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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2013 November 8 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

D. J. SOUTHWOOD, *President*
in the Chair

The President. I have an important announcement: this session of the Ordinary Meeting will be the first to be recorded. This is part of a scheme to carry the benefits of the Ordinary Meeting — which I think is a uniquely British virtue of our Astronomical Society, that we do meet once a month with an eclectic collection of speakers — to our overseas members who may not be able to get here very often. I'm not clear whether questions will be heard, but very likely you won't be seen. However, you should be aware that the talks are being recorded, with the aim of working out how to diffuse information most effectively to our Fellows overseas.

I would also like to announce that the next National Astronomy Meeting (NAM) will be held at the University of Portsmouth from 2014 June 23–26.

Now let's get to the really interesting business. I'm very pleased to introduce Mark Swinbank from the University of Durham, the Fowler Award winner, talking to us on 'Galaxies under the cosmic microscope: resolving the dynamics, star formation, and chemistry of high-redshift galaxies'.

Dr. M. Swinbank. How did galaxies, such as the Milky Way, form and evolve from the first generations of stars to the spiral galaxy we see today? Deriving the assembly history of 'normal' galaxies is one of the most active areas of galaxy-formation research.

Whilst it is possible to study the physical processes occurring in local (nearby) galaxies in exquisite detail (such as the efficiency at which gas is converted into stars, the energetics of supernovae explosions, and the growth of black holes), most of the stars in our galaxy formed seven-to-ten-billion years ago, an era when the Universe was very different than today. Images, particularly from the *Hubble Space Telescope*, of galaxies at those early times show that they are much more irregular and 'clumpy' than the star-forming spirals we see in the local Universe. To measure how the Milky Way assembled the bulk of its stars, we must therefore measure the properties (such as sizes, masses, star-formation rates) of galaxies in the early Universe.

However, such observations come at a cost. Whilst galaxies in the distant Universe (*e.g.*, seen ~10 billion years ago) were typically forming stars at rates ten times higher than those seen in the local Universe, their immense distances mean that they are extremely small and faint, making measurements of their physical properties challenging, even with long integrations with the world's largest telescopes.

Fortunately, nature provides us with a 'natural magnifying glass' with which we can study the internal properties of distant galaxies in detail. The deep potential wells of massive galaxy clusters amplify the images of distant galaxies that serendipitously lie behind them. This natural magnification (up to a factor of 100 times in the most extreme cases) has two complementary effects. First, the images of the galaxies appear much bigger, and second, since gravitational lensing conserves surface brightness, the flux (or magnitude) of the galaxy is also increased. Together, this amplification makes it possible to obtain high spatial resolution and high-signal-to-noise measurements of the dynamics (and so differentiate rotation *versus* merging), star-formation distribution and intensity, chemical make-up, and energetics of supernovae-driven outflows at physical scales approaching those of individual star-forming regions — far smaller than possible without the aid of lensing. In fact, the data yield a spatial resolution that would otherwise require the increased light grasp of the next generation of ground- and space-based observatories (such as the 40-metre *European Extremely Large Telescope*, or *James Webb Space Telescope*), both of which are at least a decade away. These images of lensed galaxies can be spectacular, often forming a near (or complete) Einstein ring.

Strong gravitational lensing by galaxy clusters is now being used extensively as a tool to measure the dynamics and star-formation properties of early galaxies. By observing lensed galaxies, three important results have emerged. First, by spatially resolving the dynamics of the gas (using the hydrogen Balmer emission lines) within the interstellar medium of strongly-lensed, distant galaxies, it has become clear that the dynamics of a large fraction of the population are dominated by turbulent, but rotating (disc-like) dynamics, perhaps similar to the thick disc of the Milky Way. Moreover, the majority of the 'normal' star-forming galaxies seen just 1–3 billion years after the Big Bang — the epoch when the Milky Way was forming its first major generation of stars — on average, contain 20% of the mass of the Milky Way today.

Second, the star-formation rates occurring within the giant molecular clouds and H II regions of distant galaxies appear to be up to a factor 100 higher than that typically seen in the Milky Way. This could be caused by a large increase in the star-formation efficiency due to the increased gas fractions (and surface-mass densities) and hence pressures of the gas within the interstellar medium which causes the star formation to be more efficient, or even variations in the stellar initial mass function.

Finally, by comparing the dynamics of the star-forming gas with that of the interstellar medium, it has become apparent that distant star-forming galaxies are surrounded by 'superwinds', high-energy blast waves which are caused by the collective effects of star-formation winds and supernovae explosions. Similar in geometry, but much more powerful than the superwind seen in the nearby starburst galaxy M 82, these outflows expel baryons from the galaxy at a rate which is (at least) comparable to the rate at which stars form within the galaxy. The ejected material can be travelling at speeds up to 1000 kilometres per second, far greater than the escape velocity of the parent dark-matter halo. This ejected gas will play no part in the future star formation of these systems, but instead will travel out into the intergalactic medium, polluting it with metals that were formed in the cores of the stars. The energetics of such outflows, predicted by theoretical models to 'regulate' the cooling of baryons into stars, are a crucial element of galaxy-formation models.

The President. One or two quick questions?

Professor M. J. Disney. It doesn't look as if these galaxies are forming from mergers of lots of little ones, does it?

Dr. Swinbank. Correct. We were surprised because the dynamics of most of them look like big discs. We weren't expecting to find very regular dynamics in so many of the objects we looked at.

Rev. G. Barber. In the simulations you showed us, you ended up with a realistic structure of a spiral galaxy, but it was at $z = 0$. Is there therefore an age problem in a sense that you are seeing well-formed galaxies at high z, when simulations just give you blobs at that time?

Dr. Swinbank. That's a good question. In the simulations we do find high-redshift discs; but we probably don't find enough of them. The get-out is that the number of galaxies we have actually observed with this sort of resolved dynamics is still in the few tens, maybe one hundred or so, with selection functions that aren't well understood. We are only just starting to build statistical samples to be able to test if the data and the models match.

Rev. Barber. Do they?

Dr. Swinbank. The next few years will tell.

Mr. N. Calder. Is any of this relevant to starburst events in the Milky Way, 2 Gyr ago?

Dr. Swinbank. The galaxies that we are observing here tend to be at much higher redshift than that would imply. One project we do have in mind is to try and age-date some of the clumps in the high-redshift galaxies to try to relate to local, giant molecular clouds. However, at the moment, no, not directly.

The President. Thank you very much. [Applause.] My next duty is to introduce Dr. Sudipta Sarkar from the Helmholtz Centre for Ocean Research in Kiel, Germany, speaking on 'Seismic evidence for shallow gas-escape features associated with a retreating gas-hydrate zone offshore west Svalbard'. However, before I allow you to speak, I am going to make a small award to you, which is your certificate for the Keith Runcorn Prize for Geophysics. [Applause.] Even better than the certificate, there is a cheque! [Laughter.] Now, you have to sing for your supper [laughter].

Dr. S. Sarkar. Active methane venting was found on the uppermost continental slope off west Svalbard, close to and upslope from the present-day intersection of the base of methane-hydrate stability (BMHS) with the seabed in water depth of about 400 m. The ocean in contact with the seabed has warmed up by 1° Celsius in the past 30 years. Such warming could trigger methane-hydrate dissociation at the uppermost edge of gas-hydrate stability.

The marine methane-hydrate province was mapped using a high-resolution multichannel-reflection seismic dataset collected in summer 2008. I can show the geophysical evidence for gas hydrate, the heterogeneous glaciomarine sedimentary system, and patterns of gas migration and accumulation below the seabed. A bottom-simulating reflector (BSR) demarcates the base of the methane-hydrate stability zone on reflection seismic data. Natural-methane seeps are located on the seabed from which the upper edge of methane-hydrate stability retreated from a depth of ~350 m (at 2° C bottom-water temperature, ~30 years ago) and deepened to a present-day depth of ~400 m (3° C bottom-water temperature condition) on the continental slope. The coincidence of gas seeps with the area of gas-hydrate retreat associated with bottom-water warming strongly evokes the role of methane-hydrate dissociation. However, gas seeps located further upslope, such as on the distal shelf and shelf break (at much shallower water depth, less than 350 m) are not related to hydrate dissociation because this region is outside of the hydrate-stability limit and here prograding glaciomarine beds deflect migrating gas upslope towards the shelf break.

The heterogeneity of the glacial sediments strongly influences focussed gas migration to the original sites of hydrate formation when conditions were more conducive for hydrate to form. With a warming ocean, the hydrates at the original sites of formation started to dissociate and gas released from therein migrates towards the seabed following more permeable routes in the glaciomarine sedimentary package.

The President. Questions or comments?

Mr. H. Regnart. If I am right in remembering that methane is a more potent greenhouse gas than carbon dioxide, is there a way in which these natural seeps of methane could be harvested and put to commercial use?

Dr. Sarkar. Commercial exploitation of methane from gas hydrates is what people are trying to achieve. In Japan, recently, they tried to do that. But it is challenging given that it is a climate-sensitive issue and the technology is not advanced enough. The major concern is that we have to operate in shallow water depths. Slope stability is a big issue; when hydrates form in sediments they bind the sediments together and provide strength. If we try to extract gas out of it, the structure breaks down, it loosens up, and the sediments would come down as slumps or slurries. This has severe consequences on platforms or rig stability. People are also thinking about CO_2 sequestration in hydrates: they want to extract methane out of the structure and inject CO_2 into it. I don't know the timescale involved in making a sound technology available but there is a potential treasure trove of energy in hydrates. You can think of hydrate as a capacitor of methane: what came out further below the seabed got locked up within the ice structure. So if we have a big hydrate vein or other massive body of hydrate, we could potentially mine it and ship it to land and break it down and extract gas. But exploiting that resource is extremely challenging.

Professor M. M. Grady. Do you get hydrogen sulphide as a clathrate in any of these?

Dr. Sarkar. There could be hydrogen sulphide; there could be many different gases — hydrocarbons, alkanes (ethane, propane), carbon dioxide, nitrogen.

Professor Grady. Can you distinguish these in seismic signals?

Dr. Sarkar. No, we cannot. We collected gas samples from the seabed site and saw a high proportion of methane in it. We had a deeper core which retrieved hydrates at 700-m water depth. From there we know methane is the predominant gas, at 99%.

Dr. G. Q. G. Stanley. With these hydrates, once they dissociate, do they take up less volume? Therefore you'd have a cascade and more dissociation at that point, leading to the pockets becoming self-perpetuating?

Dr. Sarkar. This process is more complicated. There is hydrate present in the first place; when it breaks down it does so into gas and water. If the gas cannot escape, it builds up and a local overpressure is created. If the pressure exceeds the lithostatic pressure that can produce cracks and the gas comes out. Once gas is coming out of the system, the pressure goes down and that also creates a destabilizing effect on the whole system. However, with hydrate dissociation the temperature of the system also drops which will have a stabilizing impact. The kinematics is largely decided by these opposing factors. In our study we looked at it more as a phase-stability issue. Although volume is important in the sense of how much gas is present, how much space is available, we cannot distinguish from seismic data alone, we need drilling.

Dr. Stanley. So it can oscillate around, between forming some and increasing stability — it just depends on the situation?

Dr. Sarkar. Yes.

Professor Disney. Should we be more worried as a result of your work?

Dr. Sarkar. No! [Laughter.] We should be happy that we have started to understand these phenomena. During a geological time called Paleocene–Eocene Thermal Maximum we find a negative delta-^{13}C isotopic excursion in marine geological records. People try to think of many different ways that this type of highly negative excursion of isotopes took place. One of the ideas was the clathrate or hydrate-gun hypothesis, when massive amounts of hydrates dissociated and methane came out. Something similar could have happened in recent times as well. We want to understand what is happening now in Svalbard and it serves as an ideal natural laboratory to investigate the climate-sensitive aspect of gas hydrate. The more we know, the better it is, so that we can predict the system's behaviour. There is nothing to worry about as such!

The President. On that note we should relax and let you leave! Thank you very much indeed. [Applause.] Now it's my great pleasure to introduce the Society's George Darwin Lecture, by Professor Eline Tolstoy from the University of Groningen in The Netherlands, on 'Galactic Palaeontology'.

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

The President. Questions?

Mr. M. F. Osmaston. You talked about globular clusters. What would be your opinion on the significance of blue stragglers?

Professor Tolstoy. That's an interesting point. There is a population we see in all globular clusters; even some dwarf spheroidals have something like a blue-straggler population. The standard approach is to think it is coming from regeneration from a companion star: one star becomes a white dwarf, one becomes a planetary nebula, and you get a mass transfer which rejuvenates one star more. This star then becomes younger than it seems. It is therefore critical that we understand all the processes that can place a star on the colour–magnitude diagram. It's a tiny fraction of stars, less than 5% of stars are in this phase.

Mr. Osmaston. How does the metallicity compare with the rest of the cluster?

Professor Tolstoy. The metallicities are the same.

Professor Grady. I'm a planetary scientist, studying meteorites, so you could call me a solid-state astronomer. One of the fossils that gives a really interesting different perspective on what you talked about is the pre-solar grains that we find in meteorites. If you take a primitive meteorite, and dissolve 99% of it, you're left with silicon carbide, graphite, diamonds, aluminium oxides. They also contain different noble gases, such as xenon, neon, krypton, which have got the signals of different stars. When you look at that, you see that there were at least 40 different stars, different AGBs, heavy carbon, carbon-rich stars, all sorts. We can even start to look at different astrophysical processes going on in stars. You can look at the remnants of dredge-up in AGBs. They are all preserved in meteorites.

Professor Tolstoy. As far as I know, meteoritic abundances actually coincide very well with the Sun. The conclusions you come to are very similar if you do a very detailed survey of the Sun and understand exactly where the different elements come from. It is very interesting that they are parallel and agree with each other.

Professor Grady. These are grains which have come into the molecular clouds from which our Solar System formed and come from different stars. They come from AGB stellar winds that were blowing into our System as it formed. What we can't do yet fully is get their ages because they're still a little bit too small for current technology. Once we can start dating them we'll be able to look at the age of the stars that were in the local group 4·6 billion years ago.

Rev. Barber. Given the importance of type-Ia supernovae in cosmology, how robust is the assumption that they are standard candles in the early Universe?

Professor Tolstoy. I wouldn't know, actually. The only thing I would say is that they are being used as standard candles in a region where the metallicity is fairly standard. Nobody has seen a SN Ia in the very early Universe; I don't think they are used as standard candles beyond $z = 1{\cdot}5$. They hope to observe out to $z = 2$ or 3 with *JWST* but this is still in a fairly normal region. I am not sure we will be able to identify one in the early Universe because I don't know they'd behave in the same way. However, at a redshift of a few, there is no reason to think that they are going to be any different because galaxies have become enriched. I think the real difficulty is when there are no heavy elements.

Professor I. W. Roxburgh. You made it clear that you assume that the outer parts of the star remain unchanged. Are you confident that gravitational settling won't mess things up?

Professor Tolstoy. That's a very good point and we do worry about it. There isn't much we can do about it so we look for signatures, as certain elements are believed to be affected by different processes. We are never looking at one star, we are looking at large groups of stars. Especially when looking at globular clusters or dwarf spheroidals, we look at stars that have formed in a particular environment. We look for signs of where and when it becomes important. It is a critical point that when one considers lighter elements such as carbon, nitrogen, and oxygen, it is almost certain that in a red giant they will have been affected by mixing. In dwarf stars the evidence points to subtle effects within certain elements but broadly speaking with things like alpha elements it's not a problem in dwarf stars. We have to be very careful which elements we look at and check for other effects.

Professor S. Miller. If you were to find one of these very earliest stars, would you be identifying it simply because it had no metals in the spectrum or would there be other things, maybe helium–hydrogen combinations, H^-, H_3^+, these kinds of things, that might tell you that this really is a very primitive star?

Professor Tolstoy. Up until now we have been looking for absence of metal lines, so looking for a helium–hydrogen mix is fine and the absence of metal lines is what people have looked for and not found. You may expect there to be a range in the second generation after the first stars; you would expect the gas in the outer region to be completely dominated by the abundance factors of those very first stars. You're not necessarily at the first star itself but the direct descendants of that.

Dr. Swinbank. Connected to that question, you said at the start you were looking for very-low-metallicity, high-velocity stars. Have you not found them because it's too hard with current technology or might they not exist because, for example, the CMB temperature at redshift 10 means you truncate the low-mass end of the IMF?

Professor Tolstoy. It's possible; that is the current explanation for why we are not finding them. There is some theoretical evidence that you only form very-high-mass stars with zero metallicity. It is still speculation because we haven't found clear evidence of what kind of massive star you are forming. One still expects to see the second-generation stars but we don't see those either.

Dr. Stanley. You said that the dwarf galaxies were the oldest you can find; is that because they're the most stable?

Professor Tolstoy. The reason I made that statement is more because that is what the prediction is. At the very beginning, in the very early Universe, there were very small structures. The first star formation occurred in very small clumps, which then, theory tells us, combined to form larger clumps relatively quickly. In some sense you are therefore saying that those smaller clumps should be more ancient than the larger ones, even though the larger clump is made up of smaller clumps. That was more what I meant; we predict that they are going to be very ancient. There is nothing we see to contradict that. Every time we look at a dwarf galaxy in enough detail we see ancient stars. We see no particular difference between the dwarf galaxy and the Milky Way, with the exception of the carbon stars and that could be a statistical effect. Broadly speaking, it seems ancient star formation occurs everywhere but dwarf galaxies are the origin of a lot of these processes. You should see the whole picture in a small system in a much more simple way.

The President. Thank you. [Applause.] There is a drinks reception in the RAS library immediately following this meeting. The next monthly A & G Open Meeting of the Society will be on Friday, 2013 December 13.

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Friday 2013 December 13 at $16^{h}\ 00^{m}$
in the Geological Society Lecture Theatre, Burlington House

D. J. SOUTHWOOD, *President*
in the Chair

The President. Welcome. Firstly, something a little out of the ordinary. The daughter of E. A. Milne, who was a past President of this Society and a Gold Medallist, has given the Society the means for a travel grant in memory of her father. She has also written a book. There are copies of a glossy handout on the table outside; if you were too shy to pick one up on the way in, I will ask you to pick one up on the way out. It's called *Beating the Odds*, which sounds like it could be profitable. However, I have yet to find out how to profit from it. It is a book by Milne's daughter, Meg Weston Smith, who I believe is present. It's a personal story of Milne and the story of a scientist who did go against the odds, I suppose [see review in these pages: **134**, 35, 2014 — Ed.]. I think many of us know of his work and many of us also know of scientists who've had interesting lives.

And now we come to the meat of the meeting — the talks. The first is by Jonathan Betts from the National Maritime Museum. He will be speaking on 'The Royal Astronomical Society's Harrison-clock project'.

Mr. J. D. Betts. I am going to talk about the important pendulum clock made by John Harrison which is in the possession of the Society, and the studies relating to the clock. Harrison is famous as being the creator of the first successful marine timekeeper, *H4*, and for gaining the reward offered by the Government of the day for the solution of the longitude problem. What is less well-known is that throughout his career, Harrison was developing his own ideas about fixed-pendulum-controlled clocks. Following Huyghens' pioneering work on pendulum clocks in the 1650s and that by Richard Townley and Thomas Tompion in England in the 1670s, and George Graham in the 1720s, by 1725 the pendulum clock had developed into what became known as a standard 'regulator'. It was Huyghens who showed that the pendulum was not a perfect timekeeper. Huyghens realized that swings of the pendulum were not isochronous so the rate of the swing depended upon the size of the swing. As the amplitude of the pendulum increased so the rate of swing slowed. This inequality was known as circular error. To remove circular error Huyghens designed a pendulum held on a silk thread which was suspended between cycloidal cheeks. If the pendulum swing increased the thread would wrap further around the cheeks and compensate for the circular error. It was a good idea but the weakness was the instability of the thread suspension. One solution to this was to use a heavier pendulum bob with a smaller-amplitude swing. In the 1720s George Graham invented a pendulum which self-compensated for temperature change by using differential expansion in a jar of mercury employed as a pendulum bob. At the same time Harrison was experimenting with a grid-iron pendulum made from a grid of brass and steel rods. This was to become the favoured type during the 18th Century.

Thomas Tompion invented the dead-beat escapement which, given constant temperature, was capable of an accuracy of one second per week. This became regarded as the best design for a regulator in the 1720s. Harrison then introduced a regulator with a relatively light pendulum bob (a few pounds weight) and an arc of 12 degrees. The escapement is not a dead-beat but incorporates recoil into its design, which was totally at odds with conventional thinking. The escapement, and indeed the movement, for this clock required no lubrication. Harrison claimed that his regulator had an accuracy of 1 second per month and used it, at least in the early years, to rate his marine timekeepers. Whilst working on *H3* in the 1740s Harrison continued to think about the development of his pendulum clocks and he started to construct what is now known as the *RAS Regulator*. Today only the movement exists — it consists of about 900 parts made mostly from brass and bronze alloys. It uses cycloidal cheeks and a grasshopper escapement whilst the main bearings are caged roller bearings — a remarkable invention by Harrison. It incorporates a *remontoire* which ensures a uniform drive to the escapement. Harrison thought that his regulator should be fixed as firmly as possible and it may actually have been built into a niche in a wall. It was never completed or adjusted owing to pressure of other work. In 1775 Harrison thought it capable of keeping time to within one second in 100 days and it was not until the 20th Century that this figure was achieved with the Shortt free-pendulum clock which had the pendulum enclosed in a semi-evacuated tank.

Subsequently, it passed to Harrison's grandson-in-law John Barton. It was later shown to members of the Royal Society in the 19th Century and at that time it was described as having no case or pendulum. Barton's son by marriage inherited it in 1834 and it was presented to the RAS in 1838. It was then given an oak case and it was apparently working at the Society until about 1870.

In 1927 R. T. Gould proposed the removal of the case and pendulum and proposed the construction of a new glazed case and a new grid-iron pendulum by himself. It was returned to the Society in 1929 February. Between 1960 and 1973 it was at the Royal Greenwich Observatory at Herstmonceux and from there went to the National Maritime Museum in Greenwich. The case and pendulum prior to Gould's work have not survived but his notebooks were passed to Greenwich. There was a revival of interest in 1966 when Col. Humphrey Quill wrote his biography of Harrison and in 1976 Bill Laycock authored *The Lost Science of John 'Longitude' Harrison*.

In 1976 also, a like-minded number of enthusiasts formed a Harrison research group; as they began to look at his rather obscure writings in a new light, repeated readings began to reveal his intentions. In the late 1970s the clockmaker Martin Burgess decided to make a pair of clocks, based on the technology of the RAS clock except for the pendulum, where he used invar instead of a grid-iron.

The first clock was completed for a commission and had to be delivered before any conclusive tests could be done on its accuracy. The second clock was recently completed and is now at the ROG for tests to be made. It is not yet fully adjusted but the timekeeping is already very extraordinary. It is outperforming every other clock with a pendulum swinging in air. In 100 days the clock stayed within 1 second without the barometric compensation being incorporated. This clock uses an invar pendulum but it would be more interesting to test the RAS clock itself. Using Harrison's pendulum design, the British Horological Institute team of nine craftsmen will build six replicas and it is hoped to test the first at the end of 2015. We are now incorporating barometric compensation in the Burgess clock.

In 1775 Harrison said that if he had received more encouragement from the Royal Observatory he would have made more adjustments to this regulator and bequeathed it to the Observatory. The question I would like to pose to the audience is: if he had done this and astronomy in the 18th Century had a timekeeper as good as the Shortt free-pendulum clock, in what ways would the work of the Observatory been enhanced?

The President. Exquisite timing! [Laughter.] Questions or comments?

Dr. A. Ingram. I can understand how you now can test the performance of Harrison's clock, because you have an even more accurate clock. How would he have tested the performance of his clock, since his was the best that existed at the time. How would he know it was accurate to one second in 100 days?

Mr. Betts. Harrison, in his voluminous writings, tells us a great deal about how he practised his craft. He is very clear about what time standard he used. It was a kind of monumental transit instrument, in the simplest possible sense. When he was working in Lincolnshire, and we believe to some extent when in London, he used a neighbour's chimney against the wooden window frame of his workshop. He aligned the glazing bars of the window frame and the chimney and watched specific stars as they came to culmination between the two. He claimed he was able, using five window bars, to time any single observation to within a twentieth of a second. He wasn't the first person to use that method. The method has been described several times pre-Harrison. It was described by Nicholas Saunderson in his lectures. There are also several descriptions in horological textbooks, so it was known.

