually adopted HD 38232 as a standard for spectral type F5 II, and in that capacity its spectrum in what they called the *Y* band (0·95 to 1·11 μm) was illustrated in their paper in 12 successive sections, nice-looking and at quite high resolution (resolving power ~ 25 000). That region is relatively free from terrestrial atmospheric band spectra such as plague much of the infrared, and it contains a good number of quite strong lines of common elements, including H, C, Mg, Al, Si, S, Ca, and Sr.

Lebre & de Medeiros wrote a paper[38] specifically concerned with the Hα line in 'bright giants' (stars of luminosity class II). In a full-page diagram they showed the profiles of the line in 27 such stars. That of HD 38232 is quite unlike any of the others: it has strong broad wings and then a uniquely deep and narrow core, appearing to extend all the way down to zero residual intensity and having a FWHM that the authors quantify, with remarkable precision, as 47·51 km s^{-1}. The next-narrowest profiles have widths only slightly less than 60 km s^{-1}. The present writer recalls, from a time when he was privileged regularly to use the Mount Wilson 100-inch reflector and obtained photographic spectra of a number of very bright stars in the Hα region at the high dispersion of 1·6 Å mm^{-1}, that the central residual intensity of that line was *always* near 20% of the continuum throughout the wide range of spectral types of the stars observed. With the exception of HD 38232, most of the profiles shown by Lebre & de Medeiros are more or less in accord with that 'rule', although there are a

*A description and potted history of that telescope is to be found in a great footnote on pp. 330/1 in Paper 167[34] of this series.

few anomalies that are scarcely of concern here and upon which it is fruitless in any case to speculate in the absence of additional information. It is thus a surprise to find that HD 38232 seems to exhibit a uniquely deep and narrow Hα profile.

The radial velocity of HD 38232 was first measured by Sandage & Fouts[39] in the course of a large programme aimed at obtaining the velocities of stars in the three cardinal Galactic directions. The proximity of HD 38232 to the Galactic anti-centre was what qualified it for their programme, which was carried out at the coudé spectrograph of the Mount Wilson 100-inch reflector with a *Reticon* detector. Velocities obtained with that system had an internally (and thus optimistically) estimated r.m.s. error of 4·7 km s^{-1}. Measurements of *plates* taken at the same focus were good to better than 1 km s^{-1}, so although no doubt the *Reticon* system was 'faster' than photography, in the sense that more stars could be observed in a given time, when allowance is made for the 25–50-fold-worse variance obtained with the *Reticon* system the benefit is seen not to be all on one side. The single observation of HD 38232 gave a radial velocity of –13·9 km s^{-1}, about 8 km s^{-1} lower than the minimum velocity that we find below. Its date is not available, but in any case the measurement would not contribute usefully to the determination of the orbit, in which the r.m.s. residuals of observations made with the Cambridge *Coravel* are 0·31 km s^{-1} and are therefore better in an information-theoretical sense by a factor of at least $(8/0{\cdot}31)^2$ or about 700. The integration times at the 36-inch reflector were usually 180 seconds, so (at least theoretically) the measurement with the 100-inch (despite its 8-fold greater collecting area and better site) would be worth about ¼ of a second of observing time on the 36-inch! Of course, the 100-inch observation could have furnished some information about the nature of the spectrum, too, if that had not already been known.

De Medeiros & Mayor included HD 38232 in their survey[40] of radial and rotational velocities for evolved stars; they noted that they had obtained five observations with the Haute-Provence *Coravel*, that they demonstrated that the radial velocity was variable, and that the rotational velocity was 9 ± 1 km s^{-1}. Later, in 2002, they made the individual radial velocities, with dates, available in a file at the CDS, and it was after looking at that file that the writer adopted onto his observing programme HD 38232 and a number of other stars as objects in need of orbit determination. The measurements, all made with the Cambridge *Coravel*, were promptly begun in 2002 on the star of present interest and have been maintained reasonably systematically till the time of writing; they number 49, and are set out, with the five retrieved from the CDS, in Table III. It has been assumed that the Haute-Provence measures are on a true zero-point, but to place them on the scale that has usually been adopted in this series of papers they have been adjusted by +0·8 km s^{-1} before entry into Table III. The Cambridge velocities have analogously been adjusted to be homogeneous with the Haute-Provence ones; that has involved a correction of –0·5 km s^{-1} to the 'as-reduced' velocities, an adjustment that is line with expectation for a star of the colour of HD 38232. The data are readily solved to yield the orbit that is plotted in Fig. 5 and whose elements are given in the informal table below:

P	= 2302 ± 5 days	T	= MJD 54092 ± 11
γ	= –1·63 ± 0·05 km s^{-1}	$a_1 \sin i$	= 188·6 ± 2·4 Gm
K	= 6·30 ± 0·07 km s^{-1}	$f(m)$	= 0·0506 ± 0·0019 $M_\odot$
e	= 0·327 ± 0·010		
ω	= 341·9 ± 2·0 degrees	R.m.s. residual (wt. 1)	= 0·31 km s^{-1}

TABLE III

Radial-velocity observations of HD 38232

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

Date (UT)	*MJD*	*Velocity* $km\ s^{-1}$	*Phase*	*(O – C)* $km\ s^{-1}$
1985 Dec. 3·03*	46402·03	−6·4	0·660	−0·6
1986 Jan. 26·91*	46456·91	−6·1	0·684	−0·5
Oct. 28·13*	731·13	−3·7	·803	−0·2
1987 Oct. 1·18*	47069·18	+3·1	0·950	−0·9
1989 Oct. 24·00*	47823·00	−3·2	1·277	−0·9
2002 Sept. 30·20	52547·20	−3·3	3·329	+0·2
Dec. 11·17	619·17	−4·1	·360	−0·1
2003 Feb. 18·00	52688·00	−4·9	3·390	−0·4
Apr. 7·82	736·82	−5·1	·411	−0·3
Sept. 29·18	911·18	−5·5	·487	+0·1
Nov. 28·07	971·07	−5·2	·513	+0·5
2004 Jan. 17·06	53021·06	−5·7	3·535	+0·2
Apr. 13·85	108·85	−6·5	·573	−0·5
Sept. 16·15	264·15	−6·3	·641	−0·4
Nov. 14·20	323·20	−5·7	·666	0·0
2005 Jan. 5·04	53375·04	−5·6	3·689	0·0
Mar. 24·92	453·92	−5·0	·723	+0·2
Apr. 27·85	487·85	−5·5	·738	−0·6
Sept. 17·19	630·19	−3·6	·800	0·0
Nov. 10·08	684·08	−2·2	·823	+0·6
25·11	699·11	−2·5	·829	+0·1
Dec. 17·13	721·13	−2·6	·839	−0·4
2006 Jan. 28·95	53763·95	−1·4	3·858	0·0
Feb. 20·96	786·96	−1·0	·868	−0·1
Apr. 3·86	828·86	+0·3	·886	+0·2
Sept. 11·18	989·18	+4·3	·955	0·0
Oct. 3·20	54011·20	+4·9	·965	0·0
Nov. 1·15	040·15	+5·3	·978	−0·2
Dec. 9·11	078·11	+6·5	·994	+0·4
2007 Jan. 23·01	54123·01	+6·6	4·014	0·0
Mar. 21·88	180·88	+6·4	·039	−0·1
Apr. 28·85	218·85	+6·1	·055	−0·1
Oct. 5·19	378·19	+3·5	·124	+0·1
Nov. 3·18	407·18	+2·8	·137	0·0
Dec. 8·11	442·11	+2·4	·152	+0·3
2008 Jan. 6·06	54471·06	+0·7	4·165	−0·8
Feb. 10·98	506·98	+0·6	·180	−0·3
Mar. 30·87	555·87	+0·2	·202	+0·1
Oct. 2·20	741·20	−2·6	·282	−0·2
Nov. 23·12	793·12	−2·9	·305	0·0
2009 Oct. 25·18	55129·18	−5·0	4·451	+0·3
2010 Nov. 15·09	55515·09	−5·0	4·618	+1·0
2011 Oct. 1·20	55835·20	−4·8	4·757	−0·2
Nov. 18·10	883·10	−4·3	·778	−0·2

TABLE III (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2012 Feb. 1·99	55958·99	−2·8	4·811	+0·4
Sept. 12·19	56182·19	+1·4	·908	0·0
2013 Feb. 27·87	56350·87	+5·6	4·981	−0·1
Oct. 17·18	582·18	+5·2	5·082	−0·1
Nov. 9·15	605·15	+5·1	·092	+0·3
Dec. 5·08	631·08	+4·3	·103	0·0
2014 Apr. 15·84	56762·84	+2·0	5·160	+0·3
Oct. 10·19	940·19	−0·9	·237	+0·2
28·13	958·13	−1·0	·245	+0·4
Nov. 24·07	985·07	−1·7	·257	0·0

*Haute-Provence observation, retrieved from CDS — see text

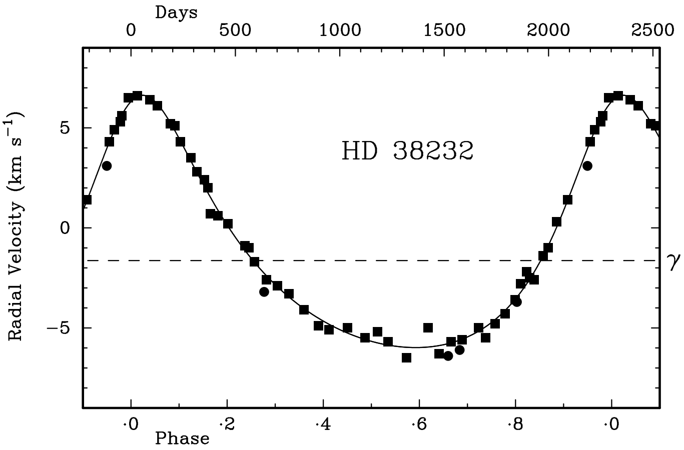

FIG. 5

The observed radial velocities of HD 38232 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The first five observations, plotted as filled circles, were made at Haute-Provence by others and have been retrieved from the CDS. They were given weight ¼ in the solution of the orbit. All the other measurements were made at Cambridge by the writer and appear as filled squares, with weight 1.

The *Coravel* traces yield very consistent values near 9 km s^{-1} for the projected rotational velocity of HD 38232. The formal mean value from the 49 traces is 8·92 ± 0·14 km s^{-1}, but values determined simply from the dip width are never claimed to be better than ±1 km s^{-1}, so the result should really be read as 9 ± 1 km s^{-1} — identical with the one found at Haute-Provence by de Medeiros & Mayor[40]. It is of course possible that the line-widths in a star such as this one, having a luminosity greater than that of a normal giant, may be significantly increased by small-scale mass motions of the type non-committally known as 'turbulence', so a cautious statement would not say that the projected rotational velocity of the star *is* 9 km s^{-1}, but that the line widths in the spectrum are such as would be produced by such a rotational velocity.

HD 130669 (*HIP 72479*)

HD 130669 appears as an $8\frac{1}{2}^{m}$ object in a region peculiarly barren of stars of naked-eye visibility* in the south-following corner of Boötes. Its visual duplicity was discovered 99 years ago by Aitken[41], who saw it as an equal double star with an angular separation of only 0·19 seconds of arc. It was just one of the extraordinary discoveries that Aitken made as a matter of routine with the Lick 36-inch refractor and published in lists of 100 at a time; it received his designation A 2983, and was actually the third-closest of the 100 pairs listed in the discovery paper. Aitken observed the system again from time to time, and when in 1932 he published his great *New General Catalogue of Double Stars Within 120° of the North Pole*, in which[42] the system appears as ADS 9397, he was still the only observer to have measured it — he listed measures at five epochs spanning ten years and appearing to show motion of nearly 180°. The main visual observers who joined him subsequently were van Biesbroeck, who used the McDonald 82-inch reflector, and van den Bos, who also used that telescope on one critical occasion but was normally at the Johannesburg 26-inch refractor.

