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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

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

O. LAHAV, *Vice-President*
in the Chair

The Vice-President. Good afternoon, I am Ofer Lahav from UCL, one of the Vice-Presidents of the RAS, and I am standing in for the President. We have quite an interesting programme this afternoon. The first talk is by the winner of the Fowler Award, one of the early-career awards of the RAS, so it is my great pleasure to introduce Vasily Belokurov from the Institute of Astronomy, Cambridge, who will tell us about 'Ultra-faint dwarfs: galaxies outside the box'.

Dr. V. Belokurov. This is a talk about dwarf spheroidal companions of the Milky Way. Why are these tiny galaxies interesting? From the spectroscopic and the photometric data gathered so far, it turns out most of the Milky Way stellar halo and the stars in its dwarf satellites formed in the first billion years after the Big Bang. Hence, by detecting and measuring the oldest stars and the satellites of our Galaxy we can explore the conditions that existed in the early Universe, at equivalent redshifts much beyond those probed by the deepest direct images. Moreover, simply by counting the dwarf spheroidal galaxies around the Milky Way we can hope to address the long-standing 'missing-satellite problem'. In the last years of the previous century, Cold Dark Matter (CDM) cosmology had emerged as the dominant theory of structure formation, capable of explaining key observational facts such as properties of the cosmic microwave background and the large-scale structure of galaxy clustering. However, locally, there came to light a gaping inconsistency between the CDM predictions and the Galactic satellite census. By nature of construction, in the CDM paradigm, the properties of sub-halo distribution are self-similar on various host-mass scales. Therefore it is natural to expect a fair match between the sub-halo mass function and the Milky Way dwarf-galaxy luminosity function, equivalent to that measured for massive galaxy clusters and their satellites. Yet, as was pointed out by several groups, the count of the Milky Way satellites was lagging behind the cumulative sub-halo mass function by several orders of magnitude. The solution to this so-called 'missing-satellite problem', appeared twofold. Either CDM over-predicts the abundance of sub-haloes on Galactic scales, and, therefore, is in need of revision, or the laws of star formation operate differently on small scales and hence require updating. It was also not possible to rule out that swarms of satellites, although present around the Galaxy, somehow avoided identification.

How easy is it to miss a Milky Way satellite? Not very, if, like the Large Magellanic Cloud, it is only forty times fainter than the Galaxy itself. However, most of the dwarfs are much fainter than that; for example, one of the brightest satellites, Leo I, is already 8000 times dimmer than the Milky Way. The closer and the fainter the dwarf galaxy is, the easier it is to resolve its milky fuzz into individual stars. If member stars are discernible then the presence of the satellite galaxy can be gauged from the detection of a stellar over-density sticking above the Galactic foreground. This is exactly the principle behind the satellite-detection algorithm that we have applied to the Sloan Digital Sky Survey (SDSS) data. SDSS is a perfect dataset to try to find such stellar overdensities as it: (*i*) covers ¼ of the sky; (*ii*) goes as deep as $i \sim 22$ mag., which implies that main-sequence stars can be seen as far as ~ 50 kpc and red giants as far as ~ 200 kpc; and (*iii*) measures stellar fluxes in five photometric bands hence providing a mechanism to estimate stars' temperature and metallicity. Our algorithm first estimates the local stellar density at a given point on the sky and then compares it to a global density background around it. Significant positive deviations are identified and followed up. Much of the subsequent classification is based on the assumptions to do with the signal and noise properties. Given the characteristics of the survey, the noise is dominated by the stellar-foreground variations, while the signal depends on the luminosity of the satellite and its heliocentric distance.