Professor M. Ryecroft. What is the relationship with the scale of music?

Mr. Betts. Oh, I was hoping nobody would ask me that [laughter]! I think even if you asked a musicologist, they would be stumped. Harrison had very strong views on how the musical scale should be arranged. It's to some extent up to us to decide how we arrange the notes and Harrison was passionate about his views on that subject. At the end of that diatribe on horology, he included an appendix with his views which seem to relate the octave to a circle in some way. He divides the octave into several parts according to π, which he regarded with almost mystical significance. It's very easy to read Harrison's writings on music and conclude he didn't know much about anything because it's such gibberish [laughter]. Fortunately, on horology, he did know what he was talking about. If anybody cares to read Harrison's writing and thinks they have a sufficient knowledge of music to be able to judge it, I would love to know, because nobody I have yet spoken to seems to understand what he is saying [laughter].

The President. Thank you very much. [Applause.] We move on to the next speaker, the Fowler Geophysics Award winner, Iain Hannah from the University of Glasgow, whose talk is on 'Electron acceleration in solar microflares and the quiet Sun'.

Dr. I. Hannah. I would first like to thank the Royal Astronomical Society for awarding me the 2013 Fowler Prize for Geophysics. I would also like to thank my *RHESSI* collaborators, especially the former PI of *RHESSI* Professor Robert Lin.

Solar flares are observed as rapid brightening across all wavelengths, indicative of energy being quickly released into heating material and accelerating particles in the Sun's atmosphere. A substantial fraction, if not the majority, of the energy released accelerates particles and these provide a measure of the overall flare energy, crucial for understanding the physics powering these processes. In hard X-rays (HXRs) solar flares appear as bright 'footpoint' sources at the ends of hot loops. The emission is due to bremsstrahlung-producing collisions of the electrons with (and heating) the dense lower solar atmosphere. These electrons are thought to be accelerated in the high solar corona before travelling downwards, so studying the HXR emission is not only an excellent diagnostic of the energetic electrons in the lower solar atmosphere but also of their energy at the acceleration site (*via* model assumptions).

HXRs are a powerful diagnostic tool of flare energetics and this has been exploited by NASA's *Ramaty High Energy Solar Spectroscope Imager* (*RHESSI*) which has observed many 10 000s of solar flares since its launch in 2002. It provides high spatial, energy, and temporal resolution of flares from its nine germanium detectors (covering 3 keV to 17 MeV), each behind a pair of collimating grids. The entire spacecraft rotates at about 15 rpm resulting in a time-modulation of the flare emission through the collimators from which spatial information and images can be recovered (in a manner similar to that of radio interferometry).

One important aspect of *RHESSI* is its movable attenuators, which are required during observations of large flares to avoid detector saturation. Previous missions typically had fixed shutters but with *RHESSI* they can move, meaning that the full sensitivity of the detectors is available for small flares. So for the first time the detailed HXR spectroscopy and imaging of microflares could routinely be conducted. Microflares are energetically about a million times smaller than the largest flares and my work with *RHESSI* showed that they are smaller versions of large flares. Like large flares they demonstrate electron acceleration and plasma heating and occur in the magnetic active regions. From my automated imaging and spectroscopic analysis of over 24 000 microflares I was able to obtain both the thermal and non-thermal energy during the time of peak emission, producing a frequency distribution of their energetics. The flare-frequency distribution is used to compare the occurrence rates of events of different sizes, and across many orders of magnitude is a negative power-law. Crucially, the index of this power-law provides information about whether the smallest or largest flares dominate energetically: an index > 2 or < 2, respectively. Unfortunately the values obtained from *RHESSI* and other observations are around about 2 and the distributions, and hence the inferred index, are strongly influenced by selection effects.

By studying microflares we are able to learn about the flaring process in general as well as the overall energetics of the system. This is important as when there appear to be no flares (no X-rays above our sensitivity) the solar atmosphere is consistently hot. We know that the accelerated electrons in large and microflares can heat the solar atmosphere *via* Coulomb collisions, so might a population of very small flares be heating the 'non-flaring' Sun? It is highly likely that there exist microflares beyond our current level of sensitivity, but microflares, like larger flares, are a uniquely active-region phenomenon so might not be able to heat the atmosphere outside of active regions (quiet Sun). A further idea is that of Parker's nanoflares, where we have very small events (at most a billionth the energy of large flares) that occur as energy is released *via* magnetic reconnection of braided field lines. In practice individual Parker nanoflares would be too small to observe but ensembles of them could be visible.

If we were able to detect HXRs from a 'non-flaring' active region or the quiet Sun then we would have clear proof of the presence of accelerated electrons, the ubiquitous feature (and a source of heating) in micro/large flares. Unfortunately HXR solar telescopes, like *RHESSI*, are optimized for flare observations and so do not have the sensitivity or dynamic range required. With *RHESSI*, however, we were able to develop an alternative mode of operation with the aim of detecting HXRs from the quiet Sun. This involved off-pointing *RHESSI* slightly from the Sun and using the shadow of the front collimators onto the rear ones to chop sharply (and predictably) the solar signal as the spacecraft rotated. This fan-beam modulation procedure was tested with off-pointing data from *RHESSI* observations of the Crab Nebula and successfully obtained the expected spectrum.

By using 140 days of off-pointing with *RHESSI*, we accumulated 11·9 days of quiet-Sun (no active regions) observations. Unfortunately I did not find a signal but I was able to produce the deepest HXR limits to the quiet Sun so far and over a wider energy range than before. With these limits I have been able to constrain the possible properties of a quiet Sun and, using the coronal-heating requirement, found that the nanoflares would have a very steep non-thermal spectrum that would extend down to < 1 keV.

To make progress towards observing faint HXRs from the Sun requires an X-ray telescope with both high sensitivity and a large dynamic range. This technology has only recently become practical and was launched in 2012 on NASA's *Nuclear Spectroscopic Telescope Array* (*NuSTAR*). Attempts will be made with *NuSTAR* to observe the quiet Sun but it is optimized for very faint astrophysical sources and is likely to saturate with even modest levels of solar activity. A similar telescope but designed to be able to deal with the immense range of solar HXR emission is needed and development is well under way with the *HEREOS* balloon and the test flight of the *Focusing Optics X-ray Solar Imager* (*FOXSI*) sounding rocket in 2012. *FOXSI*'s 6·5 minutes of observations caught the emission from a microflare (also observed with *RHESSI*) but nothing from non-flaring regions. The PhD student working on *FOXSI*, Lindsay Glesener, was awarded the RAS Patricia Tomkins Thesis Prize, and will provide more information about this and future *FOXSI* launches in her prize talk at the February ordinary meeting.

HXRs are a vital diagnostic of the energetics of solar flares and the long-term aim is for a *RHESSI* successor that has even higher sensitivity combined with a large dynamic range. This imaging-spectrometry telescope would be able to probe the unexplored 'non-flaring' regions of the Sun as well as tracking the energetic electrons back to the acceleration region, giving a fuller picture of the energetics of flares of all magnitudes.

The President. Questions? Comments?

Professor D. Lynden-Bell. You talked a lot about the electrons. Is there a knowledge of how much energy goes into the protons or ions?

Dr. Hannah. There is some information. Gamma-ray observations do give you some information about those higher-energy processes. The problem with *RHESSI* is that its gamma-ray performance wasn't brilliant, so there wasn't too much that came out of that. I think quite a lot of solar flares have been observed with *Fermi* and the gamma rays are a good diagnostic from that. It's higher energy than what I'm thinking about. A large component of the flare energy could be going into ions and protons, and if you don't know the gamma-ray sensitivity we might not be seeing it.

Professor Lynden-Bell. So you don't yet have a figure for whether it is a half, a third, or whatever?

Dr. Hannah. It depends on the flare. I could give you a flare fitting those numbers but I could give you half a dozen others that don't. A rough ballpark is maybe tens of percent but these things have huge uncertainties associated with them.

The President. Thank you very much. [Applause.] We now move to the Michael Penston Thesis Prize winner, Adam Ingram from the University of Amsterdam. Before he starts speaking, I'm going to present him with the thesis prize and a cheque for a large amount of money [laughter]! Congratulations. [Applause.]

Dr. Ingram. I would like to start by saying how honoured I am to receive the Michael Penston Thesis Prize. The title of my thesis is: 'A physical model for the variability properties of X-ray binaries', and this is also the title of my talk. But this is a large topic and I would like to go into some depth while keeping the talk self-contained so I'm just going to focus on the most interesting part of the thesis: 'A physical model for quasi-periodic oscillations in black-hole binaries'. Black-hole binaries (BHB) consist of a black hole and a normal star orbiting a common centre of mass. When gas from the normal star falls onto the black hole it forms an accretion disc in which huge amounts of gravitational potential energy are liberated as X-ray radiation. Since the brightest parts of the accretion flow are closest to the black hole, those binaries provide a unique laboratory for strong-field gravity. This very interesting region is far too small to be imaged directly but can be probed by studying rapid X-ray variability.

A geometrically thin, optically thick disc extending down to the innermost stable circular orbit (ISCO) emits a characteristic multi-coloured blackbody spectrum peaking in the soft X-rays. If you observe a black-hole binary on the right day, this is exactly what you see. However, if you look at the same object on a different day, you may also observe a power-law tail extending to hard X-rays. In the truncated-disc model, the disc truncates at some radius larger than the ISCO. Inside the truncation radius, the disc is assumed to evaporate into some hot, translucent plasma, which I will call "the flow" from now on. Compton upscattering of disc photons by energetic flow electrons creates this power-law tail. The evolution of the spectrum can then be explained by the truncation radius moving towards the black hole as the spectrum gets softer. We also see an iron K-alpha fluorescence line resulting from flow photons reflecting off the disc and being scattered into the line of sight.

If we look at a BHB on the right day, we see a quasi-periodic oscillation (QPO): an almost periodic modulation in the X-ray flux with an average period that changes from ~10 s to 0·1 s as the truncation radius is inferred to be moving from ~800 km to 80 km. QPOs were first discovered 30 years ago and their potential importance as diagnostics of strong-field gravity was immediately recognized. However, this potential couldn't be realized because their physical origin wasn't clear.

In my thesis, I explore the relativistic effect of frame dragging as the QPO origin. In General Relativity, when a massive object spins, it drags the surrounding spacetime with it. This causes particle orbits out of the black-hole equatorial plane to precess as they are swept around the black hole. The precession period of a test mass is roughly proportional to r cubed, where r is the distance to the black hole. However, we aren't considering a test mass orbiting a black hole; instead we have some two-component accretion flow consisting of a thin disc and a thick flow. So if the black-hole and binary-system spin axes are misaligned, how do these two types of accretion flow respond to the warp created by the inner regions trying to precess faster than the outer regions? For a thin disc, viscosity fixes the outer regions of the disc in the binary plane. In contrast, simulations show that pressure waves strongly couple a thick flow, allowing the entire flow to precess as a solid body. Our model thus assumes that the outer disc aligns with the binary system and the inner flow inside of the truncation radius precesses as a solid body. The precession period for the entire flow is a surface-density-weighted average of the test-mass precession period for the range of the flow. When we input the surface-density profile predicted from simulations, we found that the precession period moves from ~10 s to 0·1 s as the truncation radius moves from 800 km to 80 km, *i.e.*, the range we observe for the QPO. This model also explains why we see stronger QPOs from objects with a larger inclination angle. Also, the spectrum of the QPO consists of just a power law and has little to no contribution from the outer disc — an observation naturally explained by our model.

Although these factors are encouraging, we still have no definitive 'smoking gun' evidence that the QPO comes from precession. In the final chapter of my thesis, I showed that we could obtain such smoking-gun evidence by looking at how the iron fluorescence line changes with QPO phase. Since the disc is spinning, an inclined observer sees blue-shifted emission from the approaching side of the disc, and red-shifted emission from the receding side. This means an iron line which is a delta function in the rest frame appears in the observed spectrum with a broad, two-horned profile. The profile is also asymmetric due to time dilation boosting the blue horn of the line. If the inner flow is indeed precessing, it will preferentially illuminate different regions of the disc for different QPO phases. At the peak of the QPO cycle, the flow faces us. During the falling phase, the flow preferentially illuminates the receding disc material and we see a red-shifted iron line. During the rising phase, the flow preferentially illuminates the approaching disc material and thus we will see a blue-shifted iron line. Therefore, if we can phase resolve the QPO into rising and falling segments, we can directly test for precession!

I simulated this effect for different exposures on different instruments and found that a long exposure with *XMM–Newton* should be enough to discover a QPO phase dependence of the iron line. Unfortunately, no such observation currently exists in the archive but I have a 200-ks target-of-opportunity observation, which I will trigger the next time a black hole displays a strong QPO. Looking to the future, the *Large Observatory for X-ray Timing* (*LOFT*) will do far better than this for only a 5-ks exposure due to its exceptionally large collecting area. With *LOFT*, we will be able to obtain high-quality phase-resolved spectra in order to map the accretion flow in exquisite detail.

The President. Questions or comments?

Dr. G. Q. G. Stanley. Your observations showed a rapid rise and a slow fall of the emissions whereas I think your models are showing more even flow. Would that be right?

Dr. Ingram. The model I showed here is a very simplified one. It's assuming that the precessing flow is an oblate spheroid; and that there is no angular dependence of intensity, which isn't actually the case for a Compton ionizing slab. In addition, there are relativistic effects, which aren't taken into account here. When you do that you expect an asymmetric profile. I haven't been able to constrain the QPO waveform to fit a model. What we need to ask is what is the average QPO waveform? You can see already that it has a rapid rise and a gradual decay. I am working on trying to do that now but it is quite difficult because it's a quasi-periodic oscillation, not completely periodic. I can then go and try to fit these asymmetric profiles with relativistic effects.

Dr. K. Smith. The precession is driven by the difference between the black-hole spin axis and binary axis. Depending on the masses of the black hole and the donor star, as the system evolves that axis is going to align closer and closer but it's also going to spin up and get faster and faster, so what does that do to your precession?

Dr. Ingram. It has been shown that while you expect that to happen, it's a very slow process. That is the case particularly for what I am looking at, low-mass X-ray binaries where the companion star is less massive than the black hole. The time required to make any significant changes to the angle or the magnitude exceeds the time it takes to accrete the binary partner. There is no more binary by the time you have significantly affected it.

The President. Thank you very much. [Applause.] I am very happy to introduce Matt Nicholl from Queen's University Belfast to talk on 'Slowly-fading super-luminous supernovae: pair-instability or magnetar-driven explosions?'

Mr. M. Nicholl. Supernovae are the explosive deaths of stars, arising either from the collapse of the iron core in a massive evolved star, or from thermonuclear runaway in an accreting white dwarf. These events, powered by shock-heating of the ejected matter or by radioactive decay of ^{56}Ni, can outshine their host galaxies. In recent years, a population of so-called 'super-luminous supernovae' has been discovered, at least 10–100 times brighter than the previously known supernova types. The mechanism, or mechanisms, underlying their exceptional luminosity remains unclear, and the nature of these objects is a big question in current supernova research.

Several theories have been proposed. Some super-luminous supernovae have been interpreted as observational examples of long-sought 'pair-instability' explosions. Those supernovae, predicted in the 1960s, are thermonuclear detonations of stars over 130 solar masses, caused by a loss of core pressure following conversion of high-temperature photons to electron–positron pairs. Another possibility is that a central 'engine' deposits energy in the ejecta after the explosion — for example, a neutron star (or 'magnetar') with a spin period of a few milliseconds and a magnetic field of about 10^{14} Gauss. The final mechanism that is commonly invoked is a collision of the ejecta with a few solar masses of circumstellar material. A less extreme interaction, with a low-mass stellar wind, is a common feature of massive-star explosions.

Virtually all of the super-luminous supernovae have been found by large unbiassed transient surveys such as *Pan-STARRS* and the Palomar Transient Factory. Two recent supernovae discovered by those surveys have proven very useful in discriminating between the various models: PTF12dam (a.k.a. PS1-12arh) and PS1-11ap. These supernovae closely resemble the famous supernova SN 2007bi. The three all show a slow decline in luminosity after their respective maxima, approximately matching the well-known rate of energy input from the radioactive decay of nickel, to cobalt and then iron. Several solar masses of ^{56}Ni would be required if this was the main power source behind the observed luminosity. Such high nickel mass suggests a pair-instability explosion, as was proposed to explain the unusual light-curve of SN 2007bi.

Our new supernovae were first detected much earlier in their evolution than was SN 2007bi. This allowed us to compare, for the first time, the early (rising) phase of the light-curve to the predictions of pair-instability models. Using synthetic light-curves produced by Dan Kasen and Luc Dessart, we showed that these super-luminous supernovae were inconsistent with the predictions of pair-instability models. The models took over 100 days from the time of explosion to reach maximum brightness. That is because of the very large mass of ejected material associated with pair-instability — the energy deposited by radioactivity deep in the supernova takes a long time to diffuse through such a great mass. In contrast, our objects rose to maximum within 50–60 days, which is long compared to most supernovae, but significantly faster than any model of such a massive star.

Spectroscopically, our objects not only closely resembled SN 2007bi over its observed lifetime (from 50 days past peak light, to several hundred); they also matched many other super-luminous supernovae at earlier phases (before and around peak). Those other supernovae were of a much more common type, with a steeper light-curve decline, previously thought to be a distinct class from SN 2007bi. Our observations suggest that in fact the two groups are related. However, line formation is delayed in our slowly-fading events, relative to the other sample. It should be noted that all of the supernovae in our sample showed hydrogen-poor spectra; hydrogen-rich super-luminous supernovae are normally interpreted using the circumstellar interaction model.

We modelled the light-curve of PTF12dam using a semi-analytic diffusion model for the emission of light from the ejecta, while choosing energy-input functions appropriate to both radioactive decay and magnetar radiation. We found that nickel-powered models required tens of solar masses of ejecta (such as in a massive iron-core-collapse supernova) with nickel fractions of 30–90% (comparable to the ejecta of type-Ia supernovae from white dwarfs). These parameters are difficult simultaneously to reconcile with detailed supernova models. On the other hand, fitting our light-curve with a magnetar energy source was possible for supernovae ejecting 10–16 solar masses of material, and forming a magnetar with a period of 2·6 milliseconds and a magnetic field of about 10^{14} Gauss. These are close to magnetar-based fits to normal hydrogen-poor super-luminous supernovae.

The colours and temperatures of our supernovae also favoured a magnetar model over a pair-instability interpretation. Pair-instability explosions produce very metal-rich ejecta, which strongly absorbs blue and ultra-violet light.

Those supernovae should therefore appear red and cool. On the other hand, the magnetar inputs comparable energy, but to much less massive ejecta. This keeps the material very hot and ionized, and produces very blue supernovae. Comparing our spectra to the models of Kasen and Dessart, we found quite good agreement with a magnetar supernova, whereas our spectra were much bluer than pair-instability models at all phases.

All the evidence therefore points to a magnetar-powered explanation for these unusual, long-lived super-luminous supernovae. Many authors also favour this model for the normal, faster-evolving events. Comparing the parameters of our magnetar fits with similar fits for the prototypical SN 2010gx (a faster-fading object), we found that the key property is the strength of the magnetic field: a weaker field means that the magnetar radiates away its energy more slowly, such that significant heating persists to later times, and gives the impression of a radioactive tail in some cases.

The similarity of our objects to SN 2007bi suggests that it was not a pair-instability explosion either, leaving no clear examples of nearby supernovae of that type, and perhaps returning that mechanism to the realm of theory for the time being. To investigate how common pair-instability explosions might be in the local Universe, we ran a simulated transient survey, following the cadence of the *Pan-STARRS* Medium Deep Survey, to look for synthetic pair-instability supernovae based on Kasen's models. We found that, if present, such supernovae should be easily detectable — so if such events do occur at low redshift, they are extraordinarily rare. It is hoped that next-generation instrumentation will allow us to search for these explosions at high redshift, as such massive stars may be more common in the early Universe.

The President. Questions?

Mr. M. F. Osmaston. On the basis that coronal mass ejections are clearly shown to accelerate as they move away from the Sun, have the ejecta for supernovae been tracked at all as far as its velocity is concerned? One of the problems that I foresee in doing that is that the spectrum of the emission from the material coming out is actually changing so you're not at all sure, if you look at it at two different times, that you're looking at the same stuff.

Mr. Nicholl. In these magnetar models one of the key properties has been the magnetar winds sweeping the ejecta into a thin shell. This means you see fairly constant velocity over the whole evolution. In fact that is exactly what we saw in PTF12dam. The velocity of all the lines was about 10 000 km s^{-1} for all the observed evolution.

Professor I. Crawford. For the pair-instability ones would you expect to see gamma rays when the positrons annihilate?

Mr. Nicholl. All pair creation takes place in the core, and because there is so much matter around you would likely lose all the gamma rays before they got anywhere near the surface. There have been very few gamma-ray observations of these kinds of events. Higher-energy observations of super-luminous supernovae would be interesting to obtain.

Professor Christine Done. If these are magnetars, at what time does the ejecta become optically thin so you might have a hope of seeing the period and really testing this?

Mr. Nicholl. Our estimates of PTF12dam and similarly massive supernovae suggest that it would take of order several hundred days before the ejecta become fully optically thin. The problem is that they become very faint at those times so are hard to observe. I am not sure we have any hope of seeing the magnetars, unfortunately.

The President. Thank you very much. [Applause.] That brings us to the end of the presentations. May I invite you to a seasonal drinks reception in the RAS Library now. Lastly I will give notice that the next A&G Open Meeting of the Society will be on Friday, 2014 January 10.

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 236: HD 45762, HD 74089, HD 194765, HD 197952 (NN Del), HDE 353012, AND HD 215622

By R. F. Griffin
Cambridge Observatories

Orbits are presented for six binary systems having short periods (all but one of them <100 days). The last two of them were initially observed inadvertently and there is very little previous knowledge about them. HD 45762 is a giant star in a circular orbit with a period of 60 days; *Hipparcos* saw photometric variations with exactly half that period, perhaps arising from ellipticity of the star. The system has been reported as double-lined, but only the primary has been measurable here. HD 74089 is a very unequal double-lined system with a circular orbit having a period of only 22 days although the primary, at least, appears to be a late-type giant. HD 194765 is a bright ($6^{m}\cdot7$), conspicuously double-lined, main-sequence system with a period of 161 days and an orbital eccentricity of 0·26. NN Del is known as an eclipsing binary with a period of 99 days; the secondary eclipse is offset from the midpoint between successive primary ones, so the orbit was known to be eccentric. The radial velocities agree very closely with the period and eccentricity (0·517) found photometrically. HDE 353012 is a tenth-magnitude double-lined system with slightly unequal components, in a 67-day orbit with an eccentricity of 0·32. HD 215622 is single-lined; its orbit has a period of 21 days and an eccentricity of 0·37.

Introduction

The interesting batch of stars treated in this paper found their way onto the Cambridge observing programme by different paths. Two of them, HD 45762 and HD 194765, were among the 37 stars in the *CABS3* catalogue (*A catalogue of chromospherically active binary stars* (*third edition*)[1]) that, in late 2008, the compilers thereof invited the writer to observe with a view to providing orbits for objects that then lacked them. I had actually given orbits already for 47 of the 409 stars that featured in that *Catalogue*, and since many of the 37 'new' objects proved to have short orbital periods it was barely a year after receiving the request before I was able[2,3] to give 25 more (five of them[3] in collaboration with one of the compilers). Only a little later still I offered in this *Magazine* a paper[4] that included a sort of index to the 84 *CABS3* stars that I had observed; the paper was mostly concerned with a demonstration that six of the 84 chromospherically active stars seemed to have constant velocities. Seventy-two (47 + 25, as mentioned just above) had orbits; there remained just six that were still under observation. In four cases the orbits were not at that time properly determined because they had comparatively long periods, but in two cases the difficulty was not so much the length of the orbital periods as of the observing season, because the two stars — HD 45762 and HD 194765 — are south of the celestial equator. Their orbits are given here; actually, that of HD 45762 has been largely pre-empted by one recently published by a syndicate overlapping those that compiled previous editions of the *CABS*.