The first orbits for the system were published quasi-simultaneously in 1945 by Ekenberg[43] and van den Bos[44]. They were not at all in mutual agreement, having periods, respectively, of 20·50 and 10·50 years. There is often a difficulty with close visual binaries in which from time to time the angular separation becomes too small to be resolved and the magnitudes of the component stars are nearly equal: the quadrant is indeterminate. So, after an interval when the system has been too close for resolution, there is no telling, when it is again resolved, which component is which. By quite plausibly reversing the quadrants of some of the observations they may be fitted tolerably well by two different orbits, one of which has twice the period of the other. In such a case, an interval when the pair is unresolved represents indeed a periastron passage in the short-period orbit, but may represent merely an apparent appulse, arising usually from an orbit of low eccentricity but high inclination, in the alternative case.

In his 1945 paper[44], van den Bos admitted that when the observed positions were plotted, "they suggest a period of some 21 years, but they fail to explain why the pair should have been found too close by Aitken in 1927 and again by Voûte in 1938.". After alluding to the difficulty referred to in the paragraph above, he proceeded to show that, by reversing some of the position angles, the data could more naturally be fitted by a period of half the immediately apparent length — and he went on to demonstrate exactly how that is done. He concluded with a splendid disclaimer of his own initiative at identifying, at that comparatively early stage in the observational history of HD 130669, the pitfall into which others long afterwards continued to tumble, and how to determine an orbit in which the components are resolved only over a limited arc, as follows:

> "I hope that it will be quite clear that the procedure given is not a new orbit method but a rather obvious adaptation of the powerful method devised by Thiele.
>
> "Unfortunately, Thiele published this method in a form which delights the mathematician by its elegance and frightens the orbit computer away by its stern rigour. Since it is not the mathematician, but the orbit computer who will use the method, once he has recognized its unrivalled power and flexibility in spite of Thiele's efficient camouflage, I have quite unblushingly and blasphemously endeavoured, in this and earlier papers on the subject, to sacrifice Thiele's mathematical elegance for the adaptability rightly insisted on by the orbit computer. I am prepared to be haunted by Thiele's ghost ..." [!]

*Very small optical power, however, shows that it has two brighter neighbours quite close by: there is HD 130768, $7^{m}\cdot4$ G9 III, about 7′ following, and HD 130705, $6^{m}\cdot6$ K4 III, about 10′ south.

Van den Bos[45] later (1954) gave an updated orbit with a period of 10·10 years; he remarked upon the possibility of a solution with double that period, but said again that it did not explain the negative results — occasions when the system was not resolved but when the 20-year orbit would lead to an expectation that it ought still to be resolvable. The elements that he gave included an eccentricity of 0·80, much higher than is now believed to be the case.

Eggen[46] wrote a brief paper in 1955 extolling the benefits of considering photometric data in deciding between competing orbits. After giving his photometry of the pair, and the spectral type of G9, he said that the photometric parallax of the system was 0″·024 (there was no actually measured parallax at that time). He found the dynamical parallaxes from the 20- and 10-year orbits to be 0″·022 and 0″·039, respectively, and on that ground (wrongly) selected the long period as the correct one. (The *Hipparcos* parallax[18] is 0″·0226, so although Eggen appears to have been almost spot on with his photometric parallax he then somehow came unstuck in relating it to the competing solutions for the orbit.) In another paper[47], in 1961, Eggen gave a slightly revised value of 0″·025 for the photometric parallax; in 1965, he gave it[48] as 0″·026.

Van den Bos[49], in a short 1961 paper entitled *Double solutions of orbits of visual binaries*, discussed the case of present interest (and one other), and concluded that Eggen's choice between the two possible orbits was not so compelling as its author had made it to appear. Three years later he[50] gave a further update of the short-period orbit, with the period revised slightly to 9·85 years and no mention at all made of an alternative solution. Eggen[51,48], however, discussed the situation again and came each time to the same conclusion as he[46] did before. In one case[51], he remarked, "The resulting mass of 0·3 ⊙ for the long-period orbit is expected for a star $1^{m}\cdot 5$ fainter than the Sun whereas that from the short-period orbit, 1·4 ⊙, would appear to rule out this interpretation." But 0·3 $M_{\odot}$ does seem a remarkably small mass for a star of $M_V \sim 6$. Later, however, in yet another paper (one that was not particularly concerned with binary stars), Eggen[52] flagged HD 130669's entry in a table with an asterisk leading to a footnote, "Equal components; period near 10 years", so he must ultimately have been won round to that idea, although he gave no evidence of what moved him in that direction.

UBV photometry of HD 130669 appears first to have been given by Roman[53], in her important paper on high-velocity stars; she gave the magnitudes as $V = 8\cdot 43$, $(B-V) = 0\cdot 88$, $(U-B) = 0\cdot 63$, the spectral type as K2 V, and the spectroscopic parallax as 0″·0275 on the basis that the components were of equal brightness (and thus each of the two stars in the system was of the given type). It was the high *W* component of the system's velocity within the Galaxy, –70 km s^{-1} (the velocity towards — in this case actually away from — the North Galactic Pole), that qualified the object to feature in her paper. The star is little more than 30° from the Galactic Pole, and the high *W* velocity stems largely from the radial velocity of about –90 km s^{-1} that was already known[54] for the system. Eggen[51] gave the *UBV* magnitude and colours as 8·40, 0·88, and 0·60, but later he[48,52] put them at 8·45, 0·865, and 0·575.

The Mount Wilson spectral type[54] was G9, so Roman's K2 represented a considerable change. But O. C. Wilson[55] agreed with that type. Wilson obtained spectra of a lot of late-type main-sequence stars with the excellent 10-Å-mm^{-1} system at the Mount Wilson 100-inch coudé. He put forward an opinion that attempts to interpolate spectral types between the MK standards at G5, G8, and K0 [and he might well have added K2, since there were no standards at K1] were "futile" — so his own classifications did not include any such interpolated types.

The accurate parallax that is now known[18] from *Hipparcos* corresponds to a distance modulus of 3·23 ± 0·12 magnitudes, and thus to an absolute magnitude of 5·2, with much the same uncertainty, for the system. The mean brightness per star would therefore be three-quarters of a magnitude fainter, nearly 6^{m}, very close to the $5^{m}\cdot 9$ value expected (*e.g.*, ref. 56) for type K0 V.

Radial velocities and orbit of HD 130669

The high radial velocity of HD 130669, near –90 km s^{-1}, has been mentioned above. Wilson & Joy[54] actually gave a mean of –91·1 km s^{-1} with a 'probable error' of 1·5 km s^{-1} from three plates; the individual velocities and their corresponding dates were long afterwards published by Abt[57], whose publication also noted that they were obtained at dispersions of 36 and (in one case) 80 Å mm^{-1} with a small prism spectrograph at the Cassegrain focus of the Mount Wilson 60-inch reflector. Heintz[58] gave two velocities, of –90·9 and –89·4 km s^{-1}, obtained only six days apart at 17 Å mm^{-1} with the 36-inch coudé-feed telescope associated with the Kitt Peak 84-inch reflector. He noted, "High-velocity pair with very uncertain elements of visual orbit. Double lines are more likely to occasionally appear if the short-period alternative is the true one. Such lines have not been seen; but there are few observations." Nordström *et al.*[14], in their huge table of results from the *Coravel* spectrometers at Haute-Provence and ESO, gave for HD 130669 a mean velocity of –89·9 km s^{-1} with a standard error of 1·6 km s^{-1}, and they flagged the star as a spectroscopic binary. They listed the number of their radial velocities as 76, and the total time span covered by them as 6282 days. That span is far more than the orbital period, but despite the apparently generous number and duration of the radial velocities, those authors seem never to have offered a spectroscopic orbit for the star, and the individual radial velocities are not, to my knowledge, accessible for use by other interested parties.

The writer's own interest in HD 130669 was piqued by the graph in Dommanget's first *Catalogue d'Éphémérides*[59], which showed that the visual orbit (van den Bos's 1954 orbit[45] was the basis) implied that radial velocities of the components of the binary would exhibit a ΔV of about 22 km s^{-1} in 1969, and then in a matter of months the difference would fall to zero and increase again to 20 km s^{-1} in the reverse sense*. Very many visual binaries never have velocity differences more than a few km s^{-1}, so the spectra of the components are always hopelessly blended together and radial-velocity traces of the sort produced by cross-correlation spectrometers show little change, either in the velocity of the blend or in the width or profile of the 'dip'. But a ΔV of 20 km s^{-1} promised a separation of the dips given by the two stars to an extent where they would be presented as almost separate entities in the radial-velocity trace and could be measured independently of one another. In those days the writer was using the original radial-velocity spectrometer at Cambridge[6]. He received the *Catalogue* as a gift from Dr. Dommanget in time to make an initial observation in 1969

*To infer radial velocities from an astrometric orbit necessarily involves assuming specific masses for the components; the velocity amplitudes, and correspondingly the linear orbital radii (and inversely the derived 'orbital parallax'), vary as the cube root of the input masses. The masses that Dommanget must have assumed were about 0·55 $M_{\odot}$ per component, seemingly unrealistically small and corresponding to main-sequence stars not much earlier than spectral type M0: they give the velocity amplitude that he plotted and also a value for $a \sin i$ that agrees with the values of a and i from his orbit[45]. Van den Bos himself[45], however, actually volunteered what the maximum relative nodal velocities should be: he said that they were 52 and 47 km s^{-1}. To obtain those ΔV values requires the input masses to be about 5·1 $M_{\odot}$, a wholly improbable amount; it also leads to an expected linear separation a_0 of 11·5 AU, with serious adverse repercussions on the 'orbital parallax'.

April, when a radial velocity of +16·6 km s^{-1} was obtained, and there was no sign of actual or incipient resolution of the dip.

It is difficult in retrospect to understand how the observer could have failed to realize that he was measuring the wrong star (it is now apparent that it was HD 130768, about half a minute of time in R.A. following HD 130669), especially since he specifically noted, "Estimated magnitude $7^{m}·7$" (the *Hipparcos V* magnitude of HD 130768 is actually $7^{m}·44$) whereas HD 130669 is a whole magnitude fainter; but the magnitudes of *HD* stars that had not been individually measured by 1969 were far from reliable.

In those days there was an active programme of observations of another character[60], under the direction of Prof. Redman, on the 36-inch telescope at Cambridge, and the writer and his radial-velocity spectrometer were able to use the telescope only in observing runs separated by something like six months. The next time there was an opportunity to observe HD 130669 was not until 1970 March, when the correct star was observed but there was nothing unusual about the width or profile of the radial-velocity dip. Thereafter the star was observed from time to time until 1977 (15 observations altogether, two of them obtained with the instrument[61] that Dr. J. E. Gunn and the writer made for the 200-inch telescope), but no change in the velocity or character of the 'dip' was noticed, and interest in it waned. A new edition of the *Catalogue d'Éphémérides*[62], in 1982, showed the radial-velocity changes to be expected on the basis of one of Eggen's 20-year orbits[48]; they were so small ($\Delta V \lesssim 4$ km s^{-1}) that there could be no hope of documenting them in any useful fashion.

All the same, after the introduction of the relatively powerful new *Coravel* spectrometer at Cambridge at the end of 1999, HD 130669 was restored to the observing programme. With the new instrument, the dips were recorded digitally instead of simply being drawn in real time by a pen on a Brown-Recorder chart, and there was a prospect of disentangling even quite closely blended pairs of dips, especially if there were ever an epoch when they were at least partially resolved and their individual profiles could be ascertained and then imposed on the reductions of the observations made when they were wholly blended. Altogether, 34 observations of HD 130669 have been made with the Cambridge *Coravel*; they are set out in Table IV, after the unresolved velocities obtained previously with other spectrometers.

In 2003–05 there was an episode when the dips were appreciably wider than they had been previously (or than those typically given by late-type stars), although there was no hint of actual resolution. In 2008, however, the dip was found to have split itself into two, with a ΔV of about 20 km s^{-1}; the components were not completely separate but they were quite well enough resolved for their individual depths and widths to be accurately determined. Fig. 6 shows the longest integration (representing about an hour of observing time) made during the season when the nodal passage occurred. Six such observations were made, one in each of the months 2008 March–August inclusive. They all provided very closely similar dip parameters — neither star showed any appreciable rotational velocity, and the ratio of the dip strengths was as 1 to 0·85, with an uncertainty of only about 2%. All of the other 21 useable observations made with the Cambridge *Coravel* have been reduced with that ratio and zero rotational velocities imposed on the calculations.