In the attempts to find new Galactic satellites we realized that the prior assumptions as to the smoothness of the foreground density field had been too optimistic. The stellar halo of the Milky Way turned out to contain multiple sub-structures in the form of tidal streams and shells, as best illustrated in the 'Field of Streams' image. To make matters worse, galaxies misidentified as stars provided additional highly clustered signals that contributed to the false positive rate. Luckily, such large-scale-structure-induced false positives can be cleaned away by applying the over-density search to the SDSS galaxy catalogue and rejecting all peaks detected with both stars and galaxies. Most amazingly, we had also over-estimated the satellite intrinsic luminosities by several orders of magnitude! The satellite hunt in the SDSS data revealed 15 new systems, each fainter than any previously known dwarf galaxy, some of them so faint they can be outshone by a single supergiant star. These satellites, dubbed Ultra-Faint Dwarfs (UFDs), are truly invisible objects: their surface brightness is so low that no trace of the galaxy can be seen in the original SDSS images. Consider Boötes I, one of the first to be identified; it is also one of the biggest and brightest of the new discoveries, but its total luminosity is 2 000 000 times lower than that of the Milky Way. Having come up with an automated way of finding the satellites, we could translate our discoveries into the predictions for the total size of the Galactic dwarf population. Because the efficiency of the satellite detection is set primarily by the surface brightness of the object, many of the moderately faint systems are missed by the SDSS at larger Galactocentric distances. Overall, the 15 systems we found meant that there are tens or perhaps even hundreds more waiting to be discovered.

The Ultra Faint Dwarfs possess unique properties. According to the early kinematic studies, their mass-to-light ratios are huge, sometimes exceeding hundreds, even thousands. This in turn would imply high dark-matter densities in their interiors. The spectroscopic studies also revealed a large number of extremely-metal-poor stars and very wide spreads in metallicities of individual member stars. These observational signatures point towards the creation scenario where many of the UFDs came into existence at the dawn of the Universal structure formation, maybe even before the re-ionization. However, these measurements have to be taken with pinch of salt, as illustrated by the 'evolution' of structural parameters of Boötes I. In the five years of detailed studies by several groups, only its size remained approximately constant, while its metallicity, metallicity dispersion, and velocity dispersion all 'evolved' significantly. These objects are complex and faint, and therefore are notoriously difficult to study, but there is yet hope to decipher the puzzle of UFDs. And if currently there is no obvious answer to the question "What is a galaxy?", I believe soon there might be.

The Vice-President. Thank you very much, Vasily, for a very interesting talk; questions?

Mr. N. Clerke. Is there any idea about the formation of these objects? Where do they come from?

Dr. Belokurov. In the formation of dwarf galaxies, these can be explained by extending this paradigm into a very-low-mass halo. These galaxies, supposedly, exist in halos of very low mass that could only have kept the gas that they have accreted for a short period of time; the gas was either blown in during the re-ionization epoch, or was blown out by the first supernovae that went off. And so they lost their gas, and only could have produced a small number of stars. So, in principle there is no mystery there; in practice, there are a lot of open questions.

The Vice-President. You've mostly focussed on the optical visibility of those objects; what do you see when you look at other wavelengths?

Dr. Belokurov. Not much is seen in other wavelengths, apart from in one system that has been detected in H I, and this is a galaxy that seems to have some amount of recent star formation. It's not really a dwarf spheroidal, but a so-called transitional object, of which we are now finding more, and again they don't fit into a predefined group. This is Leo-T, a dwarf galaxy that lives at the very outskirts of the Milky Way, and has produced, I think, one star in the last 500 million years or so. It has very little gas, but somehow it is able to be seen in the radio.

Rev. G. Barber. Is this a top-down or a bottom-up scenario? Are these dwarf galaxies older than the other galaxies; are they the basic building blocks; or are they a secondary feature, forming after the main galaxy has formed?

Dr. Belokurov. The body of evidence from observations and the simulations that have been run point to the scenario in which these galaxies have been formed first, and then accreted, with some surviving.

The Vice-President. Thank you. I see that there are many more questions but I think we should move on. Let us thank Vasily once more and offer our congratulations on the Fowler Award. [Applause.]

The next speaker is also a winner of the 2011 Fowler Award, Dr. James Wookey from the University of Bristol. The title of his talk is, 'Between a rock and a hot place: the core-mantle boundary'.