HD 74089 was discovered to be double-lined in the course of a renewed radial-velocity survey in 'Clube Selected Area 4'[5], and its short period has allowed its orbit to be determined before the new survey itself is ready for publication. NN Del distinguished itself in the *Hipparcos* photometry by a sudden brief fade of half a magnitude, which identified it as an eclipsing system, but since only one such event was seen no period or light-curve could be given.

It is admitted a bit sheepishly that the remaining two stars, HDE 353012 and HD 215622, arrived on the observing programme serendipitously through errors on the part of the observer. In the case of the *HDE* star, on one occasion he accidentally mis-set the telescope by 10° in declination when he intended to observe NN Del. (The setting is done from analogue dials very similar to clock dials; the declination one has a 'minute hand' that turns one revolution for 10°, while the 'hour hand' has a scale with 10° divisions.) The star did seem rather faint for NN Del, although it gave a double-lined radial-velocity trace all right — but the character of the trace and the velocities of the stars were not quite as expected! The mistake was thus recognized 'in real time' and the position of the new double-lined system that had thus inadvertently been discovered was carefully noted and transferred to the observing programme. The discrepancy between the position of HDE 353012 and that of (NN Del + 10°) is only 2·5 minutes of arc. The writer's error in the case of HD 215622 was a bit less reprehensible, as the star was mistaken for the very similar-looking one, only 3½ minutes of arc north, with the adjacent *HD* number, 215621, the latter star being observed as part of a renewed survey of 'Clube Selected Area 10'.

HD 45762

It has been noted above that HD 45762 is one of two *CABS3*[1] stars whose observations were begun in 2008 but could not quickly be concluded owing to the shortness of the observing season at their low declinations. HD 45762 is an 8^m star below $-5°$ declination; it is about a degree and a half north of the multiple-B-star system β Mon, nearly half-way from Sirius to Betelgeuse. Exact magnitudes cannot be given for it, because it is significantly variable; that was discovered by *Hipparcos*, which found for it a period of 29·968 days, with a range between '*Hp*' magnitudes 8·367 and 8·509; the light-curve, plotted in Vol. 12 of the *Hipparcos* publication, appears to be sinusoidal, but the listing of variable stars in Vol. 11 does not venture to attribute any particular type to the variation. Kazarovets *et al.*[6], however, who on behalf of the IAU assigned the variable-star designation V723 Mon to HD 45762, classified it as 'SRD:', and *Simbad* adopts that type, giving the supposed nature of the object in its principal heading as 'Semi-regular pulsating star'.

There is one actual *advantage* in the low declination of HD 45762, inasmuch as a modern spectral type is given in Houk's catalogue of revised types for the *HD* stars, which covers from the South Pole only to as far north as +5°. There[7] it is classified as G0 II. The (revised[8]) *Hipparcos* parallax, however, though only four times as great as its own uncertainty, indicates that the mean absolute magnitude is about $+0^{m}{\cdot}9 \pm 0^{m}{\cdot}4$, so the luminosity seems to be just about what the books[9–11] quote for a normal giant, although Keenan & Barnbaum[12] have demonstrated from *Hipparcos* parallaxes that some revision of the tabular quantities towards higher luminosities is warranted. No such revision should affect the calibration of the tables that give stellar radii in the same books, which agree that a G0 III star with an absolute magnitude near +1 has a radius of about 6 $R_{\odot}$.

The *ASAS*[13] photometric catalogue reports no fewer than 552 measurements of HD 45762, from which it produces a beautiful light-curve of the β Lyr type — the brightness changes smoothly and continuously, the curve exhibiting two equal maxima and two unequal minima. The period is given as 59·930 days. It will be noticed that that period now agrees with the orbital one mentioned in the initial summary above, instead of being half as much, as was found by *Hipparcos*. The latter's observations were less accurate, and by mischance were sparse near minimum light, so the alternation of the depths of the minima escaped attention. According to *Simbad*, the star features in the AAVSO's *International Variable Star Index*[14] as being of type EB (eclipsing binary) and has a period of 59·9300000000 days, whose extraordinary apparent precision would be reprehensibly misleading if it were not exposed by its equally extraordinary blatancy. The range of types of variability that have been assigned to HD 45762 is truly amazing; we have already encountered SRD and EB, and others are briefly recounted in the ensuing paragraphs.

The literature reports several efforts to deduce certain characteristics of large numbers of stars from *other* characteristics determined in surveys. If their results on HD 45762 are anything to go by, they are none too reliable. Thus, Richards *et al.*[15], in *The Machine-learned ASAS Classification Catalog* [*sic*], reported the classification of the types of variability of a lot of stars. Although they recorded the photometric period of HD 45762 as 59·93032 days (whose precision, though 10^5 times 'rougher' than that of the AAVSO, must still be illusory), their machine classified it as a W UMa variable, which is *defined* in the *GCVS*[16] as an eclipsing binary with a period less than one day. One can only conclude that the machine must not have been taught to take into account certain relevant known facts.

Another demonstration of the power of machinery to do classification work 'blind' is afforded by the effort of Pickles & Depagne[17] to deduce spectral classifications from photometry assembled from a number of catalogues ranging from the ultraviolet to the infrared. The resulting type of K4 V for a system that we have to believe to be (at least principally) about G0 III is scarcely a good advertisement for either the catalogue or the method. Could interstellar reddening (HD 45762 is at –7° Galactic latitude) have such dramatic consequences?

Still another automated scheme[18] produced a type of 'RS + BY', *i.e.*, RS CVn plus BY Dra — both of which are types of variation worked by star-spots. One of the defining characteristics[16] of both types is strong Ca II *H* & *K* emission, yet HD 45762 had previously been explicitly listed[19] as exhibiting *no* such emission. Risking a criticism of repetition, one has to conclude that the automatic machinery must not have been taught to take into account the relevant known facts.

It will be shown immediately below that the star is a giant and that its orbital period is 59·94 days, just twice the photometric period found by *Hipparcos* and almost identical with that determined by *ASAS*[13], so it seems very possible that the variation is due to the distortion of the star by a close companion into an ellipsoid, tidally locked to the orbital revolution. The system could not, however, in good conscience be proposed as an ellipsoidal variable ('ELL' in *GCVS* parlance), because that catalogue[16] asserts that "Light amplitudes do not exceed $0^{m}\cdot1$ V", whereas HD 45762 has a total range of $0^{m}\cdot2$. It is a fact that somewhat analogous light-curves are produced by eclipsing binaries of the β Lyr type ('EB') whose specifications[16], however, include the caveats that they are *usually* of early spectral types *and* have *considerably* unequal minima — neither of which characteristics is exhibited by HD 45762.

HD 45762 was placed on the Cambridge radial-velocity programme as soon as that was requested by the compilers of *CABS3*[1]; owing to the unfavourable position of the star, which is almost 30° south of the ecliptic and is accessible to the Cambridge telescope only within two hours of the meridian, only ten observations were obtained in the first observing season. They sufficed, however, to yield a preliminary quasi-circular orbit with a period of just under 60 days; that period was promptly reported in the paper[4] described above as including an index to the 84 *CABS3* stars that I had observed. The star has been retained on the observing programme and there are now 41 measurements, listed in Table I, for the determination of the orbit, which is illustrated in Fig. 1.

Simbad does not report any radial velocities at all in the 'measurements' section of its bibliography for HD 45762; it lists only five papers referring to the star (it omits mine[4]!), none of whose titles is particularly promising as regards radial velocities. In reviewing the literature before writing this paper, however, the writer discovered that the most recent of the five retrieved papers, by Strassmeier *et al.*[20], includes a very nice orbit for the star! The observations were made, with very good accuracy and presumably effortlessly as far as those authors were concerned, robotically with a telescope in the Canary Islands. While they were about it, they re-observed a number of the 72 *CABS3* stars for which I had already given orbits. Table II shows the orbital elements found for HD 45762 by Strassmeier *et al.* as well as the writer's (which will be seen to be outclassed in the comparison!). The Cambridge radial-velocity traces normally appeared to be single-lined; at least, no additional feature that could routinely be measured for radial velocity could be seen in them. Strassmeier *et al.*, however, who were dealing with actual spectra and not just cross-correlation functions, reckoned to see a second component, itself the primary of a single-lined binary system, in the spectrum of HD 45762; their results will receive comment below.

The eccentricity is very small, but even in the Cambridge orbit (plotted in Fig. 1) it is more than five times its standard deviation and so is probably real.

TABLE I

Cambridge radial-velocity observations of HD 45762

Date (UT)			*MJD*	*Velocity km s⁻¹*	*Phase*	*(O–C) km s⁻¹*
2008	Dec.	10·09	54810·09	+58·8	0·703	+0·3
		27·09	827·09	+24·0	·987	+0·4
2009	Jan.	3·02	54834·02	−25·4	1·103	−1·1
		6·97	837·97	−46·1	·169	+0·1
		14·02	845·02	−62·3	·286	−0·3
		20·96	851·96	−44·6	·402	+0·1
	Feb.	3·98	865·98	+40·0	·636	+0·3
	Mar.	23·81	913·81	−34·5	2·434	+0·8
		25·82	915·82	−24·9	·467	−1·0
	Oct.	25·19	55129·19	+5·4	6·027	−1·4
	Dec.	1·09	166·09	+41·4	·643	−0·6
2010	Mar.	1·89	55256·89	−42·1	8·158	+1·1
		4·90	259·90	−54·4	·208	+0·8
		6·89	261·89	−61·0	·241	−1·0
	Oct.	7·22	476·22	+67·0	11·817	−0·8
	Nov.	24·11	524·11	+31·7	12·616	−1·2
		28·13	528·13	+54·2	·683	+0·5
	Dec.	12·12	542·12	+50·7	·917	+1·7
		17·07	547·07	+19·0	·999	+0·3
2011	Jan.	18·98	55579·98	+6·9	13·548	−0·2
	Mar.	8·85	628·85	−54·0	14·363	−0·3
	Oct.	16·20	850·20	−3·8	18·056	+1·7
	Nov.	28·11	893·11	+67·0	·772	−1·0
	Dec.	5·13	900·13	+56·0	·889	−0·4
		8·07	903·07	+41·5	·939	−0·4
		10·10	905·10	+28·5	·972	−1·0
		15·09	910·09	−4·9	19·056	+0·3
		18·06	913·06	−26·2	·105	−1·0
2012	Jan.	4·01	55930·01	−48·1	19·388	+0·3
		12·96	938·96	+4·6	·537	+1·8
		17·00	943·00	+28·2	·605	−0·6
		24·00	950·00	+63·4	·721	+1·3
	Feb.	1·98	958·98	+60·8	·871	+0·3
	Mar.	7·86	993·86	−27·3	20·453	+1·6
	Nov.	3·17	56234·17	−27·2	24·463	−1·5
		6·20	237·20	−8·0	·513	−1·3
	Dec.	6·08	267·08	+14·9	25·012	+1·4
		10·12	271·12	−15·9	·079	−1·0
		26·06	287·06	−56·6	·345	+0·4
2013	Nov.	9·18	56605·18	+46·2	30·652	+1·2
		20·08	616·08	+65·7	·834	−0·6

That can be checked objectively by the usual statistical test, the F test explained in this *Magazine* by Bassett[21], in which the sums of the squares of the deviations from the circular and eccentric solutions of the same data are compared. In the former case there remain $(n - 4)$ degrees of freedom (where n is the number of observations) after the four elements of the circular orbit have been fitted; in the latter case there are $(n - 6)$. The difference in the sums of squares has to be laid at the door of the two extra degrees of freedom. The actual sums are 70·80 $(\mathrm{km\ s^{-1}})^2$ for the circular solution and 37·21 for the solution with e free,

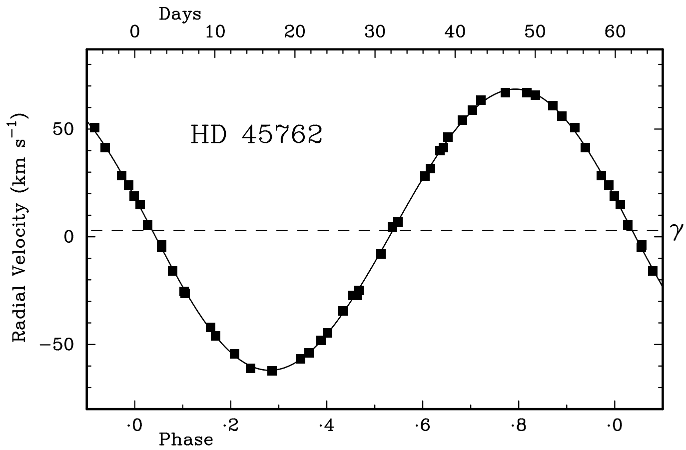

FIG. 1

The observed radial velocities of HD 45762 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. All the observations were made with the Cambridge *Coravel*.

so the two extra degrees cost the circular solution an extra 33·59 (km s^{-1})^2, or 16·8 per degree. The remaining 35 degrees cost the 37·21 (km s^{-1})^2 of the eccentric solution — 1·06 per degree. The variance ratio, F = 16·8/1·06, about 15·8, can be compared with tables (*e.g.*, ref. 22) of F that give the significances of such quotients for the relevant numbers of degrees of freedom. We find that even the 0·1% point of $F_{2,35}$ is only 8·47, so 15·8 is very significant indeed. That already tells us all that we need to know, as is further reinforced by the fact that the already-published orbit[20] independently arrives at a similar eccentricity and longitude of periastron, with even smaller standard errors. But if we were keen to learn the actual numerical significance level, we could apply to web sites, *e.g.*, http://www.danielsoper.com/, where one can enter the numbers and find that the probability of obtaining such an F ratio just by chance is only 0·0000129.

TABLE II

Orbital elements for HD 45762

Element		*This paper*	*Strassmeier et al.*[20]
P	(days)	59·9385 ± 0·0037	59·9363 ± 0·0016
T	(MJD)	55607·1 ± 1·9*	54110·40 ± 0·61
γ	(km s^{-1})	+2·98 ± 0·17	+2·23 ± 0·048
K	(km s^{-1})	65·27 ± 0·25	65·447 ± 0·064
e		0·0192 ± 0·0034	0·0150 ± 0·0009
ω	(degrees)	77 ± 11	84·6 ± 3·7
$a_1 \sin i$	(Gm)	53·78 ± 0·21	53·935 ± 0·053
$f(m)$	($M_\odot$)	1·730 ± 0·020	
R.m.s. residual (km s^{-1})		0·95	0·533

*T_0 = MJD 55594·298 ± 0·031

Because the eccentricity is so very small and its uncertainty (in the Cambridge case) is nearly a fifth of the eccentricity itself, the longitude of periastron is uncertain by about a fifth of a radian. That does not mean, however, that the phasing of the whole velocity curve is so uncertain: the uncertainty of its positioning along the time axis is given by that of T_0, the time of maximum velocity of recession, whose value is given in the footnote to Table II. Its uncertainty is about one-sixtieth (perhaps not coincidentally about *e* times) as great as that of *T*.

A striking feature of the Cambridge orbital elements is the huge mass function, of 1·73 $M_{\odot}$. In straightforward single-lined binary systems, it is normally reasonable to suppose that the component whose velocity it has been possible to measure is the more massive one. In that case, both components must have masses more than four times the mass function. Not only do such masses seem unreasonable here, where one of the components is believed to be a star whose type is about G0 III, but Strassmeier *et al.* have reported the companion object to be itself a binary, so it may well have a greater mass than the star whose velocity has been measured, and therefore 'all bets are off'. The masses of late-type giants cannot be estimated accurately: not only is their part of the Hertzsprung–Russell diagram a collecting ground for stars that have evolved from quite a range of the main sequence and so have started with disparate masses, but (depending on how far the evolution of any given object may have progressed) they may (but in an individual case may not) have lost a very significant proportion of their original mass. To give an indication of the implication of the mass function, however, we may say that if we take the mass of the observed star as 2 $M_{\odot}$ then the *minimum* mass that is required for the companion object is about 4 $M_{\odot}$, whereas if we thought the observed one to be only 1 $M_{\odot}$ then the other would have to be at least 3 $M_{\odot}$. Thus, for a plausible range of masses for the G0 III star, the companion object must be much more massive; and since *it* is said by Strassmeier *et al.* to be a single-lined object, it would seem that the second star that they were able to measure must, itself alone, be more massive than the G0 one. Unfortunately, although they report having seen and measured it in the spectrum, they give no indication at all as to what sort of star it is. An A-type main-sequence object might satisfy the mass function and not overwhelm the spectrum to a degree such that the late-type giant could scarcely be measured with the *Coravel*; but if that were the case, the present giant, having evolved first, ought to have come from still higher up the main sequence than the A star and could be expected to be of higher luminosity — as indeed would correspond to Houk's[7] classification of G0 II but seems scarcely to be reconciled with the parallax[8].

In Strassmeier *et al.*'s paper[20], the radial velocity of the second star (whose measurements are comparatively ragged — most of the other velocity curves in that paper look 'like beads on a string') — behaves in a very odd fashion. It has a period exactly one-third of that of the obvious component, a situation that seems unlikely to be dynamically stable★. Its velocity curve duly crosses that of

★The smallest widely accepted ratio between the periods of hierarchical orbits is believed to be the factor of about 8 that has long been attributed to the λ Tau system, whose inner binary also exhibits eclipses. The photometric variability was discovered with the naked eye from Southport in 1848 by Baxendell[23], who (incredibly enough) determined its 4-day period in the selfsame month (specified only as 'inst.' in his note but probably 1848 November) as that on the 6th of which he discovered it. Schlesinger[24] gave a single-lined spectroscopic orbit for it 101 years ago; he adopted the photometric period of 3·9529 days but was aware that it sometimes failed to represent the actual times of eclipse, and said that "If we assume that the velocities are affected by an additional oscillation with a period slightly in excess of one month, the residuals … can be considerably reduced". The actual period of the outer orbit was eventually determined at just over 33 days by Ebbighausen & Struve[25].

the obvious star at the γ-velocity, but, instead of being a mirror image, it turns back towards the γ-velocity in such a way as to exhibit an approach to it just when the obvious star is at an orbital node. It seems such an unlikely situation that one might hope that an alternative explanation could be found for whatever is seen to happen in the spectrum.

If the 'T_0' listed by *ASAS* is correctly understood by the writer to be a truncated Julian date of an epoch of primary minimum, it coincides quite closely (within 0·01 in phase) with an epoch of maximum velocity of recession, implying that the system appears faintest when the component that is the one whose radial velocity is easily measured and is apparently ellipsoidal must be expected to be seen side-on as opposed to end-on. It is hard to understand how that can happen, if indeed the information given by *ASAS* has been correctly construed here.

It should be mentioned that Strassmeier *et al.* themselves obtained photometry for HD 45762, but its whole duration was only 49 days; since its period is listed as 62 days (differing, therefore, from the orbital period but inevitably not very accurate) and it is referred to an epoch several years before the actual measurements, its phasing seems unlikely to be trustworthy. Strassmeier *et al.*, however, say, "Our photometry shows a double-humped light curve with a clear 62-day periodicity but must be partly due to an ellipticity effect because of its phase coherence with the orbit.". The photometry itself is not accessible; neither are the radial velocities, so it is not possible to add to Table II above a panel giving a joint solution to complement the two separate ones shown there.

The Cambridge radial-velocity traces indicate a mean $v \sin i$ of about 15 km s^{-1} for HD 45762, but they give the impression that the apparent rotational velocity is phase-dependent. In Fig. 2 the rotational velocities yielded by the individual traces are plotted against their respective phases given in Table I. It is tempting to see structure in the diagram, in the form of a double wave with minima near phases ·25 and ·8. Those are in fact about the phases of the nodes of the orbit; the conjunctions, at which the star is seen as nearly end-on as the (unknown but certainly high) orbital inclination may allow, occur near phases ·04 and ·54, just about where there appear to be maxima in the rotational velocities. Such a relationship does not seem to make any sense. It would be quite understandable for the very massive companion implied by the

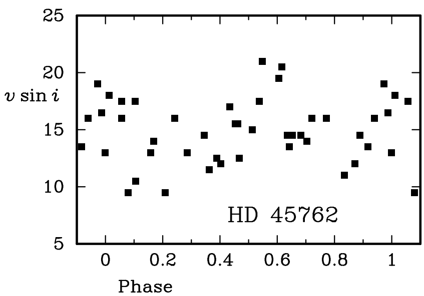

FIG. 2

The apparent rotational velocity of HD 45762, plotted as a function of the phases given for the respective observations in Table I. It is thought that there is suggestion of a variation with a periodicity of half the orbital period.

extraordinary mass function to distort the observed component of HD 45762 into an ellipsoid, but that ellipsoid would appear side-on at the nodes of the orbit, when the visible area of the star surface would be a maximum and where both its brightness and its apparent rotational velocity could be expected to be maximal. Perversely, they both appear to be minimal at those phases. It has also to be admitted that there are several other pieces missing from the HD 45762 jigsaw, including explanations of the very large mass function and the anomalously small ratio of the period proposed by Strassmeier *et al.*[20] for the companion object to the 60-day orbit of the obvious star.

A mean $v \sin i$ of 15 km s^{-1} is nearly a quarter of the radial-velocity amplitude K, so *if* it could be understood in terms of rotation synchronized to the orbital revolution it would imply a stellar radius of nearly a quarter of $a_1 \sin i$ — say 12 Gm or about 17 $R_{\odot}$. That is nearly three times the radius expected from the parallax-based luminosity, so it would require the real parallax to be little more than a third of the listed[8] value — about 1·2 arc-milliseconds in place of the listed 3·33. That is formally 2·8 standard deviations away from the central value, but perhaps when it is considered that the quoted standard deviation will have its own uncertainty, it might not be entirely outwith the bounds of possibility. Then the luminosity would be greater than is suggested above by a factor of about 8, or in magnitude terms about $2^{m}\cdot 2$, bringing it into much better agreement with the value implied by the MK classification[7] of G0 II. Acceptance of the substantially higher luminosity would also alleviate to some extent the difficulty posed by the large mass function.

HD 74089

The *Introduction* above explains that this star came to attention through its qualifying for inclusion in 'Clube Selected Area'[5] no. 4; the dimensions of the Area were deemed to be increased (although the position of the centre and the whole spirit of the enterprise were retained) in order to embrace an increased number of stars, which all have to be of type K0 in the *Henry Draper Catalogue*. Area 4 is centred at Galactic coordinates l = 180°, b = +35°, approximately $\alpha = 8^{h}\ 30^{m}$, $\delta = +40°$. (It is viewed with particular favour by the observer, as it passes near the Cambridge zenith at a sidereal time when, between the Hyades and the North Galactic Pole field, there are few other calls on the observing time!) HD 74089 itself passes only 12′ from the zenith; another agreeable feature is that at $8^{m}\cdot 53$ (transformed from *Tycho 2* by *Simbad*) it is almost half a magnitude brighter than the nominal mean magnitude of Clube stars. The $(B - V)$ colour index, from the same source, is $0^{m}\cdot 88$.

Being reasonably bright and in an area of the sky not heavily populated by brighter ones, HD 74089 featured in the *Hipparcos* survey, from which a (revised[8]) parallax of only 1·85 ± 1·05 milliseconds of arc was derived, corresponding to a distance modulus of about $8\cdot 7^{+1\cdot 8}_{-1\cdot 0}$ magnitudes — so the object is clearly of approximately the luminosity of a giant star of its spectral type. *Simbad*, however, cites a paper[26] that refers to X-ray (*ROSAT*) sources[27] as giving the star's distance as 22·07 ± 0·69 pc, obtained by a method called 'ST-L', but I have not located that misinformation in the paper.