In the determination of the orbit, seven particularly closely blended observations have been discarded. Six of them indicate blending that is a bit closer even than is to be expected from the orbital velocity curves determined from the measurements whose blending is less dire. That could indicate that

TABLE IV

Radial-velocity observations of HD 130669

Except as noted, the sources of the observations are as follows:
1970–1983 — original Cambridge spectrometer; 2001–2015 — Cambridge Coravel

Date (UT)	MJD	Velocity Prim. km s⁻¹	Velocity Sec. km s⁻¹	Phase	(O–C) Prim. km s⁻¹	(O–C) Sec. km s⁻¹
1970 Mar. 22·18	40667·18	−86·9		0·174	—	—
27·16	672·16	−90·1		·176	—	—
July 11·91	778·91	−88·4		·205	—	—
12·90	779·90	−89·1		·206	—	—
1971 Feb. 14·13	40996·13	−88·9		0·265	—	—
1972 Apr. 8·10	41415·10	−90·2		0·381	—	—
1973 June 13·26*	41846·26	−88·0		0·500	—	—
1974 June 2·21*	42200·21	−88·1		0·598	—	—
1975 Mar. 4·15	42475·15	−90·1		0·674	—	—
June 7·96	570·96	−89·2		·700	—	—
1976 Jan. 28·24	42805·24	−90·8		0·765	—	—
July 28·89	987·89	−88·4		·816	—	—
1977 Jan. 30·23	43173·23	−90·7		0·867	—	—
Apr. 16·03	249·03	−91·7		·888	—	—
July 26·90	350·90	−89·7		·916	—	—
1983 June 15·94	45500·94	−86·6		1·510	—	—
2001 July 10·96	52100·96	−86·9	−89·3	3·334	−0·5	+0·8
2002 May 29·04	52423·04	−85·1	−91·7	3·423	+0·4	−0·6
2003 May 12·06	52771·06	−84·9	−91·3	3·519	0·0	+0·4
July 12·94	832·94	−85·0	−91·8	·536	−0·2	−0·1
2004 May 23·01	53148·01	−85·0	−91·8	3·623	−0·3	+0·1
2005 May 9·07	53499·07	−85·1	−91·2	3·720	−0·1	+0·4
July 18·93	569·93	−85·1	−91·1	·739	+0·1	+0·3
2006 Apr. 11·09	53836·09	−87·2	−89·3	3·813	−1·0	+1·0
2007 Apr. 6·12	54196·12	−91·2	−85·4	3·913	−0·9	+0·8
May 30·99	250·99	−90·0	−87·2	·928	+1·5	−2·2
July 7·94	288·94	−92·5	−85·0	·938	−0·1	−0·9
2008 Mar. 5·19	54530·19	−98·0	−78·3	4·005	0·0	+0·2
Apr. 8·12	564·12	−98·4	−78·9	·014	−0·3	−0·6
May 3·03	589·03	−98·1	−78·2	·021	0·0	+0·2
June 25·91	642·91	−97·3	−78·5	·036	+0·3	+0·4
July 20·91	667·91	−97·0	−79·2	·043	+0·2	+0·1
Aug. 14·86	692·86	−96·7	−79·6	·050	+0·1	+0·1
2009 Feb. 12·25	54874·25	−93·5	−82·9	4·100	−0·1	+0·2
Mar. 6·20	896·20	−92·9	−84·3	·106	+0·2	−0·9
Apr. 2·10	923·10	−93·0	−83·9	·113	−0·3	0·0
21·05	942·05	−92·6	−84·1	·119	−0·2	+0·1
May 24·99	975·99	−92·2	−85·2	·128	−0·3	−0·6
June 24·96	55006·96	−91·3	−85·0	·137	+0·2	+0·1
July 30·90	042·90	−90·6	−86·4	·146	+0·4	−0·9

TABLE IV (*concluded*)

Date (UT)		MJD	Velocity Prim. km s⁻¹	Velocity Sec. km s⁻¹	Phase	(O–C) Prim. km s⁻¹	(O–C) Sec. km s⁻¹
2011 May	13·01	55694·01	−87·2	−89·0	4·326	−0·7	+1·0
Aug.	9·87	782·87	−87·2	−89·4	·351	−1·0	+1·0
2012 Apr.	16·12	56033·12	−85·5	−91·7	4·420	0·0	−0·6
July	23·93	131·93	−85·6	−91·0	·447	−0·3	+0·3
2013 Mar.	27·12	56378·12	−84·7	−91·7	4·515	+0·2	0·0
Aug.	27·82	531·82	−84·5	−91·4	·558	+0·3	+0·4
2014 Feb.	16·21	56704·21	−84·1	−91·9	4·606	+0·6	0·0
Apr.	27·14	774·14	−84·6	−92·2	·625	+0·1	−0·3
June	13·02	821·02	−84·6	−92·0	·638	+0·1	−0·1
July	23·92	861·92	−84·6	−92·2	·649	+0·1	−0·4

* Observed with Palomar 200-inch telescope

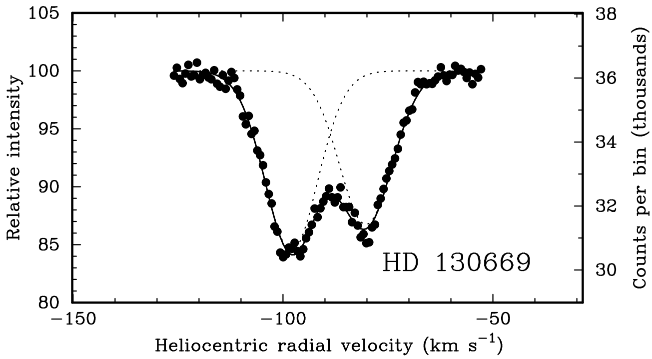

FIG. 6

Radial-velocity trace of HD 130669, obtained with the Cambridge *Coravel* on 2008 July 20 very nearly at the node of the orbit and showing the two 'dips' about as well separated as they can ever be.

the dips are actually very slightly narrower than the adopted basic profile for dips not broadened by rotation at all, but the discrepancies are so small that the effect on the reductions of less-closely-blended traces is thought to be minimal. The six observations made during the 2008 season that embraced the nodal passage, which were the only ones reduced with all seven parameters (the positions, depths, and widths of the two dips, plus the level of the 'continuum') 'free', have been double-weighted in the solution of the orbit, as befits their variances from the solution as well as acknowledging their independence; the remaining 21 accepted observations have all been given equal (single) weight. Multiplicatively, the weightings of all the observations of the secondary star have been halved. It is admittedly counter-intuitive for a dip that is only 15% smaller than that of the primary to give velocities that are only half as good in terms of their variance from the solution of the orbit, but that is what the calculations show. Disparity in the residuals of the radial velocities could easily arise from causes that cannot be identified from the observations concerned.

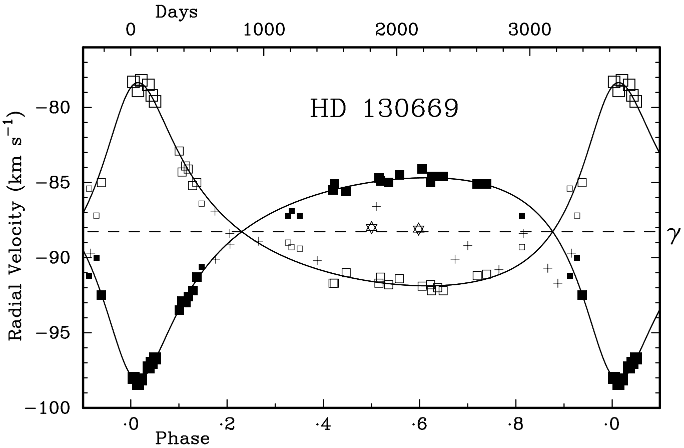

FIG. 7

Orbital velocity curves for HD 130669, with the observations plotted. Most of the measurements have been made with the Cambridge *Coravel* and are plotted as squares, filled for the primary and open for the secondary. The six large symbols of each sort represent the six observations made monthly during the season of nodal passage in 2008; they were double-weighted in the solution of the orbit. More-moderate-sized symbols plot velocities that were given unit weight. Small ones were zero-weighted, having been taken at times when the system was very nearly single-lined and might not yield reliable velocities for the individual components. Plusses plot blends, as observed with the original radial-velocity spectrometer at Cambridge; the two open stars show analogous observations made with the 200-inch Palomar telescope about 40 years ago. They have very generous signal levels, but the blending is too serious, and the procedures for disentangling it in early Palomar observations were insufficiently developed, for them to be used in the solution of the orbit.

For instance, the secondary star might suffer — or have suffered during this particular observing campaign — from more virulent star-spots than those that might plague the primary. The finally adopted orbit is plotted in Fig. 7, and its parameters are shown in the table here:

P	$=$ 3619 ± 66 days	T_4	$=$ MJD 54513 ± 11
γ	$=$ −88·27 ± 0·05 km s^{-1}	$a_1 \sin i$	$=$ 293 ± 6 Gm
K_1	$=$ 6·73 ± 0·07 km s^{-1}	$a_2 \sin i$	$=$ 294 ± 7 Gm
K_2	$=$ 6·76 ± 0·09 km s^{-1}	$f(m_1)$	$=$ 0·0763 ± 0·0029 $M_\odot$
q	$=$ 1·005 ± 0·016 ($= m_1/m_2$)	$f(m_2)$	$=$ 0·0773 ± 0·0036 $M_\odot$
e	$=$ 0·488 ± 0·009	$m_1 \sin^3 i$	$=$ 0·308 ± 0·012 $M_\odot$
ω	$=$ 163·0 ± 2·0 degrees	$m_2 \sin^3 i$	$=$ 0·306 ± 0·011 $M_\odot$

R.m.s. residual (unit weight) $=$ 0·27 km s^{-1}

Expressed in years, the orbital period is 9·91 ± 0·18, and so is in quite good agreement with those astrometric orbits that have recognized that the period is 10 and not 20 years. It must be admitted that there is a slight idiosyncrasy in the orbit found here from radial velocities, inasmuch as the mass ratio is very close to unity whereas there is (as Fig. 6 shows) a clear disparity between the strengths of the radial-velocity signatures of the components. The disparity might originate from differences either in luminosity or in spectral line-strength, but it is hard to believe that the components of a tolerably close binary could have line-strength differences unrelated to the difference in their spectral types and luminosities. It has been noted above that the ratio of dip depths (or of areas, which is the same, since the dips have similar profiles, neither having any detectable rotational velocity) is 1:0·85. Expressed in terms of stellar magnitude, it is equivalent to $0^{m}·176$. Because spectral line-strength increases towards later types, the magnitude difference between the stars (assumed both to be on the main sequence) is greater than the difference, expressed in terms of magnitudes, in dip depths, by a factor that has been empirically estimated at 1·15, making the true Δm very nearly $0^{m}·20$. That corresponds to between one and two sub-types in spectral type, and *should* imply a difference in mass of about 5%, whereas the difference in masses according to the orbital elements is only 0·5 ± 1·6%. The discrepancy is regretted, but no origin or excuse for it can be suggested. The Δm of $0^{m}·20$ found here may well be considered to be more reliable than the values of either $0^{m}·0$ or $0^{m}·5$ which have often been put forward from direct observation. The *Hipparcos* value of $0^{m}·09 \pm 0^{m}·72$, though apparently averaged from 79 observations (so it would appear that the uncertainty of an individual observation must have been $\sqrt{79}$ times as great, or about *six magnitudes*, which seems absurd!), illustrates by its large error bar the uncertainty of a Δm obtained by direct observation, whereas it is thought that Fig. 6 here gives an impression of relative certainty in quantifying the disparity of the components.