Dr. J. Wookey. The story of the core–mantle boundary region begins at the very start of Earth's history, at the time of its formation. The most likely formation scenario for the Earth has the planets forming from the solar nebula, in a violent set of energetic collisions with increasingly large objects, from dust particles through to planetesimals.

These left the Earth largely molten, allowing the separation of the iron core from the silicate mantle. This process actually happened remarkably quickly, and was largely complete only a few million years after formation. After suffering one final cosmic indignity, a collision with a Mars-sized object which caused the formation of the Moon, the Earth was largely as we see it today in forms of gross structure: a rocky, solid silicate mantle, and a liquid iron core. (The freezing of the solid inner core would come much later.)

The legacy of the Earth's violent birth was a massive amount of primordial heat, augmented by the heat generated by the decay of the radioactive elements throughout the core and mantle. Since that time the Earth has been acting to remove that heat, by convection of the core and mantle. The convection of the liquid iron in the core is rapid, and the movement of charges leads to the Earth's magnetic field: the geodynamo. The convection of the mantle is much slower — overturn takes hundreds of millions of years. This drives plate tectonics and the violent phenomena such as earthquakes, tsunamis, and volcanoes, which we associate with the dynamic Earth.

Thus, we are familiar with the consequences of Earth dynamics at the surface, but important to the whole system is the expression at the base: the core–mantle boundary (CMB) region. While this only makes up around 2% of the Earth's volume, its properties have profound consequences for both the core and the mantle. On the core side, the lowermost mantle acts as a static, heterogeneous boundary condition on the geodynamo, and may influence its long-term evolution (for example, the rate at which it reverses). For the mantle, the CMB region may influence the style of dynamics (for example, the number, size, and fixity of mantle plumes), or act as a repository for primordial or subducted material (the so-called 'slab graveyard', sequestered for all time from the broader system).

Obviously, studying a region as remote as the lowermost mantle is a challenging endeavour. The only way we have to observe it directly is by using seismology. Earthquakes send vibrations throughout the Earth — the waves traverse even the deepest parts that we are interested in. By studying the properties of those waves (how quickly they arrive and how strong they are) we can infer the structure of the Earth.

The global seismic network has been expanding rapidly over the last few decades and there are also thousands of regional stations and temporary stations. So, it is fair to say that in terms of data, global seismologists are living in an age of plenty. We have enough stations recording the earthquakes all over the globe to make cross-sectional images of the whole Earth. These show the variations in the speed at which seismic waves travel in the mantle, predominantly due to temperature heterogeneity: there are cold regions like where material is descending at subduction zones, for example, beneath the Americas, or where hot material is apparently rising, such as beneath the central Pacific.

So, what does seismology tell us about the core–mantle boundary region? Over the years we have built up observations of a menagerie of anomalous seismic features at the base of the mantle. These include the massive, hulking superplumes, the thin ultra-low-velocity zone just above the core, and hints of sub-wavelength-scale structure implying strong chemical heterogeneity. Another unusual property of the lowermost mantle is its strong seismic anisotropy, which is the variation of seismic-wave speed with direction. This is caused by a texturing of the minerals at the base of the mantle, we presume in response to a strain imposed by convective flow. Measuring this anisotropy gives us a way to constrain these otherwise invisible dynamical processes. We can use techniques like tomography to give us a first-order picture of its regional variation, something which appears to be considerable. However, in order to link the seismic picture to the dynamics, we need to know how the mantle's constituent minerals deform under the titanic pressures and temperatures at the CMB. My colleagues in experimental mineral physics are exploring such things by using a variety of methods, from recreating the conditions at the CMB between the tips of diamonds to finding analogues whose behaviour mirrors that of Earth minerals at more agreeable conditions. This experimental work is augmented and validated by advances in computational mineral physics. The availability of vast, modern computer resources have allowed the simulation of minerals at the quantum-mechanical level, yielding a new class of information which we can bring to bear on understanding the seismic observations.