There do not seem to be any radial velocities of HD 74089 in the literature; the star was first observed with the Cambridge *Coravel* spectrometer as recently as 2012 February. The initial observation showed the cross-correlation 'dip' to be substantially broadened, to a degree representing a $v \sin i$ of about 20 km s^{-1}. That created such interest in the object that it was re-observed at the first opportunity and found then to have changed its velocity by nearly 50 km s^{-1} in five days! —

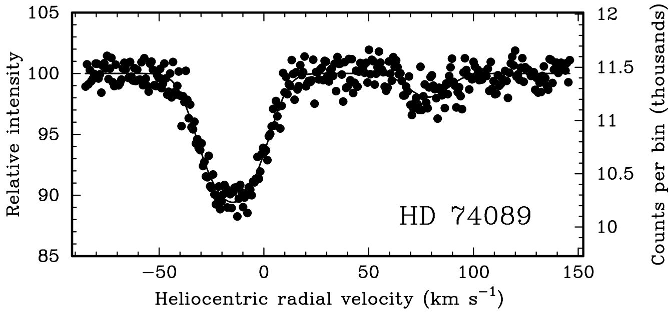

FIG. 3

Radial-velocity trace of HD 74089, obtained with the Cambridge *Coravel* on 2012 December 6; it illustrates the very unequal dips and the unusual width of the primary one, which arises from a star whose projected rotational velocity is some 20 km s^{-1}.

not at all normal behaviour for a late-type giant. Continuing attention soon demonstrated the extraordinarily short orbital period of about 22 days. Soon after the opening of the second observing season, in 2012 December, the system was belatedly recognized as double-lined, the radial-velocity traces possessing a weak secondary dip only about 1/7 of the strength of the primary. A *Coravel* trace taken nearly at a node of the orbit appears here as Fig. 3.

The existence of the secondary is so obvious in the figure that the writer feels that the delay in its recognition calls for some excuse! In fact the secondary was sometimes outside the range of velocity over which the cross-correlation function was scanned — imagine a trace consisting only of the left-hand half of Fig. 3 — or else it was blended with, and/or masked by, the primary dip; and many of the traces were integrated only to about half photon-count level of the one illustrated, whose integration time was about an hour. On still other occasions, if the weak dip were adjacent to the primary one it could make the whole trace appear to be on a slight slope, which *Coravel* traces sometimes *are* anyway; a slope is 'corrected' (in inverted commas in this present instance!) as part of the routine reduction procedure, which in this context actually has the helpful effect of largely compensating for the apparent 'dragging' of the primary dip towards the γ-velocity by the blending with the secondary.

There are now 39 radial velocities for the primary star and 20 for the secondary; they are set out in Table III and, with the Secondary weighted ¼, yield the orbit that is illustrated in Fig. 4 and whose elements are as follows:

P	$= 21{\cdot}9126 \pm 0{\cdot}0017$ days	T_0	$=$ MJD $56255{\cdot}501 \pm 0{\cdot}016$
γ	$= +30{\cdot}38 \pm 0{\cdot}15$ km s^{-1}	$a_1 \sin i$	$= 14{\cdot}11 \pm 0{\cdot}07$ Gm
K_1	$= 46{\cdot}85 \pm 0{\cdot}23$ km s^{-1}	$a_2 \sin i$	$= 15{\cdot}36 \pm 0{\cdot}19$ Gm
K_2	$= 51{\cdot}0 \pm 0{\cdot}6$ km s^{-1}	$f(m_1)$	$= 0{\cdot}234 \pm 0{\cdot}003\ M_\odot$
q	$= 1{\cdot}088 \pm 0{\cdot}014\ (= m_1/m_2)$	$f(m_2)$	$= 0{\cdot}301 \pm 0{\cdot}011\ M_\odot$
e	$\equiv 0$	$m_1 \sin^3 i$	$= 1{\cdot}11 \pm 0{\cdot}03\ M_\odot$
ω	is undefined in a circular orbit	$m_2 \sin^3 i$	$= 1{\cdot}020 \pm 0{\cdot}017\ M_\odot$

R.m.s. residual (unit weight) $= 0{\cdot}9$ km s^{-1}

TABLE III

Cambridge radial-velocity observations of HD 74089

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
2012 Feb.	19·00	55976·00	+31·6	—	0·245	−0·3	—
	24·99	981·99	−16·8	—	·518	−0·6	—
	25·95	982·95	−12·1	—	·562	+0·9	—
Mar.	1·94	987·94	+39·8	—	·790	−2·1	—
	3·92	989·92	+64·9	—	·880	+0·4	—
	7·90	993·90	+73·5	—	1·062	−0·3	—
	8·06	994·06	+72·2	—	·069	−0·7	—
	9·91	995·91	+59·9	—	·153	+2·8	—
	11·98	997·98	+31·0	—	·248	0·0	—
	14·94	56000·94	−4·2	—	·383	+0·1	—
Apr.	1·90	018·90	+43·8	—	2·202	−0·4	—
	10·91	027·91	−4·5	—	·614	+0·5	—
	15·91	032·91	+56·7	—	·842	+0·8	—
	17·93	034·93	+74·7	—	·934	+1·4	—
May	15·88	062·88	+39·9	—	4·209	−2·3	—
Nov.	11·23	242·23	−6·4	—	12·394	+0·1	—
	18·21	249·21	+21·1	—	·713	+1·6	—
	19·20	250·20	+31·8	—	·758	−0·9	—
Dec.	2·18	263·18	+4·0	+59·3	13·350	+1·2	−1·1
	6·14	267·14	−15·8	+81·4	·531	−0·2	+1·0
	26·14	287·14	−13·2	+79·9	14·444	+0·4	+1·7
2013 Jan.	3·14	56295·14	+47·2	+10·6	14·809	−0·1	−1·3
Feb.	3·01	326·01	+38·2	+14·4:	16·218	−1·6	−5·7
	6·99	329·99	—	+74·4	·399	—	+2·9
Mar.	5·95	356·95	−2·5	+66·9	17·630	−0·7	+1·5
	12·02	363·02	+68·7	−10·4	·907	−0·7	+1·7
	13·96	364·96	+76·7	−18·8	·995	−0·5	+1·8
	29·03	380·03	+10·2	+48·6	18·683	−1·0	−2·7
Apr.	2·01	384·01	+60·8	−3·7	·864	−0·4	−0·5
	5·89	387·89	+76·0	−18·4	19·042	+0·4	+0·5
	6·92	388·92	+70·8	−11·4	·089	+0·6	+1·6
	15·92	397·92	−15·3	—	·499	+1·2	—
	17·98	399·98	−8·9	+73·0	·593	−0·2	+0·1
	19·86	401·86	+9·3	+50·0	·679	−0·9	−2·4
Oct.	29·25	594·25	−14·5	—	28·459	+0·4	—
	30·25	595·25	—	+81·5	·505	—	+0·1
Nov.	9·24	605·24	+76·3:	—	·960	+0·5	—
	13·23	609·23	+60·7	−0·9	29·143	+1·0	+0·6
	19·22	615·22	−10·6	+74·4	·416	−0·5	0·0
Dec.	28·11	654·11	+46·6	+14·3	31·191	−0·8	+2·5
2014 Jan.	5·12	56662·12	−14·2	+80·0	31·556	−0·6	+1·8

The component of the HD 74089 system that gives the principal dip in radial-velocity traces is pretty certainly responsible for the majority of the luminosity of the system, which we know from the parallax has the absolute magnitude of a late-type giant. The orbital period is so short that a giant star in such an orbit is almost certainly in synchronous rotation, so we can use the rotational velocity found for the primary star to set a lower limit to the stellar radius. The mean value of $v \sin i$ is 22·1 ± 0·2 km s^{-1}; it leads to a radius of 9·5/sin i $R_{\odot}$.

The masses of the components differ by only 9%, and the secondary itself has a raised rotational velocity, quantified by the radial-velocity traces at

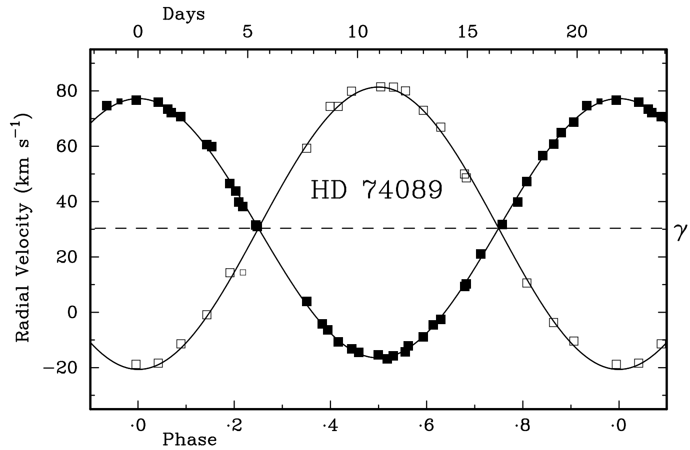

FIG. 4

The observed radial velocities of HD 74089 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. The filled squares plot the velocities of the primary, the open ones the secondary. Smaller symbols refer to velocities that were regarded at the time of observation as being less certain than normal; they are identified with colons in Table III and were given half-weight in the solution of the orbit.

$11{\cdot}3 \pm 0{\cdot}8$ km s^{-1} — just about half that of the primary. The most probable way of understanding the near-equality of the masses, the difference in strength of the radial-velocity signatures of the components in the *Coravel* traces, and the rotational velocity of the secondary, is in terms of a secondary star that, like the primary, is an evolving object rotating in the orbital period, but has not evolved as far as its companion: it is lower down on the giant-branch evolutionary track in the H–R diagram, is only half the size of the primary, and being of earlier type gives a weaker dip than would correspond arithmetically to its relative surface area.

The information available is hardly sufficient to support a detailed model of the system, but from the brief discussion above one could hazard a guess that the components' spectral types are about G8 III and G2 III–IV, and the orbital inclination is near to 45°. The sum of $a_1 \sin i$ and $a_2 \sin i$ is 0·20 AU, so at the estimated inclination the actual separation of the components should be about 0·28 AU and their angular separation should be at most 0·28 times the parallax — only of the order of half a millisecond of arc.

HD 194765

HD 194765 is one of the many stars from the *CABS3* catalogue[1] that were placed on the writer's observing programme in late 2008. It proved to be a spectroscopic binary — the bare fact of which could actually have been discovered from the literature — but the observations of it were not satisfactorily distributed in phase at the time that the principal results of the *CABS3* work were written up[2,4]; the information was given[4], however, that the object is double-lined and has an orbital period of about 160 days. It is in a visually rather barren region of the sky in Aquila, about 14° south-following Altair; it is about 2° south of the equator, so the observing season from Cambridge is rather short. On the more positive side, it is quite a bright star, bright enough to have been included in the *Supplement to the Bright Star Catalogue*, where its magnitudes are given as $V = 6^{m}{\cdot}70$, $(B-V) = 0^{m}{\cdot}52$, $(U-B) = 0^{m}{\cdot}02$ (no doubt quoted from Johnson & Morgan[28] and/or Johnson[29], although closely similar values have been obtained by other observers[30–33]). The spectral type, F8 in the *Henry Draper Catalogue*, was recognized to be that of a main-sequence star by Adams *et al.*[34], who found the star to have a spectroscopic absolute magnitude of $+3^{m}{\cdot}5$; it has been classified on the MK system as F8 V[35], F6 U [*sic*] (ref. 36), and F7 V[37].

Attention has been drawn to HD 194765 through its being the primary of a wide binary system known as S 749, after the observer who in 1826 first listed it, South[38]. The secondary is one minute of arc almost due south (just a coincidence, no joke intended!), and although its right ascension is very slightly preceding that of the primary star its *HD* number is the one immediately following (HD 194766) — the two stars were in the same 0·1-minute zone of RA in equinox-1900 coordinates, within which the *HD* numbers were assigned successively from north to south. The magnitude difference is $0^{m}{\cdot}8$, and by the time Burnham came to compile his great 1906 catalogue[39] of visual binaries, in which the pair is no. 10216, he was able to comment that the stars showed no relative motion although they shared a very significant proper motion. Similarly, Aitken[40], cataloguing the system anew in 1932 as ADS 13868, noted it as a common-proper-motion pair. If any further support were needed for the conclusion that the stars form a physical pair, it would be provided by the parallaxes, whose values (revised[8] from *Hipparcos*) are 21·84 ± 0.58 and 21·32 ± 0·57 milliseconds of arc, respectively.

The radial velocity of HD 194765 appears first to have been published by Christie & Wilson[41] in 1938, from Mount Wilson. They gave a mean value of –16·7 km s^{-1}, with a 'probable error' of 1·7 km s^{-1}, as the mean of five plates; they also gave the spectral type as F2. The five velocities were much later listed individually by Abt[42], and it appears from their quoted dispersion of 36 Å mm^{-1} at Hγ that they were all taken with the 'γ' spectrograph on the 60-inch reflector. They were of course all measured as single-lined, and when plotted on the orbit presented below they exhibit little relationship to it. Struve & Zebergs[43] included ADS 13868 in a programme of radial-velocity measurements of "some visual double stars", and took one spectrogram of each component with the beautiful 10-Å mm^{-1} system of the Mount Wilson 100-inch coudé spectrograph. We can see in retrospect that they were unlucky in happening to observe the primary star just at a time when it was single-lined, so they did not discover its duplicity. They found a difference of 2·6 ± 0·6 km s^{-1} between the velocities of the two visual components. They remarked, "Johnson's colors (1953) are identical for both components. Spectra are very similar." They assigned a type of F8 and a 'rotation class' of 0 to both stars; the rotation class was defined thus "Zero stands for lines of perfect sharpness, as in Procyon."

Nordström *et al.*[44] included both visual components of the binary in their survey of F and G dwarf stars. They reported obtaining nine radial velocities of the primary over a total interval of 7 years, and gave a mean velocity of –15·4 km s^{-1} with a standard error of only 0·1 km s^{-1}, although the standard error per measurement was given as 9·7 km s^{-1}. There is a flag to indicate that the mean is intended to represent the systemic velocity of a double-lined binary; that could explain why its standard error is so much smaller than the single-observation value divided by $\sqrt{n}$. The adjacent entry in their table giving the results of the programme pertains to the visual secondary, but unfortunately does not include any measurement of its radial velocity.

Halbwachs *et al.*[45] recently presented an orbit for HD 194765, a somewhat belated deduction from data obtained with the Haute-Provence *Coravel* in 1990–1998. They had altogether 72 radial velocities, counting those of each component and even those of blends individually. They published a double-lined orbit, whose elements are reproduced in the first (left-hand) panel of Table V. The underlying radial velocities are accessible by computer. They probably include the nine referred to by Nordström *et al.*[44], since both of Halbwach's co-authors were also members of the Nordström consortium; the only question is why the latter seems to have been privy to only *some* of those recently discussed. I have transcribed the velocities in order to experiment with them. Halbwachs *et al.* seem not to mention any relative weighting of the components, but since one of them gives a substantially deeper dip than the other, some weighting would certainly be appropriate; moreover, the mean residuals that Halbwachs *et al.* give for the two components are not as different from one another as they surely would be if all the data had been equally weighted. In running an orbit solution on their data myself, I took certain liberties with the data set. In the first place, where the two stars had been observed in separate consecutive integrations (sometimes necessary with the OHP *Coravel* owing to the limited range of velocity that could be scanned at one time), an averaged time was assigned to both measurements. Then, there were cases in which two measurements had been made in quick succession on the same night; in such cases, of which there were five for the primary and two for the secondary, the pairs of measurements were considered not to be truly independent and were averaged both in time and in velocity and treated as one. The five observations which had been reduced by the OHP authors as single-lined were nevertheless incorporated in their solution of the orbit by a process that they do explain but which I could not easily follow. In any case it is hard to believe that they could contribute anything of significance to the outcome, however cleverly they might be treated, and in re-computing the OHP orbit I have not hesitated to follow my own convention by excluding them. After those (individually minor) amendments to the data set, and half-weighting the measurements of the secondary component, I obtain the elements listed in the second panel of Table V. They are in fact not very different from those given by the original authors, but their standard deviations are modestly larger. The principal reason for the increase in standard errors is no doubt that the omission of duplicate and blended observations has reduced the count of the effective data from 72 to 60, with a corresponding reduction of the divisor of the sum of squares of residuals when it comes to determining the error of the mean from that of an individual observation.

As related above, HD 194765 was placed on the Cambridge radial-velocity programme in late 2008; it has been observed until late 2013, and 61 measurements have been made of it*. They are set out in Table IV, and an example is illustrated by Fig. 5. The first five, on which the two dips in the radial-velocity traces appeared unresolved, were reduced as single-lined and not used in the solution of the orbit. The other 56 observations have all been reduced as double-lined and form the basis of the orbit whose elements occupy the third panel of Table V; the corresponding diagram appears here as Fig. 6. The variances of the velocities of the two components have been brought into

**Here*, one integration at the telescope is counted as 1, even though two radial velocities may be determined from it.

TABLE IV

Radial-velocity observations of HD 194765

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

Date (UT)	*MJD*	*Velocity*		*Phase*	*(O – C)*	
		Prim. *km s⁻¹*	*Sec.* *km s⁻¹*		*Prim.* *km s⁻¹*	*Sec.* *km s⁻¹*
1926 Oct. 20·13*	24808·13	−13·2		$\overline{186}$·055	—	—
1928 Aug. 29·26*	25487·26	−28·0		$\overline{182}$·277	—	—
1929 June 18·46*	25780·46	−24·2		$\overline{180}$·099	—	—
1931 May 27·46*	26488·46	−13·2		$\overline{176}$·501	—	—
1932 Oct. 16·17*	26996·17	−15·0		$\overline{173}$·657	—	—
1953 Aug. 19·35†	34608·35	−12·6		$\overline{126}$·980	—	—
2008 Nov. 7·81	54777·81	−15·2		0·370	—	—
14·77	784·77	−14·2		·413	—	—
18·76	788·76	−13·7		·438	—	—
22·76	792·76	−13·7		·462	—	—
Dec. 6·74	806·74	−12·1		·549	—	—
9·71	809·71	−6·9	−21·7	·568	−0·3	0·0
2009 May 31·11	54982·11	−3·2	−26·2	1·640	−0·4	−0·1
June 2·11	984·11	−1·9	−26·6	·652	+0·3	+0·2
12·10	994·10	+0·2	−29·8	·714	−0·1	0·0
24·09	55006·09	+2·1	−31·6	·789	+0·2	−0·1
July 7·11	019·11	−0·4	−29·0	·870	0·0	−0·1
20·12	032·12	−9·7	−17·4	·951	+0·2	+0·5
28·02	040·02	−18·7	−7·5	2·000	−0·1	+0·3
30·10	042·10	−20·4:	−6·2:	·013	+0·5	−1·0
Aug. 8·04	051·04	−28·6	+4·5	·068	−0·3	+1·0
11·98	054·98	−30·2	+6·5	·093	−0·1	+1·0
16·02	059·02	−31·0	+7·3	·118	0·0	+0·7
19·96	062·96	−31·1	+7·2	·142	+0·1	+0·5
29·96	072·96	−29·3	+5·2	·204	0·0	+0·6
Sept. 3·95	077·95	−28·1	+3·2	·235	−0·5	+0·5
9·91	083·91	−25·1	+0·1	·272	+0·3	0·0
12·95	086·95	−24·1	−1·5	·291	+0·1	−0·2
18·88	092·88	−21·9	−4·2	·328	−0·1	0·0
20·91	094·91	−20·8	−5·3	·341	+0·1	−0·2
25·88	099·88	−18·7	−7·0	·372	+0·2	+0·5
Oct. 19·85	123·85	−8·9	−18·5	·521	+0·4	0·0
22·83	126·83	−8·2	−19·9	·539	0·0	−0·1
Nov. 3·74	138·74	−4·6	−23·9	·613	−0·5	+0·7
2010 June 15·10	55362·10	−19·6	−8·0	4·002	−0·6	−0·7
17·07	364·07	−21·6	−6·0	·014	−0·4	−1·2
23·09	370·09	−26·2	+1·4	·052	+0·4	−0·1
Aug. 11·06	419·06	−20·4	−6·3	·356	−0·5	0·0
Sept. 16·94	455·94	−5·8	−23·0	·585	−0·2	−0·2
Oct. 10·88	479·88	+1·0	−29·8	·734	0·0	+0·7
27·82	496·82	+1·2	−30·0	·839	+0·1	+0·7
Nov. 11·80	511·80	−7·4	−21·5	·933	−0·3	−0·4
2011 Sept. 10·89	55814·89	+1·3	−31·2	6·817	−0·4	+0·1
27·89	831·89	−5·9	−22·7	·923	−0·2	+0·1
28·89	832·89	−6·3	−20·5	·929	+0·3	+1·3
30·89	834·89	−8·2	−19·5	·941	+0·2	+0·1
Oct. 7·91	841·91	−16·7	−9·2	·985	−0·7	+1·7

TABLE IV (*concluded*)

Date (UT)		*MJD*	*Velocity* Prim. km s⁻¹	Sec. km s⁻¹	*Phase*	*(O – C)* Prim. km s⁻¹	Sec. km s⁻¹
2011 Oct.	14·88	55848·88	–23·6	–3·1	7·028	–0·2	–0·9
	15·86	849·86	–24·0	–1·7	·034	+0·3	–0·5
	18·86	852·86	–26·4	+1·4	·053	+0·3	–0·2
	19·85	853·85	–27·2	+2·0	·059	+0·2	–0·4
	22·84	856·84	–29·1	+5·1	·078	0·0	+0·7
Nov.	19·77	884·77	–26·8	+1·2	·251	–0·1	–0·4
2012 July	25·98	56133·98	+2·3	–31·6	8·801	+0·4	–0·1
Aug.	8·99	147·99	–1·9	–27·3	·888	–0·1	0·0
	10·02	149·02	–2·3	–26·5	·894	+0·1	+0·1
	20·03	159·03	–10·6	–17·4	·956	+0·3	–0·7
2013 July	1·07	56474·07	–4·7	–23·7	10·915	0·0	+0·2
	18·03	491·03	–22·4	–4·3	11·020	–0·2	–0·6
Sept.	2·95	537·95	–22·7	–3·5	·312	+0·1	–0·6
	14·93	549·93	–17·9	–8·1	·386	0·0	+0·5
Oct.	29·78	594·78	–1·4	–27·5	·665	+0·2	0·0
	30·75	595·75	–1·4	–27·3	·671	–0·1	+0·5
Nov.	4·73	600·73	–0·4	–29·2	·702	–0·3	+0·1
	12·74	608·74	+1·4	–31·3	·752	0·0	–0·3
	14·75	610·75	+1·4	–31·4	·765	–0·2	–0·1
	21·73	617·73	+2·1	–31·7	·808	+0·3	–0·2

* Mount Wilson photographic observation[41,42], weight 0.
† Mount Wilson photographic observation[43], weight 0.

approximate equality by weighting the velocities of the secondary ⅓. There is no obvious explanation for the run of positive residuals seen in the secondary's measures in the vicinity of the node near Day 20 of the orbit.

The OHP velocities have considerably larger residuals than the Cambridge ones, notwithstanding that the photon rates are higher at OHP and the star itself is 9° higher in the sky because of the lower latitude there. The OHP data, however, extend the time base from five years to 23, and so must be expected to

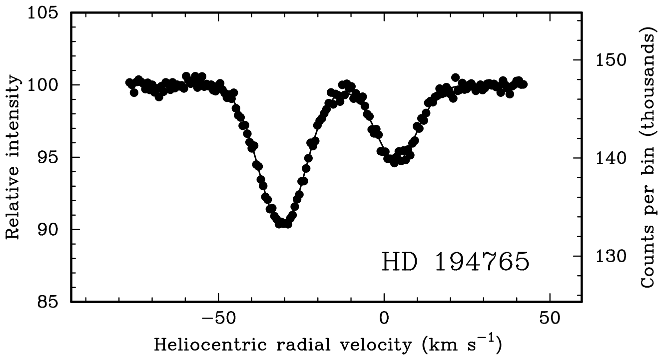

FIG. 5

Radial-velocity trace of HD 194765, obtained with the Cambridge *Coravel* on 2009 August 29.

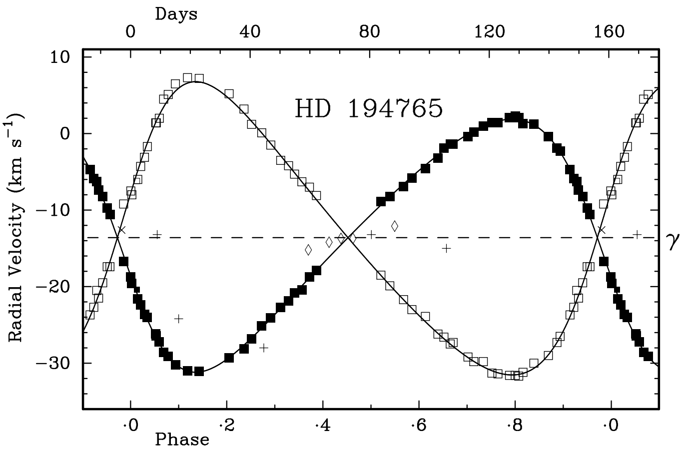

FIG. 6

As Fig. 4, but for HD 194765. Open diamonds plot the first five Cambridge observations, which were reduced as single-lined and not used in the solution of the orbit. The five plusses and one cross relate to the early Mount Wilson photographic observations[41–43], which were all measured as single-lined.