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CORRESPONDENCE

To the Editors of 'The Observatory'

Cosmology and Cosmogony

Like Alan Batten[1], I too noticed the matter-of-fact use of the word 'cosmogony' in David Hughes' review[2] of the book on the origin of our Solar System by Chambers & Mitton[3]. Batten's remarks reminded me of a series of articles 20 years ago with identical or similar author(s) and titles[4–9] — usually 'Testing Cosmogonic Models with Gravitational Lensing' — which were followed by another with the title 'Testing Cosmological Models by Gravitational Lensing: I. Method and First Applications'[10] (there doesn't seem to have been a Paper II). I remember thinking that 'cosmogonic' was inappropriate, since to me it meant 'regarding the origin of the Universe' whereas those papers discuss testing cosmological models, in this case simulations of the Universe based on different values of the cosmological parameters (*i.e.*, nothing directly to do with the origin of the Universe), in line with my view (then and now) that cosmology is 'the study of the Universe'. My understanding of the distinction between those terms was influenced by their etymology; I don't know if this distinction was standard anywhere at any time.

Batten remarked on how the meaning of the term 'cosmogony' has changed over time, the most important development probably being that it was originally applied to the origin of the Universe when it was thought to be much smaller than it is, essentially a collection of planetary systems, so the meaning later shifted to 'regarding the origin of planetary systems'. An ADS[11] search (with 'Synonym Replacement' turned off; the default is on) for 'cosmogony' in the title finds 259 works, a mixture of cosmogony in Hughes' sense (*i.e.*, planetary origins), cosmology, and cosmogony in the original sense (*e.g.*, ancient creation myths), but also such gems as 'In Furtherance of Militant Soviet Cosmogony'[12]. Searching for 'cosmogonic', on the other hand, turns up 80, mostly planetary, themes; this is also the case for 'cosmogonical', which finds 32, but among those is *The Universe and its Origins: From Ancient Myth to Present Reality and Fantasy*[13].

Searching for 'cosmology' finds 15462 and for 'cosmological' 20267. (Some of those, though, are concerned with cosmogony in the sense of the origin of the Universe.) Interestingly, 14 works can be found by searching for 'cosmologic'. That is not really an English word, and it is no coincidence that all of those appear to be by non-native speakers of English. In many languages, there is one word for the corresponding English adjectives ending in 'ic' and 'ical', *e.g.*, German '*elliptisch*' means both 'elliptic' and 'elliptical'. (Thus, a non-native speaker of English might translate '*kosmologisch*' as 'cosmologic'.) Not always do both forms exist; there are electrical engineers but no magnetical engineers, nor does anything magnetical exist at all. (Presumably, an electric engineer would be some sort of humaniform robot, perhaps a cousin of a high-energy physicist.) The rule seems to be that the basic form ends in 'ic' while later variants end in 'ical' (though 'cosmological' is an obvious exception). It is not clear to me, though, why cosmologists are concerned with elliptic integrals (used to solve the Friedmann equations) and astronomers with elliptical galaxies.

Yours faithfully,
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The Sunspot Observations by Rheita in 1642

The Maunder Minimum (MM) was a period of very low solar activity that occurred from 1645 to 1715 approximately[1]. It is the only grand minimum of solar activity that has been observed in the telescopic era and, therefore, scientists are very interested in that episode of the history of our Sun[2,3]. Hoyt & Schatten[4] provided a database of observed sunspot groups that is the only daily index to study solar activity around the MM. Some studies have tried to improve this database. For example, Vaquero *et al.*[5] added, changed, and removed some records for the period 1637–1642, just before the onset of the MM. In particular, they changed the interpretation of the sunspot observations recorded by A. M. Rheita. According to Hoyt & Schatten[4], Rheita observed eight sunspot groups from 9 to 21 February 1642. However, Vaquero *et al.*[5] showed that Rheita observed one sunspot group during 1642 June 9–22. Zolotova & Ponyavin[6] have argued recently that "in February 1642, Rheita reported eight sunspot groups, but all other observers registered a fewer number of groups. Thus, Cycle –10 can be high or in the middle." The correct interpretation of this fragment of Rheita has crucial importance in estimating the amplitude of this solar cycle just before the MM. Therefore, we show here that this record has been misinterpreted by Zolotova & Ponyavin[6], presenting the original Latin text and a modern English translation.

The original text, located on pages 242–243 of the book entitled *Oculus Enoch et Eliae*[7], and the modern English translation are the following:

Certe quod iam diximus, propria experientia, Coloniae, anno 1642, experti sumus, dum ingentem stellarum solarium turmam maiorum et minorum, per 14 dies et ultra, sibi inuicem continua serie succedentium, cum stupore, solarem discum adeo occupare vidimus, ut lux eius, maxime media et intensissima, haud leuiter illis fuerit hebetata. Nam tubo optimo, in medio Solaris disci globum perfectissime rotundum, subnigrum, pugni magnitudinem quasi excedentem conspeximus, idque directissimo aspectu; qui et per octiduum Solis haud exiguam portionem eclipsauit, maximasque aeri turbationes, utpote ventos, imbres et frigora, in medio iunii attulit. Prout crebris observationibus iam a multis annis compertum habemus scilicet fere semper aeris insigniores et magis notabiles mutationes ex dictarum stellarum solarem discum subeuntium agmine contingere et euenire.

Certainly, what we have just said [that sunlight is weakened by the appearance of sunspots] we experienced ourselves in Cologne in 1642, when, during 14 days and more, we saw with astonishment that a large number of larger and smaller solar stars, passing over the Sun one after another in continuous succession, occupied the solar disc to such extent that the light, especially the central and most intense, was not a little attenuated by them. Indeed, with a telescope perfectly suited we observed in the middle of the solar disc a perfectly round, blackish circle, almost exceeding the size of a fist, and this with total clarity; and this circle, for eight days, eclipsed not a small part of the Sun and caused major disruptions in weather, in that it brought wind, rain, and cold in mid-June. On the basis of frequent observations for many years, we have it as established that almost always the most marked and significant weather changes occur and happen from a group of the mentioned planets superimposing above the solar disc.

From the original text, it is clear that (*i*) there are not eight sunspot groups on the solar disc but eight days when one large sunspot covered not a small part the Sun; (*ii*) Rheita observed in mid-June instead of mid-February; and (*iii*) Rheita did not provide an exact number of sunspot groups on the solar disc (maybe he was referring to one large group or maybe a chain of two or three consecutive sunspot groups).

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Yours faithfully,

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REVIEWS

Cosmigraphics: Picturing Space Through Time, by Michael Benson (Abrams, New York), 2014. Pp. 321, 31 × 25 cm. Price £30/$50 (hardbound; ISBN 978 1 4197 1387 3).

In the foreword, Gingerich describes this book as "an extraordinary visual sampling of the human response to the beauty and mystery of the heavens", and indeed it is. The book is divided into ten chapters covering the familiar topics of naked-eye astronomy, although not all images are of naked-eye objects, with additional chapters on 'Creation' and 'The structure of the Universe'; the former ranges from early myths to the CMB as observed by *Planck*, the latter from antique 'world systems' to modern maps of large-scale structure. Before the table of contents is a timetable of illustrations: most chapters contain illustrations made during (usually the latter part of) the last millennium, though Chapter 7 on 'Constellations, the Zodiac and the Milky Way' covers a bit more than the previous two millennia, and Chapter 3 on 'The Moon' reaches back to almost 2000 BC. Each chapter consists of a few pages of text followed by many illustrations, presented almost strictly chronologically, each with a detailed caption. The text of each chapter provides a condensed history of the relevant field of astronomy or cosmology, in most cases without reference to the following illustrations; the historical context is sketched in the introduction. Most of the images are in colour and most are full-page (or, if the caption is on the same, as opposed to a neighbouring, page, almost so), with some covering some or most of the facing page as well. (The few not in colour are reproductions of black-and-white originals.)

There are illustrations based on myths and illustrations based on science, with the latter becoming more common as time increases throughout each chapter. There are no photographs (either of the traditional type or of the modern version, a JPEG file made from data collected by an electronic detector): most images are from the time before photography was invented, but those from the time after its invention are drawings, paintings, or computer graphics (based on observations or simulations). Most were unfamiliar to me, though there are a few classic images I have seen many times, such as Flammarion's mediæval-looking engraving of someone poking his head through the celestial sphere, Kepler's Platonic solids, Thomas Wright's galaxies, Bonestell's paintings of planets and their satellites, and the CMB as seen by *Planck*. The lesser-known images are by no means less interesting, but rather a treasure trove of pictures which depict humanity's ideas about the cosmos as much as the cosmos itself. The lack of photographs means that there is practically no overlap between this book and other coffee-table books featuring images from *Hubble*, *etc*.

Although not a text book, astronomy, cosmology, and their history are described well, though, of course, briefly. The necessary attention has been paid to proofreading and typesetting. There are neither footnotes nor end-notes and no bibliography. A two-page index is sufficient. The image credits and acknowledgements are a (large) page each of small print (like that of the index and captions; only the main text is in normal-size type): a book like this draws on the work of many people, but it is much more than just a compendium.

This is a beautiful book, very well produced, and surprisingly inexpensive considering the large size, number of pages, and number and quality of (mostly colour) images. I highly recommend it to everyone interested in astronomy, art, and/or the history of either. — PHILLIP HELBIG.

William Parsons, 3rd Earl of Rosse, edited by C. Mollan (Manchester University Press), 2014. Pp. 368, 24 × 16 cm. Price £70 (hardbound; ISBN 978 0 7190 9144 5).

Readers of *The Observatory* will know of William Parsons as the builder of the 'Leviathan of Parsonstown', the 72-inch telescope at Birr Castle in the middle of Ireland. It was the largest telescope in the world for about 70 years and led to the discovery of spiral structure of galaxies. Less well known are the circumstances under which William came to build the telescope, lived, and worked — deficiencies made good by this excellent volume which gives a well-rounded picture of a remarkable family. He was born into the Anglo–Irish aristocracy, being known as Lord Oxmantown until he succeeded his father as Earl of Rosse — readers will need to stay alert as he is referred to by a variety of names in different places in the book and, of course, the same names were used in successive generations. He was a responsible and conscientious landlord, a reluctant politician, above all a gifted engineer, and an astronomer who played a significant role in the Royal Society and the British Association for the Advancement of Science. Besides telescope building and contemporary astronomical controversy, readers will learn of Irish history at the local level, and of the early interactions of politics and science.

The often turbulent history of the family and their castle is recounted by the present Earl and Countess, who leave us with a picture of William being brought up and educated at home by enlightened parents in an atmosphere of inquiry and intellectual stimulation. The Parsons family were unusual for their class, educating their sons at home instead of sending them to board at English public schools, with the great advantage that "they avoided contamination with the all-pervasive contempt for active involvement in industry that characterised wealthy members of the establishment in Britain". The Castle workshops would have provided exposure to engineering as William's father reconfigured the castle and demesne, including building the first suspension bridge in Ireland. Later, the astronomer Robert Ball was to comment on his taste for mechanical pursuits and manual dexterity, describing him as "a skilful mechanic, an experienced founder and an ingenious optician". In due course, William provided a similar home education for his own sons — of whom the youngest, Charles Parsons, gave us the steam turbine. Their home life and the achievements of William's talented and energetic wife, Mary, are described by the present Countess. Mary was well educated, and came with a large fortune which enabled her to rebuild the Castle — and William to build telescopes. She was also one of the pioneers of photography and many of her photographs are reproduced in the book.

Although others, notably Herschel, had built large reflectors, they had not published their methods, so William had to start from scratch. Charles Mollan describes his experiments with recipes for speculum — a higher proportion of tin gave better reflectivity but very brittle metal — casting and annealing to avoid fracture, grinding and polishing, for which William constructed his own machinery. Whereas he followed Herschel in constructing an altazimuth mount for his 36-inch telescope, the 72-inch was mounted between two massive walls which supported it, but restricted observing to within a few degrees of the meridian.