(and do in fact) offer a significant refinement to the orbital period. Accordingly, the two data sets that independently give rise to the sets of elements shown in the second and third panels of Table V have been amalgamated to give a final solution on the right-hand side of that table. To equalize the variances the OHP data have been assigned a global weighting of ¼ of the corresponding weights of the Cambridge set. Then, to harmonize the zero-points, the OHP velocities have all been adjusted by +1·7 km s^{-1}, and because that step was necessary before they could be used in the joint solution the same adjustment has been made to the γ-velocities quoted in the first two panels of Table V just so that all the panels are immediately comparable. It is readily admitted that the zero-point of the OHP data set is probably more nearly correct in the absolute sense than that of the Cambridge set, and it is only for homogeneity within Table V that the OHP ones have been chosen for adjustment. It has long been noted in this series of papers that there is a discrepancy of 0·8 km s^{-1} between the OHP and Cambridge zero-points, and in recent years the discrepancy has been shown to be larger for stars of the earlier types among those discussed in this series. A recent paper[46] in another journal, where there was an opportunity to compare the well-determined mean velocities of a fair number of constant-velocity stars, demonstrated a quasi-linear variation with colour index in the offset between OHP and Cambridge velocities. It could be approximated by the equation $\Delta V = -1{\cdot}98 + 0{\cdot}91(B - V)$, which if extrapolated considerably beyond the blue limit of the colour indices from which it was constructed would suggest an offset of 1·51 km s^{-1} for a star of the colour of the HD 194765 system — perhaps not too remote from the 1·7 km s^{-1} derived empirically here.

This is not really an appropriate place to launch into a major discussion of radial-velocity zero-points, but lest the above remarks are seen as exhibiting a reprehensibly casual attitude on the part of the author towards what is clearly a serious problem, it could be remarked that γ-velocities are in any case none too certain. They are 'falsified' by the best part of a kilometre per second in the cases of main-sequence stars by the gravitational red-shift (a tabulation of that quantity, assessed as a function of type and luminosity, is to be found in ref. 47), and are further affected in a type-dependent fashion by convection currents such as those that create solar granulation, whereby the brighter parts of the stellar surface, where hotter material is upwelling, are systematically blue-shifted with respect to dimmer areas. That effect has been explicitly documented in the observed radial velocities of stars of different colour indices in the same star cluster (*cf*. ref. 48, Fig. 1).

TABLE V

Orbital elements for HD 194765

Element	*Halbwachs et al.*[45]	*Same, re-computed*	*Cambridge solution*	*Joint solution*
P (days)	160·831 ± 0·057	160·82 ± 0·07	160·855 ± 0·019	160·833 ± 0·007
T (MJD)	49251·30 ± 0·82	49251·3 ± 1·0	55683·50 ± 0·29	54879·51 ± 0·28
γ (km s^{-1})	−13·555 ± 0·081	−13·64 ± 0·10	−13·60 ± 0·04	−13·60 ± 0·03
K_1 (km s^{-1})	16·303 ± 0·128	16·35 ± 0·16	16·53 ± 0·06	16·51 ± 0·05
K_2 (km s^{-1})	19·297 ± 0·211	19·31 ± 0·22	19·16 ± 0·11	19·20 ± 0·09
q	—	1·181 ± 0·018	1·159 ± 0·008	1·163 ± 0·007
e	0·2536 ± 0·0071	0·251 ± 0·009	0·2601 ± 0·0025	0·2594 ± 0·0023
ω (degrees)	106·5 ± 1·8	106·5 ± 2·1	104·2 ± 0·8	104·7 ± 0·7
$a_1 \sin i$ (Gm)	34·87 ± 0·26	35·01 ± 0·35	35·30 ± 0·13	35·27 ± 0·12
$a_2 \sin i$ (Gm)	41·28 ± 0·43	41·3 ± 0·5	40·92 ± 0·23	41·01 ± 0·20
$f(m_1)$ ($M_\odot$)	—	0·0663 ± 0·0020	0·0679 ± 0·0007	0·0677 ± 0·0007
$f(m_2)$ ($M_\odot$)	—	0·1092 ± 0·0038	0·1057 ± 0·0018	0·1064 ± 0·0016
$m_1 \sin^3 i$ ($M_\odot$)	0·3688 ± 0·0086	0·372 ± 0·011	0·367 ± 0·005	0·368 ± 0·004
$m_2 \sin^3 i$ ($M_\odot$)	0·3116 ± 0·0059	0·315 ± 0·008	0·3165 ± 0·032	0·3168 ± 0·0029
R.m.s. residual (wt. 1) (km s^{-1})	0·769	0·63	0·29	0·30

In the cases of 35 of the best-resolved radial-velocity traces the mean equivalent widths of the two dips expressed in terms of kilometres per second (the abscissae of the traces, analogous to wavelengths in the case of spectroscopic equivalent widths) are 1·78 and 0·83, giving a ratio close to 1:0·47, or $0^{m}{\cdot}83$ expressed in stellar-magnitude terms. It was found in a substantial previous investigation[49] that the actual difference in V magnitude for pairs of F- and G-type main-sequence stars is close to 1·15 times the magnitude difference corresponding directly to the measured equivalent widths of the 'dips'. (The factor is greater than unity because of the increase in general spectroscopic line-strengths towards later spectral types.) On that basis, the Δm_V of HD 194765 is found to be $0^{m}{\cdot}95$, with an uncertainty likely to be less than a tenth of a magnitude.

HD 194765 is known to be $0^{m}{\cdot}8$ brighter than its visual companion, whose colour indices have been found to be identical and whose spectral type is the same. Obviously, the basic reason for its being brighter is its duplicity. A pair of stars with a Δm of $0^{m}{\cdot}95$ is jointly $0^{m}{\cdot}38$ brighter than the primary alone — so that explains half of the observed magnitude difference. The other half is largely explained on the grounds that in order to exhibit jointly the same spectral type and colour indices as the (constant-velocity and presumed single) visual companion, the primary star has to be earlier and therefore (as a main-sequence star) brighter than the type found from the pair in combination. That can be illustrated by the summation in the informal table below of the absolute V, B, and U magnitudes of a pair of main-sequence stars of types F6·5 and G2. The V magnitudes and the colour indices are interpolated from the table of properties to be found in *AQ*[9]. There is some slight indeterminacy in the interpolations because the tabular relationships between spectral type and the photometric quantities are mildly non-linear, but the interpolations were honestly made to the best of the writer's ability *before* the summation of the magnitudes was effected — so no advantage has been taken of any indeterminacy to make the result appear better than is really warranted!

	Star	*Type*	M_V	$(B-V)$	$(U-B)$	M_B	M_U
Model	Aa	F6·5 V	3·7	0·47	0·03	4·17	4·20
Model	Ab	G2 V	4·65	0·63	0·10	5·28	5·38
Model	Aa + Ab		3·32	0·52	0·04	3·84	3·88
	A (observed)	~ F8 V	6·70*	0·52	0·02		
	Tabulated[9]	F8 V	4·0	0·52	0·04		

**Apparent* magnitude

In the first two lines of the table, the colour indices are successively added to the M_V to give the M_B and M_U magnitudes; they are added together to obtain the magnitudes in the third line which then give the colour indices of the combination by subtraction.

The model binary is seen to fit the observed facts almost embarrassingly closely! The Δm between its components has of course been deliberately engineered by the choice of their types, and the colour indices of the combination accurately reproduce the observed ones. The combined absolute magnitude is $0^m·68$ brighter than that tabulated for type F8 V, differing from the Δm of $0^m·80$ observed between the binary and its F8 visual companion by an amount much less than corresponds to one spectral sub-type. Moreover, comparison of the model absolute magnitude with the observed apparent magnitude shows the distance modulus of the model to be $3^m·38$, while the (revised[8]) parallaxes of HD 194765 and its visual companion are $3^m·30$ and $3^m·36$, respectively, each having a standard error of about $0^m·06$.

The spectroscopic secondary star has no measurable rotational velocity, but the primary's lines are significantly broadened, to a degree indicating a rotational velocity whose mean value, from the 35 well-resolved traces mentioned above, is 6·5 ± 0·3 km s^{-1}, although the (normally relatively minor) effects of other broadening mechanisms, neglected here, impel a caveat that the numerical value is not to be regarded as more accurate than ±1 km s^{-1}.

HD 197952 (*NN Delphini*)

HD 197952 is (usually) an $8^m·4$ star that is (always!) about 8½° due south of γ Del, the left-hand star of the little quadrilateral of Delphinus as seen by northern observers. It did not command any particular attention from astronomers until 1994 — three years before the *Hipparcos* results were published comprehensively — when Makarov *et al.*[50] listed it in a brutally brief *IBVS Bulletin* as one of "35 new bright medium- and high-amplitude variables discovered by the TYCHO instrument of the HIPPARCOS satellite". They identified it only by a *GSC* designation[51] and, fortunately, by its coordinates, and the only information that they gave for it was that it had an amplitude exceeding $0^m·5$ and its type was "eclipsing?". When the *Hipparcos* catalogue[52] was actually published, in 1997, the object came with its variable-star designation NN Del ready-made, although the fact of its designation was not formally announced[6] until two years later. At that time its photometric period was unknown: the *Hipparcos* volume 12 that plots light-curves[52] shows it as having a constant magnitude except for one deep (apparently $0^{m}\!\cdot\!6$) and very brief fade that surely identified it as an eclipsing object. Its spectral type was known only from the *Henry Draper Catalogue* as F5. Although one might expect that the interest shown in its eclipses would have led to its out-of-eclipse magnitudes in different colours being well established, it seems that that is not the case, and even now we have to fall back on *Simbad*'s transformation of the *Tycho 2* V_T and B_T magnitudes to find that $V = 8^{m}\!\cdot\!40$, $(B-V) = 0^{m}\!\cdot\!54$. Such a colour index is on average associated[9] with a main-sequence spectral type of about F8.

NN Del seems to have commanded considerable interest in Germany, whence five of the total of 12 papers that are retrieved for the system by *Simbad* were published in *BAV Rundbrief*. Schirmer[53] first tried, in 2002, to determine the period, and offered three trial values near 10 days, which are actually not even near to being sub-multiples of the real period, which is nearly 100 days. The true period was established in the following year by a syndicate of Spanish observers. Their very nice investigation was published by Gomez-Forrellad *et al.* in *Astrophysics & Space Science*[54], but still better in an *astrogea*[55] item on the Web. They found the period to be 99·268 days, the eclipse depths to be $0^{m}\!\cdot\!54$ (primary) and $0^{m}\!\cdot\!49$ (secondary), and — from the fact that the secondary eclipses are by no means half-way between primary ones but instead come at phase ·188 — the orbital eccentricity to be 0·5176. They put the semi-major axis of the orbit at 100 Gm, and concluded that the component stars (assumed equal) are subgiants rather than main-sequence ones, with radii of 2·2 $R_{\odot}$. Attention was promptly drawn to that work in *BAV Rundbrief* by Hassforther[56], who shortly afterwards drew further attention to it as an example of how things could be done in the introduction to a paper[57] on another variable star. The following year Schirmer[58] returned with observations of additional minima.

In 2011 the writer belatedly made a trawl through vol. 12 of the *Hipparcos Catalogue* to look for interesting objects to place on his radial-velocity programme. There were disappointingly few suitable candidates: most of the exciting ones were out of declination and/or spectral-type range, or their orbits were already known. (That was in some cases the fault of the writer himself! — for example, V454 and V455 Aur and UW LMi were already treated in Paper 160[59], where the *Hipparcos* period of 2·7 days for the first-named was shown to be in error (the true period is 27 days) and the period of V455 Aur, unknown to *Hipparcos* but extremely close to being π days, was established for the first time.)

NN Del was one of the few 'new' objects selected for observation, and was reasonably assiduously monitored in the 2012 and 2013 observing seasons. It gives a trace that is conspicuously double-lined, as is exemplified here in Fig. 7. There are 37 observations, set out in Table VI; all but two have yielded velocities for both components. At the first observation, only the primary dip was noticed, the smaller one being almost beyond the end of the scan; and on 2013 July 8 there was scarcely any secondary dip because the system happened to be in eclipse. In view of the early spectral type and the fact that the cross-correlation dip is split into two by the double-lined nature of the star, the individual dips are quite shallow and the radial velocities derived from them are accordingly somewhat less accurate than usual. They will be seen from Fig. 8, however, to delineate the orbit quite nicely; the velocity amplitudes are large because

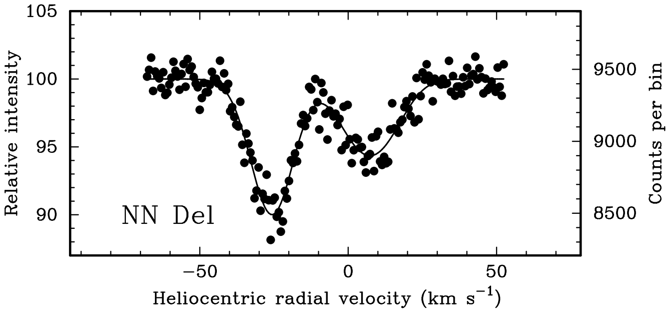

FIG. 7

Radial-velocity trace of NN Del (HD 197952), obtained with the Cambridge *Coravel* on 2012 August 22.

the early spectral type means that the components are quite massive, and the eclipses seen in the system mean that the orbital inclination is very close to 90°. Thus the $\sin i$ factors in the expressions for the orbital radii and the mass function are to all intents and purposes unity, and have been so taken in the listing of orbital elements that follows:

P	= 99·244 ± 0·019 days	T_6	= MJD 56389·39 ± 0·06
γ	= −8·19 ± 0·08 km s^{-1}	a_1	= 42·11 ± 0·26 Gm
K_1	= 36·04 ± 0·21 km s^{-1}	a_2	= 46·09 ± 0·33 Gm
K_2	= 39·45 ± 0·27 km s^{-1}	$f(m_1)$	= 0·303 ± 0·006 $M_\odot$
q	= 1·095 ± 0·010 (= m_1/m_2)	$f(m_2)$	= 0·397 ± 0·008 $M_\odot$
e	= 0·5168 ± 0·0029	m_1	= 1·454 ± 0·025 $M_\odot$
ω	= 350·4 ± 0·4 degrees	m_2	= 1·328 ± 0·021 $M_\odot$

R.m.s. residual (unit weight) = 0·56 km s^{-1}

The photometric quantities that can be compared with those above are the orbital period of 99·2684 ± 0·0005 days, the eccentricity, 0·51759 ± 0·00002 (its precision difficult to believe!), and (similarly!) the longitude of periastron, 171·710 ± 0·005 degrees, which as expected is reversed from the spectroscopic value. The epochs can also be compared, but not so directly, because the photometric epoch is that of eclipse whereas the spectroscopic one is that of periastron. The secondary photometric minimum is reported to come at phase ·1881 with respect to the primary minimum; the two conjunctions are predicted by the spectroscopic orbit to be at phases ·9221 and ·1113 from periastron, differing therefore by ·1892 of the period. Of course it is to be expected that the timings of eclipses will be more accurately determined photometrically, where the whole of the changes take place in a matter of hours, than spectroscopically, where the times are inferred from measurements that change gradually over the whole orbital period. If the interval between the photometric and spectroscopic epochs, 6161·80 days, is divided by the 62·0779 intervening orbital periods (an integer plus the time between the spectroscopic phase of the photometric primary minimum, ·9221, and periastron), we obtain the period as 99·259 days, to which the uncertainty of the spectroscopic epoch contributes an uncertainty of 0·010 days. So within their quite small uncertainties, the periods derived

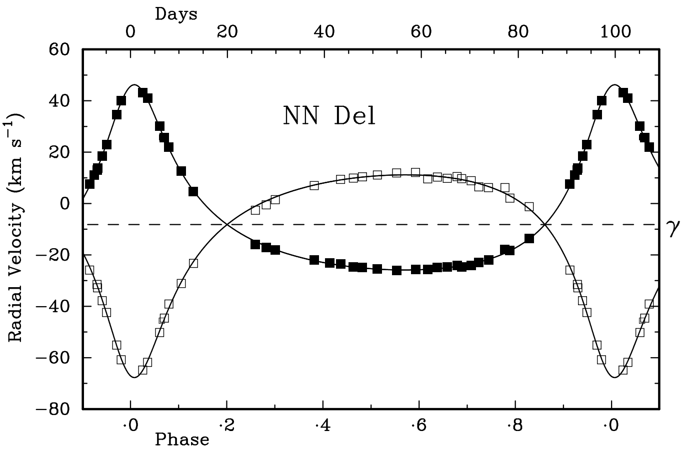

FIG. 8

The observed radial velocities of NN Del plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them.

independently by photometry and spectroscopy, and by their comparison across the 17-year interval between them, are in complete agreement.

The parallax of NN Del is[8] 6·28 ± 0·89 arc-millseconds and leads to a distance modulus of about 6·0 ± 0·3 magnitudes and thus to an absolute magnitude for the NN Del system as a whole of $+2^{m}\cdot 4$, with the same uncertainty. That would make the average absolute magnitude, per star, to be $3^{m}\cdot 15$, about[9] the value for a main-sequence star of type F3, whose colour index is expected to be about $0^{m}\cdot 37$, whereas the observed value for NN Del is $0^{m}\cdot 54$. There may be slight reddening in the ~160-pc sightline between NN Del and here, but not enough to explain the difference. The Spanish investigation[55,56] found the fractional radius of the primary star to be 0·0153 — that is the radius expressed as a fraction of the semi-major axis of the relative orbit, which they took as 100 Gm and thereby obtained a value of 2·2 $R_{\odot}$. The semi-major axis found here ($a_1 + a_2$ in the informal table above) is 88·2 Gm; substitution of that value reduces the stellar radius to just under 2 $R_{\odot}$, but it is still large enough to characterize mid-A main-sequence stars rather than late-F. The stellar masses found here, too, are appreciably larger than tables of main-sequence properties allow for stars of types near F7 and F9 such as the mass ratio, in association with the combined colour index, might lead us to expect. The weight of evidence points, therefore, to the stars being somewhat over-massive and over-luminous in comparison with main-sequence objects of their colour index, so we could agree with the Spanish authors that the stars are somewhat evolved; they could perhaps be classed as one sub-type or so on either side of F8 IV–V.

The inequality of the depths of the eclipses, neither of which is total, does not tell us directly much about the relative sizes of the stars. The Spanish papers show the secondary star as being slightly larger, though slightly cooler, than the primary, and to have a bolometric luminosity about 3% higher. That does not agree too well with the appearance of Fig. 7, where the two dips are seen to be considerably unequal. Their mean equivalent widths, averaged over the 33 traces in which they were allowed as independent parameters, are 1·86 and

TABLE VI

Cambridge radial-velocity observations of NN Del

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 Sept. 30·96	55834·96	−23·2	—	0·414	+0·2	—
2012 Aug. 20·05	56159·05	−24·0	+10·5	3·679	+0·5	+0·9
21·00	160·00	−24·6	+9·7	·689	−0·4	+0·4
22·97	161·97	−24·1	+8·9	·709	−0·6	+0·4
29·95	168·95	−17·9	+6·2	·779	+1·4	+2·2
30·98	169·98	−18·3	+2·1	·789	+0·1	−0·9
Sept. 3·96	173·96	−13·7	−1·2	·829	+0·1	+0·9
14·01	184·01	+13·8	−32·8	·931	−0·1	−0·5
14·98	184·98	+18·4	−37·8	·940	−0·2	−0·3
15·88	185·88	+23·0	−42·4	·949	−0·4	+0·4
17·97	187·97	+34·6	−55·1	·970	−0·3	+0·2
18·90	188·90	+40·0	−60·8	·980	+0·5	−0·4
Nov. 5·87	236·87	−24·7	+9·9	4·463	0·0	0·0
10·84	241·84	−25·5	+11·1	·513	0·0	+0·3
14·80	245·80	−26·0	+11·9	·553	−0·2	+0·8
18·72	249·72	−25·7	+12·1	·593	+0·1	+1·0
Dec. 1·79	262·79	−23·0	+6·5	·724	−0·2	−1·3
3·79	264·79	−22·0	+6·2	·745	−0·3	−0·4
2013 May 3·12	56415·12	−15·9	−2·6	6·259	−1·2	−1·6
July 7·11	480·11	+7·6	−25·9	·914	+0·7	−1·2
8·10	481·10	+11·1	—	·924	+0·1	—
18·07	491·07	+43·2	−64·8	7·025	−0·5	+0·1
19·08	492·08	+41·0	−61·8	·035	+0·7	−0·6
22·01	495·01	+25·8:	−45·6:	·064	−1·9	+1·9
26·05	499·05	+12·7	−31·1	·105	+0·5	−0·6
Aug. 27·96	531·96	−23·5	+9·4	·437	+0·6	+0·2
Sept. 14·96	549·96	−25·7	+9·6	·618	−0·1	−1·3
16·97	551·97	−24·9	+10·3	·638	+0·5	−0·3
19·00	554·00	−24·6	+9·8	·659	+0·4	−0·4
Oct. 15·89	580·89	+13·1	−31·5	·930	−0·3	+0·3
28·82	593·82	+30·1	−50·2	8·060	+0·5	−0·7
29·80	594·80	+25·6	−44·6	·070	+0·3	+0·3
30·74	595·74	+21·9	−39·1	·079	+0·5	+1·5
Nov. 4·81	600·81	+4·6	−23·3	·130	−0·4	−0·7
19·84	615·84	−17·1	−0·5	·282	−0·5	−1·5
21·72	617·72	−18·1	+1·5	·301	−0·1	−1·0
29·77	625·77	−21·9	+7·0	·382	+0·3	−0·2
Dec. 9·73	635·73	−24·8	+10·4	·482	+0·3	+0·1

1·02 km s^{-1}; that is a ratio of 1:0·55, or expressed in terms of stellar magnitudes it is 0^{m}·65. It would not be appropriate to try to deduce the actual Δm_V from that, as was done in the case of HD 194765 above, since we cannot assume a particular relationship between spectral type and luminosity in the present case, where we cannot rely on the stars being near the main sequence. We can say with more assurance that the ~9% difference in the masses of the stars implies[60] that their original positions on the main sequence would have differed by approximately three sub-types and would have led to a difference in absolute magnitude *then* quite similar to the difference that is now seen in the dip areas in Fig. 7. Another point of interest is that the radial-velocity dips from both stars exhibit broadening that is very likely to arise from axial rotation, and can be quantified from the traces where the two dips are well resolved to be 7·4 ± 0·3 and 7·7 ± 0·6 km s^{-1} for the primary and secondary, respectively. Rotational velocities sometimes have significance if they can be understood in terms of synchronism with the orbital revolution (as was proposed to be the case with HD 74089 above), but in an eccentric orbit synchronization occurs not at the orbital period itself but more nearly at the rate of angular motion at periastron. At the eccentricity of the NN Del orbit, the 'pseudo-synchronous' period has been shown[61] to be shorter than the orbital one by a factor of about 2·9, making it about 34 days. If that were the period of rotation of the components of NN Del, they would have to have radii of about 5 $R_{\odot}$ — which is unthinkable, so we must conclude simply that both stars must be rotating substantially faster than pseudo-synchronism would require.

HDE 353012

The serendipitous discovery of this double-lined spectroscopic binary as a result of a mistake on the part of the author has been described in the *Introduction* above. *Simbad* knows of only one paper[62] referring to it — one that describes a catalogue wherein its position and proper motion are listed, since the original designation of the star, together with its classification as G0, was indicated only on a chart[63]. The position could be approximately read from coordinates marked round the margins of the chart. (When you see even one chart, in a volume containing hundreds, it makes you marvel that Miss Cannon could have done such an amazing job, and you can understand that towards the end of her life she could not afford the enormous amount of extra work and delay that would be created by determining positions, too, of all the stars so that they could be listed in a catalogue, as in the earlier volumes of the *Henry Draper Catalogue*[64] and its *Extensions*[65].)