William's real passion was for engineering. Robert Ball commented "... that it was more the mechanical processes incidental to the making of the telescope which engaged his interest than the actual observations with the telescope when it was completed". Nevertheless, it fell to William to make the first significant discovery accredited to it: the spiral structure of M 51. This led to a change of direction of research at Birr, from the resolution of nebulae to the study of their forms and discovery of many more spirals. Why the experienced observers Robinson and South missed the spiral structure when they observed M 51 a few weeks earlier, and the possible influence of Robinson's hostility to the 'nebular hypothesis', I will leave you to read in Wolfgang Steinicke's absorbing chapter on the observers and observations. He gives a thorough assessment of the scientific output of the telescope and also describes the physical difficulty of using it: the observer was perched high above the ground, moving the telescope to follow the target and sketching it in the dimmest light while still retaining some dark adaptation. Four men were required to assist the observing with moving the telescope and observer's gallery. William's public responsibilities reduced his own time for observing. When he did observe, as recalled by his son Randal, he used to go out to the telescope with pistols in his pockets because of the murders and thefts of arms in the region, although there was no real danger as the Parsons family was relatively popular.

The family dominated the County, holding all the powerful positions: William himself was returned unopposed as Member of Parliament when he was 21. Their local influence and relations with the sometimes frustrating townspeople are covered by Margaret Hogan, and his political life by Andrew Shields: he was initially a moderate conservative but later hardened. William served as president of both the British Association and the Royal Society although, as recounted by Simon Schaffer, the latter presidency was marked by a number of disagreements as William tried to impose his will on the Society.

Finally, Trevor Weekes gives us an evenly balanced assessment of William's successes and failures. The chapters in this book were written independently, leading to a little overlap, but this has the advantage that they can be read individually. There are fascinating stories here, and much detailed history, well referenced for the scholar. The illustrations, many of them photographs by Mary, are excellent — but I would have liked a map showing the location of telescope and castle in the demesne, and their relationship to the town. Overall, the coverage is comprehensive and the book very well edited and produced. I have no hesitation in recommending it highly. — PEREDUR WILLIAMS.

Hippolyte Fizeau: Physicien de la lumière, by J. Lequeux (EDP Sciences, Paris), 2014. Pp. 150, 24 × 16 cm. Price €19 (about £15) (paperback; ISBN 978 2 7598 1196 0).

I have to admit that until I read this biography, I only knew the name Fizeau in connection with his determination of the speed of light, and from the fact that French scientists and writers generally employ the term 'Doppler–Fizeau effect'. Both of these facets of his work are, of course, discussed in full, and Lequeux makes a good case for Fizeau to be fully recognized, given that he was the first to suggest that observation of spectral lines was the only true way of determining velocities. (Doppler maintained, to the end, that the apparent colours of stars were determined by their velocity.)

Fizeau's considerable contributions in other fields, including photography (more specifically, his improvements to the daguerreotype process), determination of the speed of propagation of electrical signals, the experiments in aether-drift, together with his seminal work in interferometry and the determination of the diameters of stars, are also covered. His initial collaborative work with Foucault, their eventual rupture, and subsequent scientific competition over the speed of light in different media is described.

The work, in French, is comprehensive, and generally free from errors: I have noted only one insignificant example, where some words are repeated in Box 6.2. I was, however, puzzled to see the infrared image of Betelgeuse obtained by *IOTA* in Arizona described as the first image of the star. I suddenly realized that I found this same error in the last book (*L'exploration des planètes*) that I reviewed for this *Magazine* (**134**, 294, 2014), and that Lequeux was one of the authors of that work. The first image of Betelgeuse — which I have been using in my own presentations for decades — was obtained by Antoine Labeyrie, using speckle interferometry in 1983. Interestingly, Lequeux mentions Labeyrie and shows his first interferometer on p. 119.

One particular feature concerning the references — which are, as one might expect, very comprehensive — is that specific symbols are used against nearly every reference to indicate where they may be consulted on-line: on the Gallica website, on Google Books, or on the ADS database — an extremely useful device, and one that I hope other writers will employ. I have already been able to use the references to locate material and illustrations for my own works and presentations. I was also delighted to discover that someone known to all meteorologists, the Dutchman C. H. D. Buijs-Ballot (Buys-Ballot), was the person who confirmed the Doppler effect — or should I say 'Doppler–Fizeau effect'? — by stationing musicians at intervals along the railway line between Utrecht and Maarsen, and putting a trumpeter on a moving locomotive.

The extremely reasonable price for this book — if only all biographies were similarly priced! — and the quality of Lequeux's coverage, encourage me to try to obtain other biographies in the same series, in particular those on Arago and Le Verrier, which he has also written. — STORM DUNLOP.

Faithful to Science, by A. Steane (Oxford University Press), 2014. Pp. 272, 21·5 × 13·5 cm. Price £19·99/$34·95 (hardback; ISBN 978 0 19 871604 4).

This book deals in a very persuasive yet gentle manner with those basic questions that have plagued the thinking world for æons concerning the meaning, purpose, and relevance of the place of *homo sapiens* in the cosmos, and in so doing it bravely tackles the famed opposition between science and religion. The cryptic title is actually slightly misleading: Steane is not arguing for the need to uphold as truth the evidence which scientific investigation incontrovertibly yields, but is probing the complementarity of science (including the scientific method) to those other realms of our wakefulness that try to express experience in intellectual and spritual descriptors as well as in physical ones. And he does that thoroughly, from many different angles. Although Steane's own background is physics, a considerable amount of cosmology is necessarily woven in, as it is obviously a dominant player in discussion of origins, universes, and evolutions.

The very personal angle from which Steane addresses this subject and all its ramifications is reflected in discussions which are exemplary for their honesty and integrity. Those qualities are the fruit of strenuous hard thinking, and when the depths were such that they could not be fully plumbed (or not within the compass of this text) the style is disarmingly deferential, even hesitant. But that adds to, rather than detracts from, the sense that here is an author who has the courage to grapple with a very tough subject, and who is not afraid to confess that some of the solutions are still beyond reach. He examines the meanings, interpretations, and (mis)applications of the concepts, practices, and 'laws' that are generally believed by even the most thoughtful mind to represent 'science', and skilfully argues away any temptation to idolize the scientific method as mentor and guide rather than as the means to information that it more truly is. He successfully defuses the tensions and conflicts which so often taint appreciations of natural science and religion, and convincingly overturns the convenient notion of a 'God of the gaps' to cover those instances where current science meets an end-stop. He also experiments with different vehicles to present an argument, using imaginary conversations, stories, and encounters, so that he can sometimes move away from the dominant first-person-singular. All in all, it is a very positive experience for the reader.

Nowhere does Steane play down or belittle either science or religion. The role of science is to describe *what*, *where*, and *when*, while philosophy and religion grapple with *why*. Big Bang cosmology may have refined its *model* of the creation of this present Universe to quite a high precision, though try as it may it cannot explain the *rationale* behind it all. Nor does the opening of Genesis stand in contradiction to the evolution of Nature as revealed through countless studies in science; there was a progression of some kind as chaos evolved into order, patterns, and cycles, and whether the descriptions of that progress take the form of art, literature, poetry, or scientific data, each contributes its own beauty and mathematical elegance towards our halting comprehension of the different facets of those origins.

The writing is fluent and clear without being simplistic. Just once or twice I felt that an argument was not pursued quite as far as it might have been, or that some definition was not quite what I expected, but such shortcomings were very probably my own, and the author does point out more than once the need to limit the scope of the discussions to the topic which is the principal theme of the book.

For anyone who sometimes puzzles over the fundamental reasons for human existence as individuals, as members of a communicating community, and possibly as purposeful elements in the whole created cosmos, Steane's book is an absolute 'must'. It is also warmly welcomed for its much-needed clarifications concerning the science–religion 'debate'. — ELIZABETH GRIFFIN.

Archaeology and Heritage of the Human Movement into Space, edited by B. L. O'Leary & P. J. Capelotti (Springer, Heidelberg), 2015. Pp. 166, 24 × 16 cm. Price £90/$129 (hardbound; ISBN 978 3 319 07865 6).

The authors and editors (who are also among the authors) of this volume feel strongly that surviving artefacts and sites connected with the beginnings of space exploration are as deserving of historic preservation as lighthouses, Franciscan missions, and sunken ships. Where the originals have been destroyed or are inaccessible, then descriptions or replicas become important. At some level, others clearly agree, since the surviving Space Shuttles now all live in places controlled by people who think the physical Shuttles are worth arguing over.

The focus and most of the authors are American, and other examples worthy of preservation include Edwards and Vandenberg Air Force Bases, Solar System probes that no longer respond (and so have become 'discarded' and hence 'archaeological' objects), and the Lunar Rovers and Apollo landing sites, but also *ArabSat* and *Raduga 1–7* (Soviet), which recently forced one South Korean and two Japanese satellites to take evasive action.

The lunar sites and objects present particular problems: the ALSEPs and other objects left on the lunar surface by the Apollo astronauts all still belong to the USA, but the bits of Moon-ground on which they rest are part of the 'Common Heritage of Mankind' according to international treaty, though one unsigned by the US, Russian Federation, PRC, and UK. Chapter author Joseph Reynolds finds two existing "historical heritage" analogues. First is the ship *USS Constitution* ('*Old Ironsides*'), because it can, in principle, move from US waters into the open sea and to ports belonging to other nations. It is actually only a replica of the gallant craft of The War of 1812*.

Reynolds' other analogous example consists of the 'Old City of Jerusalem and Its Walls', which UN Resolution 181 (while creating an Israeli state in 1947) declared to be *corpus separatum*, not part of any one country. The City and Its Walls have been on the World Heritage List since 1981. Somehow neither of those examples leaves one feeling terribly optimistic about the lunar sites or many of the other preservation goals of the 'space archaeologists'.

But let us end on a more cheerful note. Figure 6.5 has an image of an 1890 Berliner gramophone record (of Emile himself reading Schiller's *Der Handschuh*) on the left of the page, and on the right, a blow-up of an image of the grooves. It is immediately obvious that Berliner's system used lateral grooves rather than Edison's 'hill-and-dale', and I wish I had had this picture to show the class last spring when I was teaching physics of music. So clear, in fact, was the original photo from the magazine *Uber Land und Meer* that P. Feaster of the University of Indiana Media Preservation Initiative has been able to scan the groove images and recreate the audio, which is to be found at http://mediapreservation.wordpress.com /2012/06/20/extracting-audio-frequencies-from-pictures/

How, you may ask, does this connect up with *Archaeology and Heritage of the Human Movement into Space?* For this you will have to read the book: I hesitate to say "buy" for it is creeping close to the $1-per-page price that may sadly soon become the norm for scholarly books. The web site was working on 2015 February 23 and has snippets of recorded sound in English, French, and Italian, as well as the German item. In no case, sadly, are the sounds clear enough to say whether typical accents in the languages have changed in 150 or more years. But I continue to find old sounds more numinous than any visual heritage, except, perhaps, high-resolution black-and-white photographs of people and buildings. — VIRGINIA TRIMBLE.

Beyond: Our Future in Space, by C. Impey (W. W. Norton, London), 2015. Pp. 298, 23·5 × 15·5 cm. Price £16·99 (paperback; ISBN 978 0 3932 3930 0).

With the 'space age' more than half a century old, many observers consider that the optimism and daring displayed by the early pioneers have disappeared, to be replaced by bureaucracy and conservatism. Many of the current launch vehicles are based on missile technology developed in the mid-20th Century. The reusable Space Shuttle has come and gone, to be replaced by a souped-up version of the Apollo command module that flew to the Moon more than 40 years ago. The technological marvel known as the *International Space Station* has struggled to justify its $1 billion price tag, and it is likely to be replaced by much smaller, less-sophisticated structures.

*Yes, your reviewer can recite the whole poem, from "Aye, tear her tattered ensign down ..." to "… Give her to the God of storms, the lightning and the gale". You might suppose that author Oliver Wendell Holmes was not interested in heritage sites or historic preservation, but the poem was actually written as part of a fund-raising effort to recondition the original ship.

On the other hand, there are reasons for optimism about the future of space travel, and author Chris Impey definitely belongs to the optimists' camp. After a fairly standard overview of the pioneering work of visionaries such as Tsiolkovsky and Goddard, he moves on to the current crop of entrepreneurs — Rutan, Musk, Branson, and others — who are seeking to replace traditional, taxpayer-funded efforts with less costly, more efficient, commercial enterprises. Impey also considers the rise of China as a space power, so that it will soon become a rival to the present dominance of the United States. Looking into the near future, he examines potential exploration and utilization of the inner Solar System and beyond, describing technologies such as solar sails, space elevators, and robotics. Improvements in the search for habitable exoplanets are also covered. The final section is devoted to speculation about the existence of intelligent extraterrestrial life and humanity's possible emergence as a star-faring species in the far distant future. Much of this is necessarily speculation, but Impey gazes into his crystal ball to discuss concepts such as nanotechnology, self-assembly and self-replication, human hibernation, star-ship propulsion, and teleportation.

Impey's engaging text is easy to read and comprehend, while offering a positive prognosis for humanity's future in space. The short subsections make it easy to dip into, although, at times, I wished that subjects such as SETI were covered in one comprehensive discussion. Not everyone will agree with him, but Impey ends on an optimistic note: "These activities can let us be more than a footnote in the history of the Milky Way. Exploration is built into our DNA; we should not resist." — PETER BOND.

Alien Skies: Planetary Atmospheres from Earth to Exoplanets, by F. J. Pont (Springer, Heidelberg), 2014. Pp. 151, 23·5 × 15·5 cm. Price £16·95/$29·99 (paperback; ISBN 978 1 4614 8553 7).

Author Pont begins with the air of Earth as it is now, cycles through (or rather just above) Venus, Mars, Titan, giant planets, hot jupiters, terrestrial exoplanets, and finally back to Earth to explore the history of our atmosphere. Fascinating text is interspersed with fascinating figures: for instance, our global ocean currents, as they are now, as they were before plate tectonics so ruthlessly dismembered Pangaea, and as they would be if we could build a dam from the tip of Tierra del Fuego to the Antarctic Peninsula, 600 km away. The author suggests that that would, by interrupting the current that now circles Antarctica, melt the south polar ice, making the southernmost continent habitable (though of course drowning much now-inhabited land). Call it terraforming Earth, if you wish.

Of course, there are a few glitches. Fig. 5.7 has the Sun looking smaller from Venus than it does from Earth, and absolute zero seems reachable on page 63. Quantities of gases and liquids on planets are described in the 'kglt' system of units (kilometres, grams, litres, and tonnes, meaning metric tonnes). Artists are credited, but not models. I recognized Marilyn Monroe (Norma Jean Baker) in connection with carbon and diamond planets, but not the long-haired, unclothed Venus.

Every planet and type has features likely to surprise you, but it is the explanations of what happens on Earth that are probably most worth carrying away. Heating in the ozone layer is directly responsible for atmospheric currents rising no further, thus confining weather to the troposphere (a word the author uses sparingly). The funny shape of a water molecule is what makes it so much better a solvent than liquid methane or carbon dioxide. The submersible that filmed the remains of the *Titanic* was a Russian MIR. We are told that the hottest and coldest days in Britain lag the solstices by about two months because of the surrounding oceans. This is not a good fit to the extended very hot stretches of October weather in coastal Southern California, but then the author is at the University of Exeter.

Elsewhere in the Solar System, Venus probably had plate tectonic cycles for a gigayear or so, a gigayear in the past, though what caused her slow rotation is not addressed (unless it is that long hair). More recently, the fate of the *Galileo* probe, as it descended more and more slowly into Jupiter, sounds like something from a horror film. And, if you like 'first causes', the absence of any Solar System planets with masses between 1 and 14 Earths is because water was about 15 times more abundant than rocks and metals in the protoplanetary disc.

I also recorded a dozen or so each of "oops", "maybes", incomplete descriptions, inventories, credits, and so forth. But if I could make one change, it would be an accurate entry in the index for all the times that plate tectonics is mentioned in the text. If it is any consolation, we do not have enough fossil fuel that burning it all would Cytheriform Earth, and the period of anomalous heating will be short by cosmic standards. — VIRGINIA TRIMBLE.

Atlas of Great Comets, by R. Stoyan, translated by S. Dunlop (Cambridge University Press), 2015. Pp. 224, 32·5 × 25 cm. Price £35/$55 (hardbound; ISBN 978 1 107 09349 2).

Memories of astronomical events are inextricably mixed with those of a personal nature. Thus I recall P/Halley and Hale–Bopp from Spain: there, one also remembers dodgy roads and hire cars. And for Shoemaker–Levy 9 I had the pleasure of watching its impact scars upon Jupiter at the eyepiece of Lowell Observatory's *Clark* refractor in the company of the Shoemakers. Stoyan's atlas certainly evokes rich memories of great comets, and its generous format enables its many superb full-page illustrations to be reproduced to great effect.

I have quite a few books about comets in my library, going back to Guillemin's lovely French work of the 19th Century, but most are of an academic nature, where orbital parameters or physical constitution rate more highly than pictures. So it was with anticipation and pleasure that I opened this book. Offhand, I cannot think of anything comparable on the current market, so the present work fills a gap, and being reasonably priced it ought to have wide appeal.

Stoyan has described 30 great comets going back to 1471, including several returns of P/Halley. The last is Comet McNaught of 2007. The illustrations portray the objects (and sometimes the discoverers), and consist of modern images, photographs, drawings, paintings, and engravings. Many of the earlier illustrations are rare and a great many were new to me, especially the ones from German sources (which figure prominently, as one would expect from a translation of a work originally published in that language). It was good to see the UK represented with a classic photograph by R. L. Waterfield of Comet Arend–Roland. For each object there are details of discovery, orbital data, visibility, and the public reaction. The latter is often extremely interesting. Stoyan himself is a comet observer and has included nice drawings of his own to show the tail and inner coma of Hale–Bopp: I well recall following those intricate semi-circular 'shells' over weeks and months. For an illustration that beautifully shows a modern comet as a 'hairy star', page 201 has an unsharp-masked view of Hale–Bopp.

The atlas is not just a collection of great comet pictures. The introduction is thorough, and includes a summary of modern cometary science, as well as old beliefs and fears, comets in art and in literature, the relation between comets and meteor streams, and cometary missions. Asides include the hoax of 'Barnard's automated comet search engine', and the Kreutz Sungrazers. There is a list of the most successful comet hunters. The book concludes with a glossary, and an extensive bibliography and reference section. There are specific references to the great comets, but oddly Comets Ikeya–Seki (1965), West (1976), P/Halley (1986), and the Great January Comet of 1910 are not specifically referenced further there.

The text is excellent throughout. I enjoyed dipping into it and am glad to be adding it to my collection. — RICHARD MCKIM.

What Does a Black Hole Look Like?, by C. D. Bailyn (Princeton University Press, Woodstock), 2014. Pp. 224, 21 × 13·5 cm. Price £24·95/$34 (hardbound; ISBN 978 0 691 14882 3).

This is a nicely-presented account of much of what we know about black holes. The level is appropriate for upper-undergraduate to introductory-graduate students. The book explicitly assumes a knowledge of (US) college-level physics and mathematics, and includes simple mathematical derivations of some of the key results, particularly those properties of black holes which are important for accretion and makes them accessible to observation. The emphasis throughout is on what we can deduce directly from observation. That is sensible, since so much of accretion theory remains uncertain. The author is an expert in the observational study of stellar-mass black-hole binaries, and gives an authoritative summary of how we know what we know. The book takes a reasonable approach in discussing more controversial topics such as ultra-luminous X-ray sources, and the possible existence of intermediate-mass black holes. It gives readable accounts of what it calls black-hole exotica, meaning things such as Hawking radiation, wormholes, and multiverses.

This book is obviously very suitable in providing an overview for advanced undergraduate courses covering black holes and high-energy astrophysics, but professionals may also find it useful for priming themselves for questions from journalists or the public on the exotica that seem to catch the popular imagination. — ANDREW KING.

Atlas of Meteorites, by M. Grady, G. Pratesi & V. Moggi Cecchi (Cambridge University Press), 2014. Pp. 373, 28·5 × 22·5 cm. Price £95/$150 (hardbound; ISBN 978 0 521 84035 4).

This substantial, handsome book provides an excellent pictorial explanation of the differences and similarities between meteorites. The book consists of an introductory chapter, which is clearly written and focusses mainly on mineralogy and chemistry relevant to classification of meteorites. This is followed by chapters on each meteorite group in turn. Individual meteorites are given one page each, and three optical-microscope photographs of each meteorite are provided, typically one of the meteorite in plane-polarized light, one in cross polars, and one in reflected light. A small amount of text is also included to explain the unique features of the meteorite and to give some guidance about what can be seen in the images. Mineralogy and petrology are highly visual subjects, and a methodical collection of relevant images such as this is the best way to learn the topic.

It is a fairly comprehensive book and its coverage of the achondrite groups is particularly complete and systematic. My only gripe is that the chapter on iron meteorites is disappointingly small and incomplete; the reader is referred, unhelpfully, to an out-of-print book for more information. Perhaps a Volume 2 of this book, focussed on the iron meteorites, would be helpful.

The photomicrographs are beautiful and the figures in the introductory material are also bright and attractive. This book is an excellent read for all serious scholars of meteoritics, and in the Natural History Museum it has already been put to good use in training students in meteoritic petrology. However, it is so colourful, and the images often so aesthetically appealing, that it would also make an excellent coffee-table book (albeit for a rather erudite person's coffee table).

Overall I recommend this book highly for its systematic and graphic approach to meteorite classification and feel it is an essential buy for all meteorite researchers. The images would be appreciated by a wider readership as well. — SARA RUSSELL.

Theory of Stellar Atmospheres, by I. Hubeny & D. Mihalas (Princeton University Press, Woodstock), 2014. Pp. 923, 23·5 × 15·5 cm. Price £103/$150 (hardbound; ISBN 978 0 691 16328 4), £62/$89·50 (paperback; ISBN 978 0 691 16329 1).

Surely there can't be any practising astrophysicist with an interest in stellar atmospheres who hasn't, at one time or another, turned to 'Mihalas'. Some four decades after publication, the two editions of his *Stellar Atmospheres* (1970 and 1978, with significant changes between them) remain the standard monographs in the field, and the prices at which these volumes now change hands further testify to their lasting utility.

A new text, in collaboration with Ivan Hubeny, has been in the works for a very long time, and it's a source of regret that Mihalas' failing health appears to have provided the impetus to bring the work finally to completion.* Although originally conceived as a 'third edition', and largely following the development of topics in *Stellar Atmospheres*, this is a completely new book. It not only thoroughly documents fundamental physical principles, but also explores the numerical techniques which, alongside the growth in raw computing power, have enabled the progress in modelling that has come about since the publication of *Stellar Atmospheres*, from simple few-level atoms to today's fully line-blanketed non-LTE models (exemplified by Hubeny's own code).

Each of the two introductory chapters, 'Why study stellar atmospheres?' and 'Observational foundations', is a self-contained essay that offers a taste of the clarity and authority that pervades the entire book. Each is accessible at undergraduate or advanced amateur level, and can be commended to those readerships (the first of these texts may be freely downloaded from the Princeton University Press web-site). There follows a comprehensive exposition of the properties of matter and radiation, and their interactions — definitely not, for the most part, for those with only a casual interest. The treatment exudes rigour; for example, radiation is described in vector and tensor terms (although polarization isn't explicitly discussed). However, the authors' warning that "the reader should be familiar with the elements of quantum mechanics

*See the obituary in *The Observatory*, **134**, 92, 2014.

and special relativity" seems to me relevant only to those undertaking a cover-to-cover reading.

The basic physics — absorption, emission, and scattering processes, cross-sections, line broadening, and so on — are dealt with thoroughly. 'Model atmospheres' doesn't appear as a specific heading until Chapter 17†, wherein LTE model atmospheres are discussed, starting with the grey atmosphere, and including line blanketing. After that a chapter on classical (*i.e.*, plane-parallel, hydrostatic) non-LTE models discusses complete linearization, the implementation of accelerated lambda iteration, and the use of superlevels in treating non-LTE line blanketing. With these issues out of the way, an examination of extended and expanding atmospheres, and stellar winds, completes the main body of the text.