The little extra information that can be found for HDE 353012 is gleaned from the thumbnail picture that appears with its *Simbad* bibliography, which shows a 'conspicuous' visual companion (not obvious to the eye, however, in the telescopic field), about 11″ distant in p.a. ~63°, and the *V* and (*B* – *V*) photometry from *Tycho 2*, 9^{m}·98 and 0^{m}·70, respectively.

Once the binary had been inadvertently discovered, on 2012 August 30, it was industriously observed. Despite an unpleasant interruption of about seven weeks to the observations while demolition took place of the room that

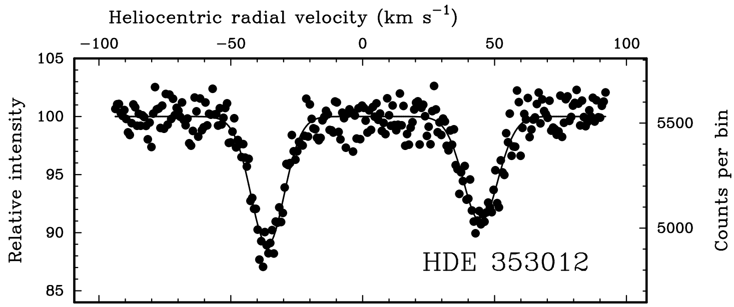

FIG. 9

Radial-velocity trace of HDE 353012, obtained with the Cambridge *Coravel* on 2013 October 29.

TABLE VII

Cambridge radial-velocity observations of HDE 353012

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*
2012 Aug. 30·99	56169·99	+32·3	−22·7	0·756	−0·3	+0·8
Sept. 3·98	173·98	+29·0	−19·3	·816	−0·1	+0·6
6·97	176·97	+23·8	−12·5	·861	+1·1	+0·7
7·93	177·93	+19·9	−9·1	·875	+0·2	+1·0
9·96	179·96	+10·9	−2·4	·905	−0·7	−0·8
13·02	183·02	−5·2	+16·7	·951	−0·1	+1·0
13·99	183·99	−11·3	+20·9	·966	−0·2	−1·0
15·00	185·00	−17·6	+28·6	·981	−0·4	+0·3
15·89	185·89	−22·0	+33·5	·994	+0·2	−0·1
18·00	188·00	−31·8	+43·6	1·026	−0·1	+0·2
18·94	188·94	−33·8	+46·7	·040	+0·6	+0·5
Nov. 5·89	236·89	+31·8	−23·6	·757	−0·7	—
10·85	241·85	+28·4	−17·7	·831	+1·0	+0·4
11·73	242·73	+25·2	−17·0	·844	−0·3	−0·8
14·83	245·83	+15·9	−6·6	·891	0·0	−0·5
Dec. 1·84	262·84	−29·6	+40·9	2·145	+0·1	−0·4
2·76	263·76	−27·1	+38·5	·159	+0·3	−0·4
3·82	264·82	−24·5	+35·8	·175	+0·2	−0·3
8·71	269·71	−13·2:	+23·7:	·248	−1·3	+0·9
9·80	270·80	−10·1	+19·7	·264	−0·9	−0·3
2013 May 3·11	56415·11	+12·1	−2·8	4·422	−0·3	−0·3
June 7·08	450·08	−1·6	+13·4	·945	+1·2	0·0
14·07	457·07	−36·5	+47·1	5·050	−0·8	−0·5
25·09	468·09	−17·7	+25·8[R]	·215	−0·1	−2·9
July 1·08	474·08	−2·9	+14·1	·304	0·0	+0·6
10·07	483·07	+13·8	−3·7	·439	−0·5	+0·7
18·06	491·06	+25·1	−16·6	·558	−0·2	−0·7
19·06	492·06	+26·2	−17·4	·573	−0·2	−0·4
26·02	499·02	+32·3	−23·1	·677	+0·3	−0·2
Sept. 3·99	538·99	−6·7	+17·8	6·275	+0·7	−0·3
4·98	539·98	−4·9	+16·3	·290	+0·2	+0·6
16·98	551·98	+17·4	−8·3	·470	0·0	−0·6
19·02	554·02	+20·6:	−12·3:	·500	+0·2	−1·5
Oct. 15·90	580·90	+10·8:	−3·3:	·902	−1·8	−0·6
28·83	593·83	−35·8	+47·3	7·095	+0·1	−0·5
29·85	594·85	−34·3	+46·5	·111	+0·2	+0·2
30·76	595·76	−32·4	+45·4	·124	+0·3	+0·9
Nov. 4·83	600·83	−20·1	+31·8	·200	0·0	+0·4
12·75	608·75	−0·8	+10·7	·319	0·0	−0·6
Dec. 4·74	630·74	+30·6	−22·2	·647	−0·2	−0·6
7·75	633·75	+32·9	−22·6	·693	+0·5	+0·7
9·74	635·74	+33·0	−23·3	·722	+0·2	+0·5
28·75	654·75	−26·7	+37·9	8·007	−0·2	−0·1

[R] Rejected

the writer managed to get built about 25 years ago as an annexe to the 36-inch telescope's dome*, where the computer and electronics (not to mention sometimes the observer) could be warm and reasonably comfortable, the orbit with its 67-day period was quite well established before the end of the first

*Just to make an emergency route for a hypothetical fire engine.

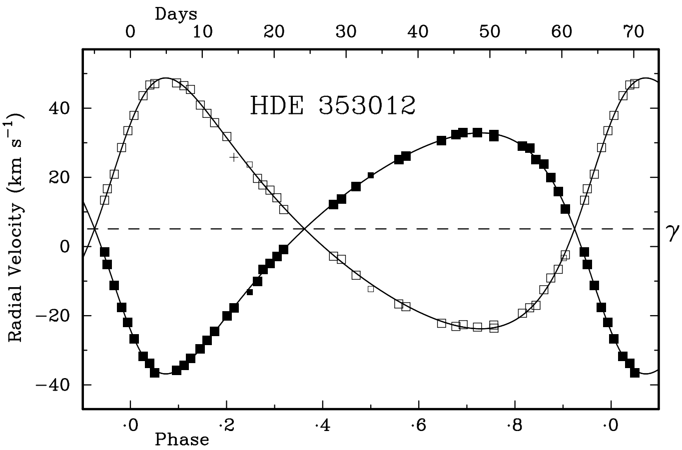

FIG. 10

As Fig. 4, but for HDE 353012. The measurement plotted with a plus was rejected from the solution of the orbit.

observing season in December of that year. Now, after a second season, there are 43 observations, all reduced as double-lined, as illustrated in Fig. 9. They are listed in Table VII and readily yield the orbit that is plotted in Fig. 10 and whose elements are set out below. In the solution of the orbit, the velocities of the secondary have been globally weighted 0·8 in comparison with those of the primary; one of them (noted in the table and distinguished in the plot) has a statistically unacceptable residual and was omitted from the calculation.

P	$=$ 66·860 ± 0·008 days	T_4	$=$ MJD 56386·87 ± 0·07
γ	$=$ +5·10 ± 0·06 km s^{-1}	$a_1 \sin i$	$=$ 30·38 ± 0·11 Gm
K_1	$=$ 34·84 ± 0·12 km s^{-1}	$a_2 \sin i$	$=$ 31·62 ± 0·12 Gm
K_2	$=$ 36·26 ± 0·14 km s^{-1}	$f(m_1)$	$=$ 0·250 ± 0·003 $M_\odot$
q	$=$ 1·041 ± 0·005 ($= m_1/m_2$)	$f(m_2)$	$=$ 0·282 ± 0·003 $M_\odot$
e	$=$ 0·3170 ± 0·0023	$m_1 \sin^3 i$	$=$ 1·086 ± 0·010 $M_\odot$
ω	$=$ 129·9 ± 0·5 degrees	$m_2 \sin^3 i$	$=$ 1·043 ± 0·009 $M_\odot$

R.m.s. residual (unit weight) $=$ 0·51 km s^{-1}

The mean equivalent widths of the dips from the primary and secondary stars in radial-velocity traces are 1·78 and 1·52 km s^{-1}, respectively. Those figures are not accurately comparable with those for the brighter stars treated above, because HDE 353012 is faint enough for the dark count from the *Coravel* photomultiplier, which is not cooled, to be a significant contributor to the counting rates at summer temperatures. The ratio, however, will not be greatly affected; it is 1:0·85, or in terms of stellar magnitudes about $0^{\mathrm{m}}\cdot17$. *If* the components of the system are assumed to be main-sequence stars, the 'rule of thumb' mentioned in the section above on HD 194765 suggests that the actual Δm_V should be very close to $0^{\mathrm{m}}\cdot20$.

The secondary star, only, seems to have an appreciable rotational velocity. An absolute majority of the radial-velocity traces yields the value zero for the rotational velocity of the primary, but only a few zeroes — not enough to suggest that any appreciable demand would exist for negative values if they were permitted — occur for the secondary, for whose rotation the mean value is $4{\cdot}5 \pm 0{\cdot}5$ km s^{-1}. It is hard to see any significance in the value for either star.

HD 215622

This star, initially observed by mistake for HD 215621, as related in the *Introduction*, was re-observed first one and then five years later on the general basis that a star once observed might as well be looked at again — and the third measurement was in serious disagreement with the first two! So the star was kept on the observing programme, and the 48 radial-velocity measurements listed in Table VIII have been made of it. *Simbad* knows of *no* papers referring to it. The only other information available is that it has a proper motion of nearly $0''{\cdot}1$ per annum, large enough to suggest that the star is a dwarf, and it has magnitudes from *Tycho 2*, $V = 8^{m}{\cdot}96$, $(B - V) = 0^{m}{\cdot}94$. The orbit follows readily from the data of Table VIII; it is plotted in Fig. 11 and its elements are presented here:

$P = 21{\cdot}2069 \pm 0{\cdot}0004$ days	$(T)_6 = $ MJD $55132{\cdot}03 \pm 0{\cdot}04$
$\gamma = -11{\cdot}29 \pm 0{\cdot}04$ km s^{-1}	$a_1 \sin i = 2{\cdot}814 \pm 0{\cdot}020$ Gm
$K = 10{\cdot}40 \pm 0{\cdot}07$ km s^{-1}	$f(m) = 0{\cdot}00198 \pm 0{\cdot}00004\ M_{\odot}$
$e = 0{\cdot}373 \pm 0{\cdot}005$	
$\omega = 359{\cdot}6 \pm 0{\cdot}9$ degrees	R.m.s. residual (wt. 1) $= 0{\cdot}27$ km s^{-1}

It is a coincidence that the longitude of periastron is, as nearly as can be told, zero, and so the radial-velocity curve sits symmetrically within the box outlining Fig. 11. The only comment that can be made on the orbital elements is that the mass function is very small. If the mass of the primary star is estimated at $0{\cdot}8\ M_{\odot}$ (more in line with the colour index than the actual *HD* classification of G0), then the secondary star is not obliged to have a mass greater than $0{\cdot}12\ M_{\odot}$ — far down the M-dwarf sequence — so it is far from surprising that

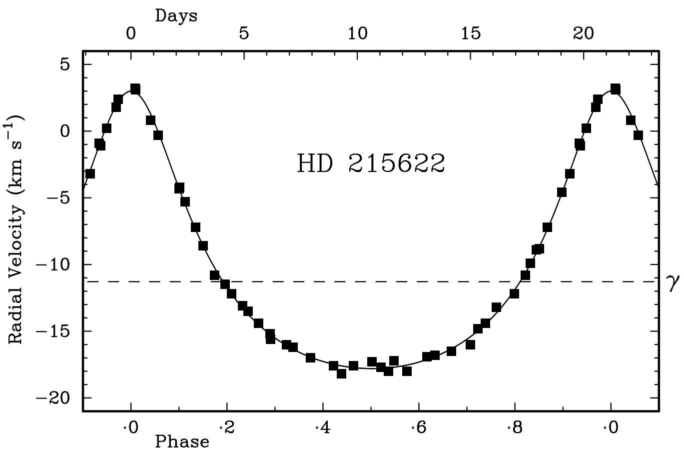

FIG. 11

As Fig. 1, but for HD 215622.

TABLE VIII

Cambridge radial-velocity observations of HD 215622

Date (UT)	*MJD*	*Velocity* $km\ s^{-1}$	*Phase*	*(O – C)* $km\ s^{-1}$
2002 Oct. 3·95	52550·95	−15·6	0·291	−0·3
2003 Oct. 27·94	52939·94	−16·8	18·634	+0·1
2007 Aug. 10·99	54322·99	−8·8	83·850	−0·1
Sept. 8·06	351·06	−10·8	85·174	−0·6
30·02	373·02	−12·2	86·210	0·0
Oct. 13·97	386·97	−7·2	·867	+0·2
17·99	390·99	−0·3	87·057	−0·2
18·94	391·94	−4·2	·102	+0·3
19·96	392·96	−8·6	·150	0·0
20·94	393·94	−11·5	·196	0·0
21·96	394·96	−13·5	·244	+0·2
23·95	396·95	−16·2	·338	+0·2
31·79	404·79	−16·0	·708	−0·6
Nov. 1·92	405·92	−13·2	·761	+0·5
14·92	418·92	−17·0	88·374	−0·1
15·93	419·93	−17·6	·422	−0·1
16·82	420·82	−17·6	·464	+0·1
23·92	427·92	−12·2	·798	−0·2
2008 Jan. 7·80	54472·80	−3·2	90·915	−0·2
July 30·09	677·09	−17·2	100·548	+0·5
Aug. 11·08	689·08	−5·3	101·113	+0·2
30·07	708·07	+3·2	102·009	+0·3
Sept. 19·01	728·01	+0·2	·949	−0·1
Oct. 21·94	760·94	−17·3	104·502	+0·5
2009 Jan. 24·76	54855·76	+2·4	108·973	+0·2
Sept. 10·06	55084·06	−14·4	119·738	+0·1
Oct. 12·96	116·96	−15·2	121·290	0·0
2010 Aug. 11·09	55419·09	−18·0	135·536	−0·3
Oct. 19·99	488·99	−9·9	138·832	+0·1
Nov. 15·87	515·87	−4·3	140·100	0·0
25·95	525·95	−18·0	·575	−0·5
26·83	526·83	−16·9	·617	+0·2
27·91	527·91	−16·5	·668	−0·2
2011 Sept. 24·08	55828·08	−10·8	154·822	−0·2
28·06	832·06	+3·1	155·010	+0·2
Oct. 19·94	853·94	+0·8	156·041	−0·5
2012 Jan. 14·75	55940·75	−7·2	160·135	+0·2
31·75	957·75	−1·1	·936	−0·3
Aug. 5·09	56144·09	−14·8	169·723	+0·2
Sept. 6·10	176·10	−13·1	171·233	+0·2
19·07	189·07	−8·9	·844	+0·3
2013 Oct. 6·92	56571·92	−4·6	189·897	+0·1
15·98	580·98	−16·0	190·325	+0·1
28·91	593·91	−0·9	·934	+0·1
Nov. 4·94	600·94	−14·4	191·266	+0·1
19·85	615·85	+1·8	·969	−0·1
29·81	625·81	−18·2	192·438	−0·6
Dec. 22·76	648·76	−17·7	193·521	+0·1
2014 Jan. 24·75	56681·75	−2·5	195·076	−0·6

no evidence of it has been seen in the radial-velocity traces. The traces yield a mean $v \sin i$ of 2·15 ± 0·28 km s^{-1} for HD 215622, but (as noted in other cases above) the neglect of other factors that can slightly affect the line-width means that the actual value is not necessarily expected to be good to better than ±1 km s^{-1}, although in this case where the star is quite comparable with solar type it may be quite accurate. It indicates a rotation period quite close to the orbital period multiplied by $\sin i$, but there is no strong reason to expect the rotation to be synchronized (or pseudo-synchronized) for a solar-type star in a 21-day orbit, especially if the companion is of low mass.

References

(1) [Announced by] Z. Eker *et al.*, *MNRAS*, **389**, 1722, 2008.
(2) R. F. Griffin, *The Observatory*, **129**, 317, 2009 (Paper 209).
(3) R. F. Griffin & N. Filiz Ak, *Ap&SS*, **330**, 47, 2010.
(4) R. F. Griffin, *The Observatory*, **130**, 60, 2010.
(5) R. F. Griffin, *MNRAS*, **219**, 95, 1986.
(6) E. V. Kazarovets *et al.*, *IBVS*, no. 4659, 1999.
(7) N. Houk & C. Swift, *Michigan Catalogue of Two-Dimensional Spectral Types for the HD Stars, Vol. 5, Declinations –12°·0 to +5°·0* (Univ. of Michigan, Ann Arbor), 1999, p. 80.
(8) [Announced by] F. van Leeuwen, *A&A*, **474**, 653, 2007.
(9) C. W. Allen, *Astrophysical Quantities* (Athlone, London), 1973, p. 206.
(10) T. Schmidt-Kaler, in K. Schaifers & H. H. Voigt (eds.), *Landolt-Börnstein* (Springer-Verlag, Berlin), **2b**, p. 18, 1982.
(11) K. R. Lang, *Astrophysical Data: Planets & Stars* (Springer-Verlag, New York), 1992, p. 140.
(12) P. C. Keenan & C. Barnbaum, *ApJ*, **518**, 859, 1999.
(13) http://www.astrouw.edu.pl/asas/
(14) http://www.aavso.org/vsx/
(15) [Announced by] J. W. Richards *et al.*, *ApJS*, **203**, 32, 2012.
(16) P. N. Kholopov (ed.-in-chief), *General Catalogue of Variable Stars* (fourth edn.) (Nauka, Moscow), 1985, p. 22.
(17) [Announced by] A. Pickles & E. Depagne, *PASP*, **122**, 1437, 2010.
(18) P. Dubath *et al.*, *MNRAS*, **414**, 2602, 2011.
(19) K. G. Strassmeier *et al.*, *A&AS*, **142**, 275, 2000.
(20) K. G. Strassmeier *et al.*, *AN*, **333**, 663, 2012.
(21) E. E. Bassett, *The Observatory*, **98**, 122, 1978.
(22) *e.g.*, D. V. Lindley & J. C. P. Miller, *Cambridge Elementary Statistical Tables* (CUP), 1953.
(23) J. Baxendell, *MNRAS*, **9**, 37, 1848.
(24) F. Schlesinger, *Publ. Allegheny Obs.*, **3**, 23, 1913.
(25) E. Ebbighausen & O. Struve, *ApJ*, **124**, 507, 1956.
(26) M. A. Agüeros *et al.*, *ApJS*, **181**, 444, 2009.
(27) [Announced by] W. Voges *et al.*, *IAU Circ.*, no. 7432, 2000.
(28) H. L. Johnson & W. W. Morgan, *ApJ*, **117**, 313, 1953.
(29) H. L. Johnson, *ApJ*, **117**, 361, 1953.
(30) A. W. J. Cousins, *Royal* [*Greenwich*] *Obs. Bull.*, no. 64, 103, 1962.
(31) O. J. Eggen, *AJ*, **68**, 483, 1963; **69**, 570, 1964.
(32) A. N. Argue, *MNRAS*, **133**, 475, 1966.
(33) J. Pfleiderer, J. Dachs & U. Haug, *Zeitschr. f. Astr.*, **64**, 116, 1966.
(34) W. S. Adams *et al.*, *ApJ*, **81**, 187, 1935.
(35) E. A. Harlan, *AJ*, **79**, 682, 1974.
(36) D. K. Duncan, *AJ*, **89**, 515, 1984.
(37) H. A. Abt, *ApJS*, **59**, 95, 1985; *ApJ*, **331**, 922, 1988.
(38) J. South, *Phil. Trans. R. Soc. for 1826*, p. 1 (see p. 255).
(39) S. W. Burnham, *General Catalogue of Double Stars Within 121° of the North Pole* (Carnegie Institution of Washington, Washington, D.C.), 1906, part II, p. 892.
(40) R. G. Aitken, *New General Catalogue of Double Stars Within 120° of the North Pole* (Carnegie Institution of Washington, Washington, D.C.), 1932, **2**, p. 1210.

(41) W. H. Christie & O. C. Wilson, *ApJ*, **88**, 34, 1938.
(42) H. A. Abt, *ApJS*, **26**, 365, 1973.
(43) O. Struve & V. Zebergs, *AJ*, **64**, 219, 1959.
(44) [Announced by] B. Nordström *et al.*, *A&A*, **418**, 989, 2004.
(45) J.-L. Halbwachs, M. Mayor & S. Udry, *MNRAS*, **422**, 14, 2012.
(46) R. F. Griffin & A. Stroe, *JA&A*, **33**, 245, 2012.
(47) R. F. Griffin, *JA&A*, **3**, 383, 1982.
(48) J. E. Gunn *et al.*, *AJ*, **96**, 198, 1988.
(49) R. F. Griffin & A. A. Suchkov, *ApJS*, **147**, 103, 2003.
(50) V. V. Makarov *et al.*, *IBVS*, no. 4118. 1994.
(51) [Announced by] J. E. Morrison *et al.*, *AJ*, **121**, 1752, 2001.
(52) *The Hipparcos and Tycho Catalogues* (ESA SP–1200) (ESA, Noordwijk), 1997, **9**, p. 2060 (no. 102545); **12**, p. C143.
(53) J. Schirmer, *BAV Rundbrief*, **51**, 235, 2002.
(54) J. M. Gomez-Forrellad *et al.*, *Ap&SS*, **283**, 297, 2003.
(55) http//:astrogea.org/var/nn-del.htm
(56) B. Hassforther, *BAV Rundbrief*, **52**, 115, 2003.
(57) B. Hassforther, *BAV Rundbrief*, **52**, 165, 2003.
(58) J. Schirmer, *BAV Rundbrief*, **53**, 52, 2004.
(59) R. F. Griffin, *The Observatory*, **121**, 315, 2001 (Paper 160).
(60) J. Andersen, *A&A Review*, **3**, 91, 1991 (see Fig. 2).
(61) P. Hut, *A&A*, **99**, 126, 1981.
(62) [Announced by] V. V. Nesterov *et al.*, *A&AS*, **110**, 370, 1995.
(63) A. J. Cannon & M. W. Mayall, *HA*, **112**, 1949 (see Chart 262, p. 282).
(64) A. J. Cannon & E. C. Pickering, *HA*, **91**–**99**, 1918–1924.
(65) A. J. Cannon, *HA*, **100**, 1934; **105**, 1937.

CORRESPONDENCE

To the Editors of 'The Observatory'

A Classic Problem Concluded

In Martin Beech's excellent review[1] of the history of the gravity tunnel, I was surprised that the following piece of Victorian fiction was not mentioned:

"They run their railway-trains without any engines — nothing is needed but machinery to *stop* them with. Is *that* wonderful enough, Miladi?"

"But where does the *force* come from?" I ventured to ask.

Mein Herr turned quickly round, to look at the new speaker. Then he took off his spectacles, and polished them, and looked at me again, in evident bewilderment. I could see he was thinking — as indeed *I* was also — that we *must* have met before.

"They use the force of *gravity*," he said. "It is a force known also in *your* country, I believe?"

"But that would need a railway going *down-hill*," the Earl remarked. "You ca'n't [*sic*] have *all* your railways going down-hill?"

"They *all* do", said Mein Herr.

"Not from *both* ends?"

"From *both* ends."

"Then I give it up!" said the Earl.

"Can you explain the process?" said Lady Muriel. "Without using that language, that I ca'n't [*sic*] speak fluently?"

"Easily," said Mein Herr. "Each railway is in a long tunnel, perfectly straight: so of course the *middle* of it is nearer the centre of the globe than the two ends: so every train runs half-way *down*-hill, and that gives it force enough to run the *other* half *up*-hill."

"Thank you. I understand that perfectly," said Lady Muriel. "But the velocity, in the *middle* of the tunnel, must be something *fearful*!"

Like many other contributions mentioned in Beech's review, this was published[2] under a pseudonym, that of Charles Lutwidge Dodgson.

Yours faithfully,
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References

(1) M. Beech, *The Observatory*, **133**, 311, 2013.
(2) L. Carroll, *Sylvie and Bruno Concluded* (Macmillan and Co., London), 1893, Chapter VII.

REVIEWS

Architecture, Astronomy and Sacred Landscapes in Ancient Egypt, by G. Magli (Cambridge University Press), 2013. Pp. 272, 26 × 18 cm. Price £60/$99 (hardbound; ISBN 978 1 107 03208 8).