I haven't gone through every line of every page, but having read much of the book, and having scanned all of it, I have yet to notice a single textual error (even if I wish the book title had started with a definite article). I suppose that if one were to look very hard for some capricious criticism, it might be that both authors' principal research interests lay in the atmospheres of early-type stars. As a result, topics relevant to atmospheres in which convection and molecular opacities are important are perhaps treated in somewhat less depth than other material; but that scarcely limits the scope of this treatise.

This is a great brick of a book (around twice the length of the first edition of *Stellar Atmospheres*), with not an ounce of fat in it (three sections — out of 114 — are flagged to indicate that they "may be omitted on a first reading"), and yet the text is consistently lucid. The construction and organization facilitate the use of substantial sections as a basis for creating or revising graduate or advanced undergraduate lecture courses in a range of physics topics in addition to stellar astrophysics, and the book will necessarily find a home on the shelves of anyone actively working in the field. It is hugely authoritative, with nearly 1200 references to the primary literature in stellar atmospheres (and another hundred to papers describing opacity calculations), alongside a 15-page glossary of symbols. A magisterial work that will surely be the definitive reference for many years to come. — IAN D. HOWARTH.

Very Massive Stars in the Local Universe, edited by J. S. Vink (Springer, Heidelberg), 2015. Pp. 268, 24 × 16 cm. Price £90/$129 (hardbound; ISBN 978 3 319 09595 0).

For quite a while, the conventional wisdom expounded in introductory texts was that the Eddington limit constrains the formation of normal single stars to masses less than about 100 $M_{\odot}$, although this view has to be modified for first-generation stars, in which essentially zero metallicity (among other factors) drives much higher masses; and large- and small-scale anisotropies can allow the Eddington limit to be overcome. The paradigm shifted significantly in 2010, when the application of modern stellar-atmosphere codes to improved observational material resulted in an upward revision of previously accepted values for the luminosities of stars in the dense LMC cluster R136, resulting in a persuasive case for ZAMS masses of up to ~ 300 $M_{\odot}$.

The grounds for, and consequences of, these matters were aired at a Joint Discussion on 'Very massive stars in the local Universe' at the Beijing General

†Starting at page 569; the first edition of *Stellar Atmospheres* ends 100 pages before that!

Assembly, from which this book evolved. It is not a record of the JD, but a collection of eight reviews covering all aspects of Population I stars with masses greater than about 100 $M_{\odot}$. Although the number of such stars securely identified remains small (of course, this is one of the topics addressed), as extreme objects they illuminate a range of interesting questions in high-mass star formation, evolution, and death, taking in stability, mass loss, and structure.

The individual contributions are individually authoritative and clear, and come together as a well-integrated whole. The use of colour figures enhances the presentation significantly, so it's a shame that not every author took full advantage of the opportunity; it's also a pity that one of the contributions has an eye-catching error in its title ("Preupernova Instability" — obviously not eye-catching enough ...). However, these very slight reservations don't detract significantly from a timely, authoritative, and well-presented monograph. — IAN D. HOWARTH.

Resolving the Future of Astronomy with Long-Baseline Interferometry (ASP Conference Series, Vol. 487), editied by M. J. Creech-Eakman, J. A. Guzik & R. E. Stencel (Astronomical Society of the Pacific, San Francisco), 2014. Pp. 414, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 858 9).

The first thing that hit the eye of the reviewer is that this volume took over three years to come to print. In many branches of astronomy that might condemn it as being no longer of current interest. In the realm of long-baseline interferometry, however, it is also a case of long time-scales. The instruments that feature in this series of practical observations take many years to come to fruition — the *Sydney University Stellar Interferometer* (*SUSI*), for instance, started in 1991, and has not yet reached its designed maximum baseline. However, that does not necessarily protect them from the vagaries of funding. In his paper, Stephen Ridgway compares the situation in optical-interferometry research in the US and Europe and bemoans the lack of a current clear mandate, which is helping, as he sees it, to throttle development and scientific operations on the other side of the Atlantic. Since the meeting, the *Keck Interferometer*, for instance, has ceased operations, but fortunately the CHARA *Array* operated by Georgia State University goes from strength to strength and its list of papers grows apace. It is the source of many of the contributions to this meeting, and continues to derive stellar parameters such as masses and diameters with admirable accuracy.

Although the meeting was held at the headquarters of the upcoming *Magdalena Ridge Observatory Interferometer* in New Mexico, it was disappointing not to read a progress report on that exciting development, and, at the time of writing, even the website contains no news less than 15 months old, and that being that the first unit telescope (out of ten) was now on site. As a matter of interest the *SUSI* website was checked and found to have been last updated 2011 May.

The volume, like the meeting, is divided up into seven sessions, with one on posters at the end. The sessions divide up into four on stars, one on exoplanets, one on black holes, and one on new challenges, and amount to 242 pages divided between 20 papers, whilst the posters include 26 contributions amounting to 166 pages. One paper was found that did not appear to have any connection with long-baseline optical interferometry — at least it was not obvious.

What were the most exciting prospects? The plan to detect directly hot jupiters using the *MIRC* instrument of CHARA, the possible resolution of accretion discs around super-massive black holes at the 0·005-parsec scale by an upcoming second-generation instrument on *VLTI*, and the dynamics of binary asteroids which can be resolved in the infrared by 10-metre-class telescopes.

Gripes? There are several instances of colour diagrams which are produced in black and white but whose figure captions still contain references to the various colours. There is a list of authors, but not of participants, and there is no subject index. — ROBERT ARGYLE.

An Introduction to Galaxies and Cosmology, 2nd Edition, edited by Mark H. Jones, Robert J. A. Lambourne & Stephen Serjeant (Cambridge University Press), 2015. Pp. 448, 26·5 × 21 cm. Price £39·99/$74·99 (paperback, ISBN 978 1 107 49261 5).

This is an undergraduate-level introduction to the Milky Way, other galaxies, and cosmology. In my own studies at the University of Hamburg, I had a one-semester introduction to astronomy and astrophysics and later one-semester courses on each of the topics of this book (and many other courses as well), thus I never took a course which could have been based on a book like this. Covering all the material in this book would probably take a year at the usual pace of an undergraduate course. There is also a companion volume, *An Introduction to the Sun and Stars*, by Simon F. Green & Mark H. Jones. All four editors (of both volumes) work at The Open University, so it can be expected that the two volumes complement each other well. Together, they provide a detailed introduction to almost all of astronomy, astrophysics, and cosmology. (The authors of this volume are listed on the title page, together with names of people (presumably authors of the first edition) whose material has been updated for inclusion here.)

This volume, though, becomes more detailed as the scale of the topic increases. There are seven chapters of roughly equal length; the final chapter, dealing with current problems in cosmology, is about half the length of the others. The Milky Way gets just one chapter. Two chapters are devoted to galaxies: normal and active. The fourth chapter on the spatial distribution of galaxies already touches on some topics often considered to be part of cosmology. Four chapters are devoted to cosmology: an introduction, the hot Big Bang and the evolving Universe, observational cosmology, and the final chapter mentioned above. On the other hand, this division probably reflects roughly the relative amount of knowledge in the respective fields.

The book makes a busy impression, in a positive sense. Only a minority of the pages consists only of text. Most have at least a figure (often with an extensive caption), a numbered box providing more details on a topic, an unnumbered box (always containing a figure, which is numbered in sequence with the other figures) with details on one or more persons (there are also standalone pictures of people), a numbered question (with answer at the back of the book), an unnumbered question immediately followed by the answer (usually shorter than in the case of the numbered questions), an equation (important ones highlighted by a yellow box), a marginal note (usually something which in other books would be in parentheses, in a footnote, or in an endnote), or highlighted text. There are ample margins, large enough for the reader's own notes. Apart from the marginal notes mentioned above, the margins are also used for figure captions (except within boxes, which take up the full width of the page) and sometimes the corresponding figure as well. In such cases, occasionally the caption and sometimes the figure are wider than the normal margin width and the main text correspondingly narrower. Many aspects of the book's structure are denoted by pastel colours, which contrast with the usually brightly coloured figures. (Occasional black-and-white figures are reproductions of black-and-white originals.) Paragraphs are set in the unusual ragged-right style, although a proportional font is used. This is uncommon these days but not a distraction, as is the fact that parts of the book are written in the second person (*e.g.*, "you may have noticed"). The last section of each chapter is a subdivided and itemized summary, followed by further questions (with answers at the back).

The text is very up-to-date, including discussion of such current topics as results from *Planck*, the multiverse, and high-resolution numerical simulations. Most textbooks become out of date after some time, those about astronomy faster than many others, but some are badly out of date even when they appear. Fortunately, that is not the case here. I didn't notice any factual mistakes, and there are practically no typographical errors or other signs of lack of sufficient editing. One small gripe: I think it is confusing, especially in view of the target readership, to talk of measuring the deceleration parameter q_0 *via* the m–z diagram for type-Ia supernovae, since the degeneracy in the joint constraints on λ_0 and Ω_0 thus obtained does not correspond to a particular value of q_0 (which is $\Omega_0/2 - \lambda_0$), but rather, roughly, to $\Omega_0 - \lambda_0$. Historically, of course, the m–z relationship was used in attempts to measure q_0, which is a useful parameter at low redshift since it corresponds to the first-order correction to a linear m–z relation, and these are discussed; perhaps the 'inertia' of this historically important test carried over into the discussion of more modern results. One could just as easily quote a value for q_0 as measured from the CMB, but fortunately this is hardly ever done, for the same reason as it shouldn't be done when discussing modern results from the m–z relation. (Two-dimensional constraints in the $\lambda_0 - \Omega_0$ plane from these observations are discussed in the next section (mainly about results from the CMB), both alone and in combination with other observations, and this is even called "the best way" and it is noted that the original results were thus presented; this makes the discussion of modern measurements of q_0 even more puzzling.) But, again, this is my only real gripe about an otherwise excellent book.

I think there could have been more pictures of astronomers, within unnumbered boxes in some cases; these would still take up only a small portion of the book. Although I have no objections to those included, the selection seems somewhat random. There are a large number of figures: photographs, plots, diagrams. Those in the latter two groups are all in a uniform style; some are adapted from other works. All of the figures are of high quality and overall the book makes an excellent impression and is a joy to read. There are neither footnotes nor endnotes. The main text is followed by answers to and comments on the questions in the text; a three-page appendix of useful quantities, units, and mathematical functions; a 19-page glossary (terms in the glossary are in boldface at first appearance in the text); a page of suggestions for further reading; six pages of acknowledgements (mostly picture credits); four-and-one-half pages of references for the figures reproduced from elsewhere (but, as with many textbooks, no complete list of references); and an 11-page index which emphasizes terms appearing in the glossary (bold), or in figures or tables (italics).

I wholeheartedly recommend this excellent book to anyone interested in a detailed introduction to galaxies and cosmology, from the Milky Way to the multiverse. — PHILLIP HELBIG.

Galactic Encounters: Our Majestic and Evolving Star-System, from the Big Bang to Time's End, by William Sheehan & Christopher J. Conselice (Springer, Heidelberg), 2014. Pp. 385, 28·5 × 21·5 cm. Price £31·99/$44·99 (hardbound; ISBN 978 0 387 85346 8).

The leading author of this splendid book, William Sheehan, is an MD in psychiatry at the University of Minnesota Medical School. He has a lifelong interest in the history of astronomy and he is an accomplished observer. His co-author Christopher Conselice (University of Nottingham) researches galaxies in the early Universe. Together they have written a majestic account of the evolution of the Universe, from the Big Bang to the unfathomable future. But so have lots of authors before them, so what's new here? The answer lies in the narrative style, which places great emphasis on the lives and motives of key practitioners, who made breakthroughs as a result of instrument development, or taking risks, or being committed independent thinkers with a single goal in mind. Sheehan is an historian of considerable stature, whose enquiring mind probes deeply into archival sources, all of which are fully referenced. His background in psychiatry has no doubt contributed to the riveting accounts of the personalities and motives of many remarkable astronomers who have devoted their careers to extragalactic realms.

A handful of introductory chapters begin the story of the discovery of the galaxies through the eyes of Galileo, the Herschel family, the Third Earl of Rosse, Warren De La Rue, and the Hugginses, after which the pace quickens. Geographically the focus shifts from the 'Grand Amateurs' in Victorian England to the United States, where the funding of telescopes and observatories was in the hands of businessmen who had made their fortunes in the tremendous economic boom in the late 1800s and early 1900s: Percival Lowell, James Lick, Andrew Carnegie, Charles Yerkes, and John D. Rockefeller, for example. The great discoveries that flowed from this largesse are of course well known. However, the strength of this book is to be found in the detailed biographical accounts of major and minor practitioners: E. E. Barnard, V. M. Slipher, W. W. Morgan, G. E. Hale, G. Lemaître, B. Tinsley, and many others. There are 186 illustrations, including many not often seen, and the production is of high quality. Sheehan & Conselice have produced a meticulously researched masterpiece on the talented individuals who first explored the extragalactic Universe. — SIMON MITTON.

Galaxies in 3D across the Universe (IAU Symposium 309), edited by B. L. Ziegler, F. Combes, H. Dannerbauer & M. Verdugo (Cambridge University Press), 2014. Pp. 274, 24·5 × 16·5 cm. Price £76/$125 (hardbound; ISBN 978 1 107 07866 6).

The title does not mean the distribution of galaxies through 3-dimensional space, but rather the 3-dimensional distribution of matter in individual galaxies, at both low and high redshift. In either case, what is required is grids of images, and spectra covering some portion of the sky, and integral-field spectroscopy has become a vital tool.

The proceedings consist of five unequal parts — instrumentation, nearby galaxies, feedback and environment, high-redshift galaxies, and posters. The demographics were typical of recent IAU symposia: lots of folk from the host and nearby countries and participants by ones and twos from more distant spots, like Mexico, Taiwan, and South Korea. Indices of objects, topics, acronyms, facilities, and algorithms would have made the proceedings enormously more useful, even for those of us who were there. These things are easy to do (based on experience of having done a few) but no one now seems to bother.

Why would one want an index of acronyms? In reading I spotted some charming ones, but neglected to record the pages on which they were mentioned, and so cannot tell you where to look to find out what is done with FitSKIRT, MIRAGE, SLUGGS, MUPPI, TiRiFiC, HEROES, or *VIRUS-W*. Other mysteries remain: the editors' introduction says 218 participants but also about 300 interested astronomers. That the number of authors (450) is larger than either of those has become typical. The reader is also told that participants chose three favourite posters, whose presenters were given the opportunity of speaking briefly. Ah, but who were they? My notes say Sylvia Ploeckinger *et al.*: 'Simulations on the survivability of tidal dwarf galaxies' was a really neat poster (it had some moving parts). But the others are not obvious.*

As long as my notes are out, scientific highlights included (*a*) mergers of gas-rich galaxies can indeed produce ellipticals and such, but can also reform into disc galaxies, (*b*) disc galaxies are mostly baryons near the centre and mostly dark matter far out (the issue used to be called "maximal disks"), and (*c*) even seemingly isolated galaxies can experience in-falling satellites. In case you should want to look these up in the proceedings, the first authors are F. Hammer (observations), S. Courteau (data compilation), and I. Fuentes-Carrera (observations). Each of these is a partial answer to some very old questions in extragalactic astronomy.

If you had been in Vienna, you could also have discovered from the book exhibit that volume 6 alone of a six-volume reincarnation of the Kuiper Compendium costs €405·53, or had lunch at a cafe that now occupies the ground floor of the building where the former treasurer of the American Physical Society was born in 1925 (at 6 Schottengasse; there is a Starbucks at No. 9), or spent an afternoon at the Schönbrunner Tiergarten (zoo), a truly irresistible place, unless you disapprove of all zoos.

Conflict of interest statement: my copy of the proceedings is one of those that came with registration fees, but the LOC generously waived my fee in return for an historical talk on early 3-D studies of galaxies, so I guess it was a freebie. — VIRGINIA TRIMBLE.

The Cosmic Microwave Background: How It Changed Our Understanding of the Universe, by Rhodri Evans (Springer, Heidelberg), 2015. Pp. 214, 23·4 × 15·4 cm. Price £31·99/$34·99 (paperback, ISBN 978 3 319 09927 9).

This book is part of Springer's *Astronomers' Universe* series, which aims somewhere between the popular-science book which begins with the basics

*Further perusal of notes yields A. J. Battisti (mid-IR star-formation-rate indicators), D. N. A. Murphy (a late-type spiral transforming into an early-type elliptical), and C. C. Thöne (resolution of NGC 2770, host galaxy to three SN Ibs), with the Ploeckinger *et al.* poster already having been selected as a highlight by the organizers.

(making the corresponding parts of such books too familiar to those who have read more than a few) and more technical works, though whether the latter are really "not intended for enjoyable reading" as Springer claims[1] is at least debatable. This book, by Cardiff astronomer and blogger[2] Rhodri Evans, hits the mark here — it provides a good overview of the CMB to those with a basic knowledge of physics and astronomy. Books by experts in a field sometimes explain too little, or too much; perhaps the fact that the topic of the book is not Evans's main field of research allows him to see the forest and not confuse the reader with detailed descriptions of a few trees. Although, in line with the aim of the series, no attempt is made to give a thorough introduction to astronomy and cosmology, the first two chapters do provide some context, the first an overview of astronomy from Ptolemy to the beginning of the 20th Century and the second a summary of cosmology from Kapteyn's ideas to the discovery of the expansion of the Universe. The remaining five chapters cover the basics of the CMB (both theory and observation), *COBE*, ground-based observations (mostly from Antarctica), the emergence of the concordance model from several independent lines of evidence, and up-to-date discussion of such instruments as *Planck* and *BICEP2*.

The text itself is pleasant to read and the book is about the right length for the discussion of this topic at this level. It is clear that Evans not only understands the material but knows how to present it well. The explanation of the cosmological constant and its possible relation to dark energy is better than in most popular works, many of which make this topic seem too mysterious and/or don't give a correct account of how thinking on the subject has changed within the cosmological community over time. Evans includes some personal details in relation to some of the events, which makes for a more enjoyable narrative, as do historical 'detours' on such topics as 18th-Century observing expeditions and the exploration of Antarctica. I didn't notice any serious factual errors, though there are a few minor ones which are probably due to lack of editing (whether on the part of the author or the publisher I don't know). My only major complaint is that the book is badly edited. Although some things are arguably matters of taste, such as Evans often using a comma where I would use a semicolon (and one of his rare semicolons I would replace with a comma!) and Springer's notorious lack of punctuation after captions (good if the caption is just a phrase, but not in the case of a full sentence or several full sentences), others involving punctuation and word order are more annoying, especially since they can affect the meaning (though the intended meaning is usually obvious). Most annoying are careless typographical errors, of which there are very many, such as mis-spellings, wrong and/or swapped words, and bizarre constructions such as "Isle of White", and "1" instead of "one" as an impersonal pronoun. Those in the last category are almost certainly not mistakes made by the author but rather stem from incompetent editing on the part of the publisher, probably due to a non-native speaker and/or some sort of correction software. Somewhat strange are the variable bottom margins. In most cases, this is probably caused by the (automatic) placement of figures, but in some there is no obvious cause.

There are 94 illustrations (photos, graphs, and diagrams), 64 in colour. All are within the text, not on 'plates', and the quality, like the overall production quality, is fine. Credits are given in the captions, which is more useful if one really needs this information. There are neither footnotes nor endnotes to distract from the easy-going style. There is no index, but I don't think the book needs one. The glossary of technical terms might be helpful to some. The text contains numbered references which are listed at the end of each chapter in order of first appearance. This is somewhat unusual for a book at this level, but a useful aid for those seeking more detailed information, in line with the aim of the series to be between typical popular books and more advanced works. URLs are provided for older references, which is a nice touch.

I recommend the book, especially for the target readership of the *Astronomers' Universe* series, despite the fact that the sloppy editing will probably annoy some readers. — PHILLIP HELBIG.

References

(1) http://www.springer.com/series/6960
(2) https://thecuriousastronomer.wordpress.com

Biographical Encyclopedia of Astronomers, 2nd Edition, edited by T. Hockey *et al.* (Springer, Heidelberg), 2014. Pp. 2434 (in 4 volumes), 25 × 16·5 cm. Price £773·50/$1200 (hardbound; ISBN 978 1 4419 9916 0).

One's understanding of the physical Universe along with all the subtleties involved is never complete without the knowledge of the countless endeavours by the brightest minds in human history at achieving them. Standing on the shoulders of giants is not just taking an idea and advancing on where somebody left off but is to also to comprehend the path that led to such accomplishments. That understanding is not only a lesson in scientific evolution and discovery but is also a life lesson of the endeavour by those great minds and a great legacy of what makes us proud in calling ourselves human beings.

The *Biographical Encyclopedia of Astronomers* four-volume series gives a quick, yet comprehensive, biography of astronomers from the most influential figures such as Kepler and Hubble to the least remembered scholars that we might never have heard of had it not been for this series, and it spans a very wide range in historical time from antiquity to the modern era. The descriptions are very readable and easy to follow. One very appealing feature is the connections given in each biographical entry to scholars that have influenced in any way the character under discussion. They range from tutors to students to influential characters in generations earlier or later. This makes it very straightforward to pick up an argument and follow it through its historic course, and is what makes these books a very valuable resource on the history of astronomy, complemented by the wealth of references given at the end of each entry. Thus, the *Encyclopedia* is not only a tale of the scholar's life but is also a very thorough introduction to the most important astronomical achievements throughout history. The level of information provided is just about right to get the reader started off with his/her own research.

One very interesting aspect is that it does not confine itself to pure astronomers and in fact explores the biographies of a diverse range of characters who made contributions to the field of astronomy, from poets and artists to philosophers and thinkers. In that sense it is really a biographical work that goes beyond the scope of astronomy and could be used as a general reference of scientific biography. The books are alphabetical in order and a careful attempt has been made to link different name aliases for scholars with each other.

The international nature of this work is quite astonishing. The authors have done an excellent job in collecting a comprehensive biography from different parts of the world that otherwise would have been very complicated to locate by just searching into modern western articles. By looking at these volumes one would be amazed at how ancient the science of astronomy is, and I was astonished to find the very international nature of astronomical sciences. One shortcoming, perhaps, was the lack of detailed information on some individuals and the balance was not always preserved in the amount of text used for different astronomers of different status; however, the volume limitations and the perhaps less-understood life stories of some of those scholars justify the imbalances.

Overall, I very much enjoyed reading the books and I believe that a biographical work perhaps could not have been done any better. By using these books as a reference one not only learns about the history of astronomy and science but it is a very good reminder of who we are and how we got here; as Werner Heisenberg once put it: "If I were asked what was Christopher Columbus' greatest achievement in discovering America, my answer would not be that he took advantage of the spherical shape of the Earth to get to India by the western route… His most remarkable feat was the decision to leave the known regions of the world and to sail westward, far beyond the point from which provisions could have gotten him back home again."

This *Encyclopedia* is full of real-life stories of such discoveries and sparkling lives and would be a very valuable reading and research resource into the history of astronomy and science itself. — HOOSHANG NAYYERI.

OTHER BOOKS RECEIVED

Solar Polarization 7 (ASP Conference Series, Vol. 489), edited by K. N. Nagendra, J. O. Stenflo, Z. Q. Qu & M. Sampoorna (Astronomical Society of the Pacific, San Francisco), 2014. Pp. 342, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 862 6).

This is the proceedings of a workshop held in Kunming, China, in 2013 September. Recent advances in research on solar magnetic fields through studies of polarization were discussed from both observational and theoretical standpoints, as were new instrumental projects.

Here and There

BIG "EH?"

But more than 300 years later, the constant — known as Big G to distinguish it from little *g*, the acceleration due to gravity on Earth — is known for sure to only 3 significant figures (6·67384 (±0·0008) × 10^{-11} m^3 kg^{-1} s^{-1}). — *Nature*, **514**, 150, 2014.

VERY HELPFUL

"By nightfall on the 26th the sun will be below the horizon and out of the way, giving you a good chance to look at the asteroid." — *Daily Telegraph*, 2015 January 19