The most useful sentence for general readers of this book (of whom there will perhaps not be very many) is in italics on p. 157: "There is no esoteric knowledge hidden in the sacred landscape of the Old Kingdom." Early acceptance of that would have saved many hours for many people, who could have gone on to become readers of Tarot cards and tea leaves. The Old Kingdom, during which three famous pyramids and many lesser ones were constructed, extended, roughly, 2630–2150 BC. Those pyramids, their associated temple complexes, and other monumental Egyptian architecture, dating from the Middle (2134–1783 BC) and New (1550–1070 BC) Kingdoms are the focus of Magli's volume.

The author holds both a PhD in astrophysics and a professorship of Civil Architecture at the Politecnico di Milano. His interest in archaeo-astronomy is evidenced by more than a dozen earlier publications, starting in 2003, cited in this volume. His search for "ground truth" appears in the 50 of 51 photographs that are his own. Magli has also provided his own translations for most of the names of the pyramids he discusses ("the horizon of Khufu" — that's the big one — "the Ba of Sahura shines"; "the life of Pepi is enduring" — indeed it was: he reigned 95 years; "Senwosret beholds the two lands"; and so forth). The photographs are largely recent, and some of the temples, hypostyle halls, and other monuments have decayed or been restored since the images found in older books were taken.

The astronomical connections are several: two (and only two) solar eclipses that the author thinks were significant architecturally; alignments along the horizon and in the sky of various buildings, pieces of buildings, directions between buildings, and shafts within them; and the rising of Sirius along with the Sun. The heliacal rising of Sirius is perhaps the best known and in Pharaonic times predicted the beginning of the Nile's annual flood. Precession of the equinoxes has since moved the date from early July to early August.

Of the two eclipses, one (2471 BC April 1) perhaps led Shepsekaf to locate his late-fourth-dynasty tomb at Buto, where the eclipse was total. Shepsekaf is not one of the five best-known Pharoahs. Akhnaten is, and the other eclipse (1338 BC May 14, total at 2:41 pm at Amarna) possibly persuaded Akhnaten to locate his new capital there, or, more likely, on chronological grounds, strengthened the determination of his successors to abandon the site and return to Thebes. The time of the eclipse can be calculated very precisely; the dates of Akhnaten's reign somewhat less so.

Alignments include cardinal directions, intercardinals, rising and setting of the Sun (occasionally the Moon and a few stars) on significant dates, and the north celestial pole and upper culminations of Sirius, Kochab, Thuban, and Mintaka (or Al Nilam). This leads us to my 'good book' test and conflict of interest. Magli cites my 1964 paper on 'Astronomical Investigations Concerning the So-Called Air Shafts of Cheops Pyramid'. He also cites the companion paper, 'The Stellar Destiny of Pharoah and the So-called Air Shafts in Cheops Pyramid' by my Egyptological mentor, Alexandre Mikhail Badawy (1913–1986), and no other earlier papers for the idea, which he endorses, that those openings were intended to guide the pharoah's soul to the sky, where said soul would join the stars. His choice of Al Nilam rather than Mintaka to the south arises from slight disagreement about when the pyramid was built. The author also cites Brad Schaefer on astronomical issues and Miriam Lichtheim, my hieroglyphs mentor, on literary ones, though he disagrees with her on the primary meaning of Ma'at. He emphasizes "order", where Lichtheim (1914–2004) focussed on the commoner dictionary meaning, "truth". Yes, I own a dictionary of Middle Egyptian and still know how to look up words in it. No rule of the form "bird before lion except after snake" obtains, because the Egyptians, like users of Hebrew, generally did not write their vowels.

Lots of other archaeological items could be mentioned, but the one that surprised me most is the author's complete neglect of a 1974 book by low-temperature physicist Kurt Mendelssohn which suggested that a ruined pyramid at Meidum actually fell down, being too steep. A number of consequences about timing and economic significance of pyramid building follow. I hope Magli will benefit from the recent mild increase in Egyptomania! — VIRGINIA TRIMBLE.

Reflections on the Astronomy of Glasgow, by D. Clarke (Edinburgh University Press), 2013. Pp. 373, 23·5 × 15·5 cm. Price £90 (hardbound; ISBN 978 0 7486 7889 1), £35 (paperback; ISBN 978 0 7486 7890 7).

Scotland takes pride in the fact that for several centuries it had twice as many universities as England, and the early foundation of those institutions, along continental lines, made it natural that astronomy should be one of the important subjects in the syllabus. When the University of Glasgow was founded in 1451, the Papal Bull decreed that it should be modelled on the University of Bologna, with a student following a basic Arts syllabus comprising the verbal arts (grammar, rhetoric, and logic) and the numerate arts (mathematics, geometry, astronomy, and music), before progressing to professional training in Divinity, Law, or Medicine. Early teaching was pre-Copernican, based on the ideas of Aristotle and Ptolemy as described in the 13th-Century writings of Sacrobosco. David Clarke starts his book right back at this beginning and traces the development of teaching, observing, and research at Glasgow through more than five centuries up to the present day.

This is a vast canvas, with many tales of success and failure, some of which David had picked up in passing during his forty-odd years on the astronomy faculty; those triggered him in retirement to delve more deeply into the university archives and beyond and to produce this fascinating and fully-referenced account of what he found. The book is also plentifully illustrated, with a nice blend of historical documents, maps, and photographs, supplemented by the author's own photographs. It provides much more detail the material on Glasgow in Roger Hutchins' book[1], and the article[2] by his late colleague Archie Roy (slightly surprisingly, that article is not cited in the book).

Although astronomy was in the Glasgow curriculum from the earliest days, and the university library contains in its archives a remarkable collection of editions of Sacrobosco's works, there seems to be no surviving evidence of who taught it in the earliest days. However, when James Melville was appointed Principal in 1577, he extended the syllabus, and appointed a number of Regents, one of whom had particular responsibility for 'natural philosophy', which then included astronomy. Melville himself taught astronomy and astrology (*sic*), but not, it seems, for long. The first notable Regent was George Sinclair, Professor of Philosophy (1654–62) and later Professor of Mathematics and Experimental Philosophy (1691–6). He was a man of many interests, who was involved in the use of a diving-bell to retrieve items from a sunken ship from the Spanish Armada, and also measured the heights of mountains by means of a mercury barometer. Although he was also controversially (and apparently correctly) accused of plagiarism in several of his many books, he does seem to have been a good practical astronomer, observing the positions of planets and the Moon, the Great Comet of 1680, and writing on the principles of navigation.

Between Sinclair's two appointments, astronomy continued to be taught, and course notes from that time are preserved in the University's archives; they are still based on a Ptolemaic model, although the Copernican system is briefly mentioned. That syllabus continued into the 18th Century, with the addition of practical work (by 1693, the university possessed two telescopes). Things began to improve with the appointment of Robert Dick Snr. to the Chair of Natural Philosophy in 1727. Even before then, the store of instruments had increased, and a demonstrator was appointed in 1730 to look after them; Dick appears to have used the telescope regularly, and made it available for the public to view the Great Comet of 1744. Robert Dick Jnr. succeeded his father in 1751, further extended the instrument collection, and proposed the building of a university observatory. That was approved in 1754, provided public money could be raised, but nothing happened until 1756, when the University was fortuitously bequeathed the entire instrument collection of a wealthy Jamaican merchant, Alexander Macfarlane, who had also been an active amateur astronomer, and had been educated in Glasgow in the senior Dick's time. Interestingly, one of James Watt's first jobs was to clean and repair the instruments when they arrived in Glasgow; he subsequently set up an instrument workshop within the Old College premises, where he conducted his first experiments on steam engines. The University paid for that as part of its funding of the first observatory, the Macfarlane Observatory, close to the Old College in what is now the centre of Glasgow.

While the Observatory was being built, there were political machinations, involving the Duke of Argyll, over the replacement for Robert Dick Jnr., who died in 1757. Finally in 1760 Alexander Wilson was appointed as the first Regius Professor of Practical Astronomy (the word 'Practical' was dropped in 1893), and as Observer in the College (then a separate post, involving observing duties at the Observatory). The Observatory survived until 1856, by which time it was dilapidated and city-centre building developments had made observing impossible.

Wilson is probably the name that most people associate with Glasgow, because of his discovery of the apparent depression of the umbrae of sunspots. However, he had many other interests, and for many years ran a successful printing press. He also invented and manufactured specific-gravity beads, used in the whisky industry, and did a lot of work on thermometers. In astronomy, he developed better instrumentation, observed the transits of Venus in 1761 and 1769, and speculated on gravity; he was well regarded by William Herschel, who used Wilson's thermometers in his discovery of infrared radiation. In 1784 he resigned and was followed by his son Patrick, who resigned in 1799 to pursue other interests; he had not carried out much work in astronomy, but maintained and investigated meteorological records, laying some of the foundations for the later understanding of hoar frost and the dew point.

The next event of note was the building of the Garnethill Observatory, which was initially opened in 1810 by the Glasgow Society for Promoting Astronomical Science, unrelated to the University. Sadly, that site was not far enough from the centre of the rapidly-growing city for satisfactory observing, and the finances were not stable; the Observatory and its instruments were sold off in 1824 (one of the telescopes found its way to the Cape of Good Hope). Some years later, another society began to develop new plans for an observatory, this time to be linked to the University. That plan ultimately succeeded, because it was strongly supported by the new professor of astronomy, John Pringle Nichol, appointed in 1836. The new Horselethill (or Dowanhill) Observatory was built much further out, beyond the then city bounds, near the current site of the University (which moved from the city centre in 1870). Nichol equipped the new Observatory very well, and lived there, but made little use of the instruments. He spent most of his time in lecturing and writing, both of which he did very well, influencing such figures as Edgar Allan Poe and Thomas De Quincey, and impressing his colleague Lord Kelvin. He did, however, set up a meteorological station which was maintained for the next 100 years, and he saw that the results were regularly published.

After his death in 1859, Nichol was succeeded by Robert Grant, a very different character, who already held the RAS's Gold Medal. Although he also taught and wrote, he not only lived at the Observatory but actually used it.

One of his first concerns was to undertake one of the original purposes of the Observatory, to provide accurate time for mariners on the Clyde, and Clarke has a fascinating chapter devoted entirely to the battles Grant fought to be recognized as the primary provider of time for Glasgow. He fell out with Piazzi Smyth, then Astronomer Royal for Scotland, who felt that it was *his* role to provide time, but Grant's persistence won through and time was provided for many years by electric cable to public clocks throughout the city. As well as continuing the meteorological observations started by Nichol, Grant used the transit telescope (used also for the time determinations) to compile, almost single-handedly, two catalogues of accurate positions for some 8500 stars. In addition, he purchased a new 9-inch refractor, which was still in working order in the late 1950s, and used it for double-star work.

Ludwig Becker was appointed to the Regius Chair in 1893, a chair he held for 42 years, and his first task was to renovate the instruments. He was then able to continue Grant's time service and to undertake other positional observations for some ten years, until observing conditions deteriorated too much. He also used a 20-inch (metal-mirror) reflector, with a spectrograph, observing in particular the spectrum of Nova Persei 1901, and the spectra of the major planets; he was also an active and effective teacher. He was assisted throughout by an able assistant, James Connell, first appointed by Grant, who also helped with teaching (and ran the Observatory during World War I, while Becker was interned as of German origin). Becker recognized early on that observing conditions in a big city were no longer suitable for serious research and urged the University to consider a small teaching observatory on the university site and a research observatory well outside the city. That plea fell on deaf ears, and he turned to more theoretical research. The Horselethill Observatory site was eventually sold to the Roman Catholic Church in the late 1930s, and is now occupied by Notre Dame High School.

Soon after Becker's appointment, the British Astronomical Association (formed in 1890) set up a West of Scotland branch, which proved highly successful, and became the independent Astronomical Society of Glasgow on its 60th anniversary in 1954. That Society has had many links with the astronomers at the University, and is still very active, unlike its two 19th-Century predecessors mentioned above.

One of Becker's pupils was his eventual successor, William Smart, who was appointed in 1935 on Becker's retirement. Despite continuing the theoretical emphasis, he did persuade the University to construct a small teaching observatory in University Gardens, opened in 1939 April. Smart's appointment marked the beginning of the modern era, with a gradual appointment of additional staff in the post-war period; he was also the first astronomy professor to live in the 'Professors' Square' on the main university site rather than in the Observatory. Like Becker, he was succeeded by one of his pupils, Peter Sweet, who was appointed in 1959 when Smart retired. Under Sweet, an out-of-town observatory was finally opened in 1969 at Garscube (although it was not really outside a built-up area, the Clean Air Act had made it useable); it is still in active use for teaching and public outreach. Later, in the 1980s, a proper research observatory was established at Cochno Farm, the university field station for veterinary studies. Sweet also maintained research-active staff and the Department of Astronomy flourished in various locations until its merger with 'Natural Philosophy' in 1986 to form the current School of Physics and Astronomy, based in the Kelvin Building.

This book will appeal particularly to Glasgow graduates, like myself, who remember some of the more recent events, but it is also a considerable contribution to the general history of astronomy in the UK. I have a few regrets; for example, the level of detail is such that it is sometimes hard to see the wood for the trees: a time-line of the main events would have been useful. Nonetheless, the detail is often fascinating, and rarely irrelevant, and I can recommend the book strongly. — ROBERT CONNON SMITH.

References

(1) R. Hutchins, *British University Observatories 1772–1939*, (Ashgate Publishing, Farnham), 2008.
(2) A. E. Roy, *Vistas in Astronomy*, **36**, 389, 1993.

Celestial Sleuth, by D. W. Olson (Springer, Heidelberg), 2013. Pp. 355, 24 × 17 cm. Price £35·99/$39·99/€42·79 (paperback; ISBN 978 1 4614 8402 8).

In these days of Big Science, it is refreshing to step back and read how simple applications of astronomy can add to our appreciation of art works and understanding of historical events. For over twenty years, Donald Olson has contributed articles to *Sky & Telescope* describing such connections arising from a course, 'Astronomy in art, history and literature', he taught at Texas State. This book revisits some of those studies for a wider readership and adds some more. The investigations are based on astronomical calculations — mostly phases of the Moon, sometimes daylight, twilight, positions of planets and stars, often state of the tides — and written records from a variety of sources. Also there were field trips to make measurements at the sites.

Amongst the art works studied is Turner's depiction of the night sky in his watercolour *Messieurs les voyageurs on their return from Italy, (par la diligence) in a Snow Drift upon Mount Tarrar — 22nd of January, 1829* exhibited in the summer of the same year. It depicts an incident on his way back to England from Rome, when his coach (diligence) slid off the road into a snow bank. The passengers waited for three hours in the snow, warming themselves by a fire until the coach could be righted. In the watercolour recording this, Turner includes the Moon, and some stars or planets. Olson shows that the image of the Moon was consistent with its expected phase and that Saturn, Castor and Pollux were in the correct relative positions for the date — a testament to Turner's visual memory (or did he make a sketch?) of what must have been a trying wait. Olson describes how he identified the place of the accident from the picture and confirmed it by visiting the site.

The historical events covered range from the Battle of Marathon to World War II, and include many in American history. Many of them require knowledge of the tides and tidal streams for illumination. In their investigation of the controversy over the exact date and place of Julius Caesar's landing in Britain in 55 BC August, Olson's group rented a boat and allowed it to drift off the coast of Dover to measure for themselves the tidal drift at a time chosen to match the original tidal conditions in the days around the landing.

The literary examples range from Omar Khayyam to James Joyce, taking in Chaucer's *Franklin's Tale* and the exceptionally high tide near perigee and perihelion in 1340 December which would have covered the menacing rocks off the coast of Brittany which so worried Dorigen, and a range of other phenomena. Some of the astronomical connections are a little slight, but there is plenty of interest.

The book is clearly set out, engagingly written, well referenced, and copiously illustrated. Art works are compared with recent and vintage photographs from the same view points. There is plenty of bibliography for the interested reader to follow up. One does not have to be an astronomer to enjoy it and I recommend it warmly — an illuminating read and also a good present for a non-astronomer. — P. M. WILLIAMS.

The New Martians: A Scientific Novel, by N. Kanas (Springer, Heidelberg), 2014. Pp. 123, 23·5 × 15·5 cm. Price £15/$19·99/€21·39 (paperback; ISBN 978 3 319 00974 2).

This little book is the first offering of the new 'Science and Fiction' series published by Springer, in which topical scientific ideas are explored by means of short science-fiction stories. In this case the story explores a number of scientific issues related to the human exploration of Mars, and especially psychological issues arising from the confinement of an international human crew in a small spacecraft for many months. As an emeritus professor of psychiatry who has worked on a number of NASA-funded programmes in the field of space psychology, the author is certainly well-qualified to address this theme.

Because it is a work of fiction it is not possible to review it properly without giving away the story, so I won't attempt to do that here. Suffice it to say that I thought the plot was fairly well constructed and that I quite enjoyed reading it. My only slight gripe is that I found the dialogue between the characters to be rather stilted, which I imagine reflects the author's inexperience at writing fiction (not that I could do any better). The psychological issues likely to arise in long-duration spaceflight were developed well enough. In addition to psychology, the story also addressed some important issues relating to systems engineering and planetary protection. In the latter context, the final twist at the end of the story is thought-provoking (and not a little frightening!).

The geopolitical background to the international Mars mission described is also quite plausible. This envisages increasingly ambitious Chinese space activities stimulating renewed interest in the US, Europe, and Japan, followed by a recognition that the most ambitious activities, such as human Mars missions, can only be pursued collaboratively. Here I felt that a reference to the work of the International Space Exploration Group (see http://www.globalspaceexploration.org/) would have been appropriate, because one can see how something like Kanas' proposed 'World Space Council' might develop from it.

The fictional story is followed by an appendix in which the science behind the issues that have been explored is explained, with a fairly comprehensive list of references to the appropriate literature. This includes interesting, albeit brief, discussions of actual experimental analogues of deep-space missions, such as *Biosphere 2* and *Mars 500*. The references provide a useful introduction to the literature for readers who may want to explore the issues further.

In summary, I think that the book is successful in illustrating the psychological stresses that may be expected to develop among human crews participating in long-duration space missions, and in highlighting the importance of taking planetary-protection protocols seriously. Anyone with an interest in the future of space exploration is likely to find it a thought-provoking read, albeit not a very taxing one. I certainly look forward to forthcoming titles in this innovative series, which, if the list of the topics to be covered is any guide, promises much food for thought. — IAN CRAWFORD.

Sky Alert! When Satellites Fail, by L. Johnson (Springer, New York), 2013. Pp. 199, 24 × 17 cm. Price £16·99/$29·95/€32·09 (paperback; ISBN 978 1 4614 1829 0).

If you want to know what satellites do for us, ask what happens if (or rather when) they fail, either individually or *en masse*. This is what author Les Johnson of Madison, Alabama, has done in *Sky Alert!* The astronomical content is minor and not surprising, addressing *HST*, *Chandra*, *Hinode*, *Mars Opportunity*, *Cassini*, and a few others in terms of what we have learned from them. The Earth-sciences items are likely to be less familiar: satellites help monitor ocean salinity, precipitation, clouds, and a wide variety of agricultural and other resources. Yes, weather forecasting is also better than it used to be.

The surprises come from the everyday applications, The Global Positioning System (GPS) is essential not just for lost souls but for folks who know where they are and merely want to pay for their groceries with a credit or debit card. The author envisions with horror an all-cash economy, but of course the ATMs would also go down without location and timing information from satellite communication systems. Military applications are described in such general terms (navigating, imaging, communicating) that even I can be trusted with the information.

What are the main risks? Orbital debris (particularly in low Earth and geosynchronous orbits) for physical systems, and solar storms for electrical ones. *The International Space Station* is forced to manoeuvre about once a year to avoid bits and pieces of broken-up satellites. Some fragments are from earlier collisions, like *Iridium 33* and *Cosmos 2251* in 2009, some from deliberate destruction, like *Fengyuan-1C* in 2007. There are hundreds of dangerous bits from each of these, and Appendix C describes a few operating satellites that have fallen victim to chunks of old stuff (all before 1994).

The accidents knock out one or two satellites at a time; a serious war carried out in space could destroy whole fleets and systems. What can be done about it all? Johnson recommends ways to stop the increase of space debris, perhaps clear out some of what is there, and to harden satellites and probes against space radiation. He doesn't really mention attempting to keep the peace, but it is a toss-up whether World War III will be described as the biologists' war or the information technologists' war, in the way that WW I was the chemists' war and WW II the physicists' war. If there *is* anyone left to describe it.

One more horror: the coming Next Generation Air Transportation System (NextGen) for air-traffic control will (like satellite television, just-in-time manufacturing, modern fishing, and many other mod cons) be totally dependent on the GPS and associated communications systems. Welcome to Heathrow. Please remain calm while we attempt to carry on for the next two months.

This is, in summary, a fascinating book (though not primarily for its astronomy) from a former NASA manager, educated, somehow appropriately, at Transylvania University (the one in Kentucky). — VIRGINIA TRIMBLE.

Revealing the Heart of the Galaxy: The Milky Way and Its Black Hole, by Robert H. Sanders (Cambridge University Press), 2014. Pp. 207, 26 × 18 cm. Price £25/$39·99 (hardback; ISBN 978 1 107 03918 6).

This is a somewhat unusual book, in a good sense. It is mainly a history of the confirmation of the idea that a massive black hole exists at the centre of the Milky Way, told within the larger story of our improving knowledge of the Milky Way as a whole. However, the author is an astronomer, not a historian. Also, while the book is not a technical monograph, it is slightly more technical than most popular-astronomy books. I found both of these to be an advantage; the general story will be familiar to many readers, but the slightly more technical insider perspective adds some value. Thus, even readers familiar with the subject will probably learn something new but at the same time the book is accessible to a general readership.

Although the introduction contains a long quotation from Ovid's *Metamorphoses*, and later some usual suspects such as Thomas Wright and Immanuel Kant are mentioned, this is essentially a narrative which takes place during the last 100 years or so, a large part of this in the Netherlands; here Sanders has lived and worked for about a third of this time, at the Kapteyn Institute in Groningen. The story proper starts around the beginning of the 20th Century with Kapteyn's universe. Although Kapteyn was wrong about both the size of the Milky Way and the Sun's position within it, he initiated the programme which was to lead to the modern view of the Milky Way, with his student Jan Oort playing a large role. Due initially in part to the fact that he had no observatory (at a time when most astronomical institutes, even in similar climates, had one), Kapteyn began the Dutch traditions of international cooperation and interpretive astronomy in connection with large projects; these have now become the norm, not just in the Netherlands and not just in the study of the Milky Way.

The following chapters tell the story of the discovery of the size and shape of the Milky Way and our position within it (in which radio astronomy, another Dutch tradition, played a large role) as well as our increasing knowledge about the centre itself. These are interspersed with chapters on black holes and active galactic nuclei. The two topics are of course related. It now appears that all galaxies, including our own, contain a massive black hole at the centre; ours is on the small side, with a mass of only 4-million solar masses or so. Also, most galaxies are probably active galaxies during some period of their existence; ours is not very active at the moment, but there are signs of greater activity in the past. Recurring themes are the interplay between theory and observation (with observation usually inspiring theory, rather than *vice versa*, in the story told in this book) and discoveries made possible by new astronomical techniques, including but not limited to the opening up of new portions of the electromagnetic spectrum.

Some chapters end with a section of historical summary or philosophical reflections, both often concerned with the way in which new ideas gain acceptance in astronomy, with examples from the main narrative. These support the view of science as a self-correcting enterprise where the correct views win out in the end: the more diversity in the discussion, the more efficient the process. Dead ends nevertheless often spawned ideas which later proved useful in other contexts. (Although Sanders has been one of the main proponents of MOND (modified Newtonian dynamics) as an alternative to dark matter, there is almost no mention of this in the book, not even in the more personal and philosophical sections. I think this is a good thing — not because MOND shouldn't be mentioned, but because it would detract from the narrative. Discussing it briefly would introduce nothing new to the readers familiar with MOND and confuse those who are not. Discussing it in detail would need a whole book in itself; perhaps Sanders will write a popular-astronomy book on MOND.)

There are 79 black-and-white diagrams: portraits, pictures and schematics of astronomical objects and astrophysical processes, graphs, pictures of observatories, and spectra. These are all well done and there are about the right number. There are no footnotes, so I can neither complain about having to flip to the end-notes nor be happy that I don't have to. Seven pages of journal-style references provide more than enough background for those who want to pursue various topics in more detail. The names of the authors are usually mentioned in the text when their work is discussed, though usually not in journal-style citations (which wouldn't be appropriate in a book such as this). The index of a bit more than five pages covers both people and objects.

The book is well written and seasons the scientific narrative with about the right amount of personal commentary. My only complaint concerns the very large number of typographical errors (almost a hundred); most all of these are minor and create no confusion, but they do distract from reading the otherwise good text. I enjoyed reading the book and recommend it. — PHILLIP HELBIG.

Galaxy Mergers in an Evolving Universe (ASP Conference Series Vol. 477), edited by W.-H. Sun, K. C. Xu, N. Z. Scoville & D. B. Sanders (Astronomical Society of the Pacific, San Francisco), 2013. Pp. 312, 23·5 × 15·5 cm. Price \$77 (about £50) (hardbound; ISBN 978 1 583 81838 1).

A recent addition to the rapidly expanding ASP Conference Series, Volume 477 reports on the 'Galaxy Mergers' meeting held in Taiwan in 2011 October, which is said to have been attended by a total of 128 astronomers. It is the familiar mix of invited talks (just 8), contributed talks (43), and posters, though 28 other presentations at the meeting have apparently not been written up for inclusion. Papers cover the topics you might expect: star formation in mergers, merger rates, dust, *etc.*, largely from an observational perspective. The one paper which stands out most from the crowd, for its literary style, is surely the 'anti-review' (he reviews only his own work) by Pierre-Alain Duc, with its subtitle 'Tidal tails for dummies', wherein the 'Reader' is frequently addressed directly by the 'Presenter'/'Writer' (his capitals). Otherwise, it is fairly standard conference fare. — STEVE PHILLIPPS.

Astrophysics of the Interstellar Medium, by W. J. Maciel (Springer, New York), 2013. Pp. 258, 24 × 16 cm. Price £90/\$129/€106·95 (hardbound; ISBN 978 1 4614 3766 6).

The interstellar medium of the Milky Way and other galaxies is a fascinating area of study because of the diversity of states of matter contained in it (from solids to tenuous high-temperature plasmas) and in the variety of ways in which it may be explored (involving the whole electromagnetic spectrum). Since the interstellar medium is the site of star and planet formation and therefore influences galactic evolution, it is a major component of modern astronomy. Obviously, the subject of this book is an important one for new generations of astronomers. The book is based on a postgraduate course given at Sâo Paolo University, and covers the conventional range of topics for courses on the interstellar medium, with perhaps somewhat greater emphasis given to processes in diffuse gas than in the denser regions of interstellar space.

The topics covered are entirely traditional and the book gives exceptionally thorough and detailed mathematical treatments throughout. There are exercises for the reader at the end of each chapter. The book would be an excellent foundation for a postgraduate course, and it is clear that students who completed it successfully and are inspired by the subject would be well-equipped for research.

Each chapter lists a bibliography. Professor Maciel regards his book as an introduction to Lyman Spitzer's 1978 classic *Physical Processes in the Interstellar Medium*, a notoriously dense text, and I agree that this new book fulfils that purpose. The bibliographies are long (and of rather long-established books). Notably, these lists exclude two more recent works that may be competitors for this book: B. T. Draine's (2011) *Physics of the Interstellar and Intergalactic Medium* and A. G. G. M. Tielens' (2005) *The Physics and Chemistry of the Interstellar Medium*. Professor Maciel's book is a fine addition to the astronomical literature and should be found along with those volumes on the shelves of all astronomical libraries. — David A. Williams.

Semiconductor X-Ray Detectors, by B. G. Lowe & R. A. Sareen (CRC Press, Boca Raton), 2013. Pp. 583, 24 × 16·5 cm. Price £108 (hardbound; ISBN 978 1 4665 5400 9).

This volume is the latest addition to the series of monographs on sensors from the CRC Press. The authors — Barry Lowe and Rob Sareen — are well-known figures in the industrial X-ray-instrumentation community in the UK, having occupied senior technical and management positions in companies such as Oxford Instruments Analytical, e2v, and Gresham. Their book (monograph really is too insubstantial a term to support the weight of nearly 600 pages) comprehensively describes not only the operating principles of semiconductor-based X-ray detectors and the technical development of the various detector geometries, but also the rise and fall of the companies that produced them. Unusually for such a textbook, the pages are full of people. On page 389, for example, there is a brief biography of Ugo Fano, whose factor has perplexed generations of PhD students and been the subject, according to the authors, of "great controversy". Such detail is obviously attractive to those working directly in the field of X-ray-detector development, but what might persuade the general reader of *The Observatory* to add Lowe & Sareen to their library? One answer is that the silicon-based detector technology which has, since the 1999 launches of *Chandra* and *XMM–Newton*, revolutionized astronomical X-ray spectroscopy will, in the decade-and-a-half wait for the next great X-ray observatory, require as much ingenuity (and investment) as was expended in the passing era which these authors celebrate. Understanding that technology is now, as it has been in the past, important. — †George Fraser.

†The Editors note with sadness the death of Professor Fraser on 2014 March 19.

Modern Particle Physics, by M. Thomson (Cambridge University Press), 2013. Pp. 554, 25·5 × 19·5 cm. Price £40/$75 (hardbound; ISBN 978 1 107 03426 6).

Mark Thomson has written a gem of a book on modern particle physics, timed well to include the Higgs-boson discovery. It is a textbook aimed at advanced physics undergraduates, and students who are familiar with quantum physics at the levels of operators and special relativity will find this accessible. It is not a book on quantum field theory or renormalization, and a few results need to be taken on trust, but within those constraints the arguments are developed from the ground up as far as possible. The book sets out enough background to motivate the application of Feynman diagrams, and these are then used extensively to analyze processes across the whole range of the subject. The application coverage is admirably comprehensive, and some of the processes can perhaps be skipped on first reading. Experiment is never far away, and the derivations are taken to the point where the latest results can be understood. This strong connection with experiments shows well how they test and shape the Standard Model picture. The clarity of exposition is superb, and the text helps the reader in many ways, such as identifying when something is derived from a more fundamental viewpoint, when it is 'understood' from phenomenological models, and when there are remaining theoretical uncertainties. Good judgments are made about what a student is likely to find puzzling or simply not obvious, and suitable extra explanation is then supplied. The link to symmetry is continually made, and this helps the reader understand (within some framework) why things have to be as they are. This theme permeates the whole book, including of course in the final chapter on the Higgs mechanism, for which the preceding chapters provide excellent preparation. Only once did I think that more could be done pedagogically. The link between local gauge invariance in QED and the existence of the photon is such a beautiful piece of physics, and at this level it is readily understandable by the target readership. A longer pause and consolidation would be helpful, so the underlying driving principles are cemented in place before the additional complexities of QCD are embarked upon. However, this is a very minor criticism of an excellent and highly readable book, which is well structured with useful chapter summaries and many good student exercises with on-line solutions. It is thoroughly recommended. — ALAN HEAVENS.

Our Mathematical Universe: My Quest for the Ultimate Nature of Reality, by Max Tegmark (Allen Lane, London), 2014. Pp. 432, 23·5 × 15·5 cm. Price £25 (hardbound; ISBN 978 1 846 14476 9).

There are many popular books on astronomy* or quantum mechanics which don't even attempt to summarize the entire field (hardly possible at a meaningful level in a book of realistic length), but rather some subset thereof. Such a selection might be based on the tastes and/or expertise of the author, a limited period in history, *etc*. Another approach is to base the selection on a theme. For example, Singh[1] provides us with a history of astronomy, but essentially only the developments necessary for understanding the events which eventually led to the idea of the Big Bang are included. Similarly, Tegmark's book is a popular exposition of astronomy (Part One) as well as quantum mechanics (Part Two), leading up to his mathematical-universe hypothesis (Part Three). As the title says, this is a personal account, which means both that Parts One and Two are coloured by personal anecdotes and that Part Three essentially describes some of Tegmark's own, to some extent controversial, ideas. Tegmark is careful to distinguish between what is mainstream and what is (very) controversial as well as between his own work and the work of others. While the emphasis of the book is on a controversial idea of Tegmark, it is important to note that most of his work, some of which is also discussed in the book, is mainstream (and also that he discusses some controversial ideas due to other people).

After a short introductory chapter, the first three chapters in Part One discuss increasingly greater scales in space and the origin of its constituents before coming to an overview of modern cosmology. The fourth chapter in Part One (fifth overall) discusses some problems in conventional cosmology and the

*In this review, "astronomy" should be understood to include astrophysics and cosmology as well.

means by which inflation could solve them. Inflation is important for Tegmark's multiverse concept, and the final chapter in Part One introduces the Level I and II multiverses. Tegmark defines and consistently uses his definitions of the various levels, to use his term, of multiverses. Nevertheless, one should keep in mind that what he refers to as the Level I multiverse is simply the sum of what is both inside and outside our particle horizon[2], *i.e.*, "the universe" in more common parlance[†]. (Tegmark uses "our universe" to denote that which is more commonly described as the "observable universe", *i.e.*, that which is inside the particle horizon.) His Level II multiverse is made of several Level I multiverses (or universes in traditional terms), *i.e.*, volumes in the same physical space but forever causally disconnected from all parts of our Level I multiverse, what Alan Guth refers to as "pocket universes"[3]. These are other regions of space where inflation has also stopped and which have the same underlying physical laws as we have but where the expression of those laws (values of physical constants, *etc.*) might be different. The Level I multiverse is completely mainstream (though as noted above the terminology is not): in a big-bang universe, the size of the observable universe (Level I multiverse) starts out at zero and, at least in co-moving coordinates, continues to increase, so that more and more objects become visible with time[2,4]. The Level II multiverse follows more or less directly from the concept of eternal inflation (*e.g.*, ref. 5) so, while not proven, is nevertheless accepted by a substantial fraction of cosmologists and is not original with Tegmark.

Part Two consists of two chapters on quantum mechanics. The first is a basic introduction and the second discusses the many-worlds interpretation of quantum mechanics[6] which has become increasingly more accepted in recent years, especially owing to the fact that this makes it easier to connect quantum mechanics to other fields[7]. Tegmark's Level III multiverse consists of the many worlds in the many-worlds interpretation of quantum mechanics. As such, it is as controversial as is that interpretation, *i.e.*, not much anymore these days. Part Two is much shorter than the other two, perhaps because the multiverse aspect is an essential part of the many-worlds interpretation of quantum mechanics while the Level II multiverse and (at least the name of) the Level I multiverse, discussed in Part One, will be less familiar to most readers, while the Level IV multiverse of Part Three will be new to most. In Chapter 8, Tegmark discusses his rebuttal of Penrose's claim that quantum processes are important for consciousness. That is the first of several controversial claims which Tegmark discusses, disagreeing with some (as here) and agreeing with others. That is followed by a discussion of Tegmark's concept of quantum suicide and the related concept of quantum immortality, both of which follow from the idea of taking the many-worlds interpretation of quantum mechanics seriously (though later Tegmark discusses other reasons for not believing in either concept).

Part Three begins with a brief chapter which explains, convincingly, I think, why a detailed understanding of consciousness is not necessary for a detailed

[†] "The universe" in this more conventional terminology is that which is described by our current model of the Universe (*e.g.*, a Friedmann–Lemaître model with certain measured cosmological parameters), including both observable and unobservable parts, and whether or not more than one such entity exists; Tegmark uses "universe" to mean "observable universe". Of course, in neither case should "universe" be taken in its literal definition to mean "all that (physically) exists" (for which Tegmark uses the term "physical reality"), although one should keep in mind that the term is used in this way by some authors (which by definition rules out multiverses and hence makes the discussion of what others describe as various levels of multiverses more difficult).

understanding of physical reality, even though all that we know about the latter is filtered by the former. The next chapter presents and discusses Tegmark's Mathematical-Universe Hypothesis (MUH), which states that physical reality is not merely described by mathematics but, in a very real sense, *is* mathematics or, more precisely, a mathematical structure. Tegmark argues that this follows from his external-reality hypothesis (ERH), which simply states that there is a physical reality independent of humans. Although the book contains much information which is independent of the MUH, the book is essentially one long argument for the MUH. As such, it is difficult to do it justice in a review. However, it is not as absurd as it might at first sound and, even if one does not agree with it, the argument is worth reading.

The next two chapters discuss various issues in modern cosmology in light of the MUH. The MUH implies that time is an illusion, which is of course a concept that has been explored by others in the past. The chapter discussing this also touches on topics of current debate such as Boltzmann brains, the doomsday argument (and the related topic of reference classes), and the measure problem. The next chapter discusses the Level IV multiverse, which consists of all mathematical structures. (Note that, except for the certainly true Level I multiverse, which in some sense would be implied by the Level II multiverse, the acceptance of one level of multiverse does not imply acceptance of another; they are related but independent concepts. Tegmark also stresses that all of his multiverses are not theories but rather consequences of other theories and are in principle testable.) Tegmark discusses the question of whether we could be living in a simulation in the context of the Level IV multiverse. Apparent fine-tuning of physical constants can be seen as a weak-anthropic argument for both the Level II and Level IV multiverses. While many of the topics discussed in Part Three have been discussed elsewhere, often quite extensively, in the literature, Tegmark's multiverses lend a sense of coherence to many topics which at first sight might seem only loosely related. The final chapter discusses the future of humanity and the future of the Universe and mentions the "singularity" popularized by Vernor Vinge and Ray Kurzweil as well as the Fermi paradox, with Tegmark concluding with the minority view that we might be the only technological species in our observable universe.

The book is well written and well organized. At the end of each chapter is a summary of the ideas discussed. The introductory chapter points out which chapters are mainstream, controversial, or very controversial as well as the related question of which chapters could be skipped (except for the summary at the end of each) based on the reader's background. Suggestions for further reading, grouped by chapter and with the level of difficulty indicated by symbols, give useful suggestions for those who want a deeper understanding of the topics discussed in the book, except for the most controversial. Further information on these can be gleaned from Tegmark's own papers. He cites few of them directly, but his web pages provide context for and links to them. Several photographs and diagrams supplement the text. Some of the grey-scale figures are a bit difficult to read, but I suspect that they will be better in the proper version, as opposed to the advance-reading copy I read. For the same reason, there is no index in my copy but rather 15 place-holder pages which indicate that the proper index should be sufficient.

The book is aimed at the same readership as most popular-science books, such as those by John Barrow, though the combination of mainstream and controversial topics will probably translate to a combination of familiar and new to many readers. I have only two real complaints, and they are slight. First, even though Part Three is the longest, I would have liked many of the topics to be discussed in a bit more detail. Such discussion is of course available in Tegmark's technical papers, but these might be too technical for some and in any case it would be nice for the book to be a bit more self-contained; in some cases, I felt that some essential aspects might have been lacking. Second, the final chapter, while I have no qualms about its content, seems almost tacked on. Reference is made to what has gone before, but the thrust of the one long argument is lost somewhat. But, again, these are minor complaints. The multiverse has been mentioned by many others, but Tegmark gives a good overview of the various concepts involved. Again, the point is stressed that multiverses are not a theory, but rather testable consequences of other theories. Even if some multiverse might not be directly detectable itself, it might be justified by confidence in the theory on which it is based, much as we believe what General Relativity tells us about the interior of black holes, even though they are almost as inaccessible to us as are other parts of a multiverse. Although this is by no means proof, at least some multiverses are an extension of a trend which has been going on for quite some time: Earth is just one of many planets, our galaxy just one of many galaxies, *etc*. Readers should keep an open mind and follow the arguments closely.

In summary, I recommend the book highly. Not all readers will agree with Tegmark on all points, but any disagreement will take place on a level high enough that the journey there will have been worth it for its own sake. — PHILLIP HELBIG.

References

(1) S. Singh, *Big Bang: The most important scientific discovery of all time and why you need to know about it* (Harper Perennial, London), 2005.
(2) W. Rindler, *MNRAS*, **116**, 662, 1956.
(3) A. Guth, *The Inflationary Universe* (Perseus Book Group, New York), 1997.
(4) R. Stabell & S. Refsdal, *MNRAS*, **132**, 379, 1966.
(5) A. D. Linde, *Particle Physics and Inflationary Cosmology* (CRC Press, London), 1990.
(6) H. Everett, *The Many-Worlds Interpretation of Quantum Mechanics*, Ph.D. Thesis., Princeton University, 1957.
(7) D. Deutsch, *The Fabric of Reality* (Allen Lane, London), 1997.

Communicating Science: A National Conference on Science Education and Public Outreach (ASP Conference Series, Vol. 473), edited by J. Barnes, C. Shupla, J. G. Manning & M. G. Gibbs (Astronomical Society of the Pacific, San Francisco), 2013. Pp. 411, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381830 1).

I have to be honest with you — and you would expect nothing less — this is my third attempt at reviewing these proceedings. Thank you to *The Observatory*'s Editors for their patience. In my first attempt I complained about the terrible use of English, the deadening American management-speak of many of the papers, and the overall low intellectual level. Whilst celebrating Christmas and reflecting on the "Good Will to all People" nature of that holiday, I felt that I may have been too harsh. In a review of a conference held in America (Tucson, 2012 August) on the subject of education and public outreach in America, it was at very least churlish to complain about the proceedings being too American. So my second attempt aimed at finding the few highlights in these proceedings —

and with repeated reading I found some. Now I should also add that I think nobody reads conference proceedings the way a reviewer does — least of all, it would seem, the proceedings editors in this instance. Reviewers read from cover to cover; I suspect that attenders read only their paper and maybe a few that they remember as being interesting; non-attenders read only papers they have been directed to by references in other sources.

I should also preface this review by saying that my interest in science outreach is from an amateur point of view. Like many of us with non-teaching careers in some aspect of space science I have given a few school talks, most of which gave me a profound sense of just how difficult but also how rewarding the job of teaching science is. But as a non-professional I am not familiar with current thinking on science teaching and its wider application to public engagement with science — indeed I find the term 'outreach' somewhat patronising; as if those wise intellectual folk in their castles of scientific excellence are reaching out a helping hand to save those poor masses drowning in a sea of technology and advanced physics.

So I was very much looking forward to reading these proceedings. Science outreach is something to celebrate and a conference on the subject is to be applauded — what could be a more noble ambition that to share the joy of, and encourage the participation in, science? As implied above, my anticipation faded to a sort or resigned gloom as unreadable paper followed unreadable paper. Several things contributed to my despair including some very worrying contributions on engaging "Native Americans" and "Latinos", which although not hugely relevant in a European context are perfectly valid in these proceedings — except for the astonishingly low expectations of the target recipients of this outreach. The generally low intellectual aims of the overall conference were highlighted for me in one question and answer session, in which a speaker was asked about the (trip) hazards of lawn sprinklers during a mass astronomy event. Was that really such a stand-out question that it needed publishing? Many of the published papers discuss not the paper as it was presented but rather the paper as it was intended to be presented; the contributions then have the feel of an abstract and the promised, presumably presented, talk does not actually appear in print.

The conference and the resulting proceedings were organized in four parts: a plenary session, workshops and special sessions, oral contributions, and finally poster contributions. The plenary session consisted of just two papers — well, actually one and a half papers as the first summarizes and repeats large sections of the second. Both describe end-of-the-world cosmic-doomsday scenarios, particularly those based on aspects of the Mayan calendar. The workshop sessions were typically presented as a single summary paper covering the offerings of several contributions, some of which were also discussed by an expert panel with questions from the floor. Although this may be a fine way of sharing the experience it doesn't do much for distilling the essence from some very mixed inputs. There are paragraphs that consist of a single unanswered question; perhaps that's just the way it is with teaching. Other than the only UK contribution mentioned below, I didn't come away from these papers with anything novel or useful that could be applied to science outreach. The oral contributions start with papers that are simply extended advertisements for particular software products, from a variety of both public-sector and private-enterprise suppliers. These are followed by a raft of papers about using computers, computer networks, and blogging as tools for education. A much more interesting paper from the University of Arizona summarizes the results of a survey given each year to first-year students, which has shown that science literacy (*i.e.*, basic science knowledge) among freshman students has remained unchanged for over 22 years, despite vast amounts of outreach and presumably improving education techniques. In a damning indictment of college teaching they note that "Overall college science courses have at most a marginal impact on the science literacy and beliefs of undergraduates". Their survey having had over half-a-million respondents also enabled them to point out that among the different fields of study those students aiming to be future teachers perform the worst.

The conference organizers are to be congratulated on getting these proceedings published in just over a year. The published book is clearly aimed at the conference participants themselves as a record of the schedule of events; however, as a record of the current aims and achievements at the forefront of science education in the United States it is less successful. Outsiders, *i.e.*, non-US educators and scientists, may find themselves hampered by both the language and the overall environment and context of these proceedings. There is a lot of discussion of 'Astronomy 101' as a last opportunity to provide students with the intellectual tools for scientific thinking, some strange references to "Western Science" and its non-applicability to native Americans, paragraphs so full of acronyms that the fully-spelled-out word is an interesting oddity, and some of the least-effective conference photographs I have ever seen — the backs of heads in darkened rooms. On a positive note; the contribution by the University of Glamorgan and their 'Armageddon in a classroom' programme, which uses planetary impacts as a hook on which to hang a scientific adventure and to grab attention, was an interesting and instructive read and a shining example of what I was hoping the rest of the proceedings would be like.

I imagine that for the intended readership of US-based educators these proceedings fulfil the aim of being a published record of the event, but to anyone else they are probably less useful. — BARRY KENT.

Twenty Years of ADASS, edited by I. N. Evans (Astronomical Society of the Pacific, San Francisco), 2013. Pp. 790, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381822 0).

It may indeed be worth celebrating the fact that the Astronomical Data Analysis Software and Systems conference has been going for 20 years, but whether that justifies the publication of this "best of ADASS" volume I rather doubt.

Most of the papers were chosen by the editors of each annual volume as the best few from that year but a few highly cited papers have been added to the mix. A good many of those republished here were indeed pioneering or influential at the time, although the selection criterion is not always obvious. Unfortunately computing hardware and software have both changed so much over 20 years that earlier papers are more likely to be of interest to historians of science than to current practitioners. The originals are, however, still available on-line and can be found using search engines, so this volume appears to have little value even as a historical resource. Perhaps a few young astronomers might like to browse for amusement, or to marvel at how we coped 20 years ago, when computers were around 10 000 times less powerful than they are today. Otherwise the market for it seems to be very limited. — CLIVE PAGE.

Astronomical Data Analysis Software and Systems XXII (ASP Conference Series, Vol. 475) edited by D. N. Friedel (Astronomical Society of the Pacific, San Francisco), 2013. Pp. 421, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381834 3).

ADASS has for over 20 years been the world's principal conference for those involved in astronomical computing, although this volume is only half the size of the last few, as a result of smaller attendance. I don't know whether squeezed travel budgets were to blame, Champaign (Illinois) was insufficiently attractive, or whether other conferences are beginning to compete? Anyhow, it still provides a fairly comprehensive snapshot of astronomical computing efforts world-wide as of late 2012. Although about half the papers come from North America, it is notable that the top three software systems in terms of mentions in the text are Aladin (from Strasbourg), TOPCAT (Bristol), and SExtractor (Paris), so Europe's influence is still strong. Indeed, in equal fourth place is the Starlink Collection which was written and maintained in the UK, until our Research Councils decided to terminate funding because they could see no value in software.

Trends this year include: even more systems which interoperate using Virtual Observatory protocols, more number-crunching done on GPUs rather than CPUs, and the growing dominance of Python as the programming language. New this year is a section on astronomical applications for hand-held devices. It is interesting that four out of the five papers here are from Europe. No doubt we'll see more on mobile apps in future volumes, despite the obvious difficulties of using tiny screens and fiddly keyboards, not to mention a multiplicity of operating systems. — CLIVE PAGE.

Here and There

A SENSIBLE PRECAUTION

Before diving into black holes, you should make sure you have a solid understanding of the concepts and equations related to density and escape speed. — *A Student's Guide to the Mathematics of Astronomy* (CUP), 2013.

LIGHT ON ACCURACY

Early measurements showed that the galaxy had a mass of 1bn suns, which is 40bn to 50bn times lighter than the Milky Way. — *The Guardian*, 2013 October 24.

THE SHRINKING MOON

The cluster is 95 arc-seconds across, meaning it is three times the size of the full Moon … — *JBAA*, **123**, 372, 2013.