We are now using new insights from mineral physics, in connection with codes which model the convection of the mantle, to make models of the flow in the deep Earth, and thus predict the seismic signatures we should see if our models are correct. Progress in this work is encouraging, but we still have a long way to go before we are confident of the details of the dynamics. For a start, considerable uncertainties affect the detail of the mineralogy (a fairly expected consequence of the challenge of simulating the core–mantle boundary), meaning that we have several competing models. Furthermore, while the geodynamic calculations are very sophisticated they still lack representation of some important processes like the effect of rheology. Finally, we also need to push our seismic observations harder. New techniques will allow a more complete measurement of the seismic anisotropy, which will make the comparison with models harder, but should prove a more discerning way of eliminating uncertainty. Ultimately, the goal is an understanding of the core–mantle boundary as complete as we have of the surface. This will enable us to answer long-standing questions about the Earth's thermal and chemical evolution, and the dynamic processes that have made it the planet we know today.

The Vice-President. Thank you for a great presentation. Any questions?

Mr. M. Hepburn. A year or so ago, we heard about one researcher's careful study of the relative position of the well-established plumes, and she concluded that you could not reconcile their coming from a definite fixed position from the bottom of the mantle, but that they moved about with respect to one another.

Dr. Wookey. Yes, the picture I presented of mantle plumes at the beginning is certainly not the last word on the nature of mantle plumes in the Earth. That's a whole extra talk, but it doesn't directly impact my results — and actually, better constraints on D'' would tell us a lot more about what is possible and what we might expect from mantle plumes.

Professor P. Murdin. According to the impactor theory, the mantle is a mixture of material from the proto-Earth and from the impacting planetoid. Can you distinguish two kinds of material in the mantle?

Dr. Wookey. No, probably not at the level that they would be different now. I guess you might expect some isotopic differences; but we know that the mantle is fairly well-mixed isotopically anyway, so differentiating where material comes from, whether it is from a giant impactor or from other processes, is an extremely hard problem.

Professor Murdin. So after the big splash, the big stir?

Dr. Wookey. Yes. Don't forget the problem is that we've had 4½ billion years of mixing, so picking those apart could certainly be a challenge.

Professor A. Fitzsimmons. Would we expect a similar feature at the core–mantle boundary of Venus?

Dr. Wookey. As in, Venus the planet? [Laughter.]

Professor Fitzsimmons. Yes!

Dr. Wookey. Hopefully we'd have spotted it if we had a Venus stuck inside the Earth! Do you mean from the features on the surface?

Professor Fitzsimmons. Absolutely! Any way to interpret the features of the surface as arising from the same kind of geodynamics that you have shown us here for the Earth.

Dr. Wookey. Venus is an interesting case, because it seems that at the moment Venus isn't really doing anything on the surface: it has what is called a stratified stable lid. Actually it is a very interesting question, why Earth has the style of plate tectonics that it does now, and why Venus doesn't. And I think by understanding better the thermal evolution of the Earth, we'll start to answer questions like why Earth is different from, say, Venus, and also from Mars.

The Vice-President. Thank you again, and congratulations again on the Fowler Award. [Applause.]

As I am sure you all know, one of the new initiatives of the RAS is the award of RAS postdoctoral fellowships. It is a new scheme, and we are very pleased to have here one of the recipients of this postdoctoral fellowship: Dr. Caitriona Jackman, from University College London, who will tell us about 'Energy transfer in magnetospheres'.

Dr. Caitriona Jackman. First of all, I would like to take this opportunity to thank the RAS for their very generous fellowship award. I am very grateful for their support of my research, which concerns energy transfer in magnetospheres.

Planetary magnetospheres are like giant magnetic bubbles that form around magnetized planets. The region inside the magnetosphere is controlled by the planet's magnetic field, and contains plasma (charged particles) that can be produced internally, for example, by moons or planetary rings. Outside of the magnetosphere, interplanetary space is filled with the solar wind, a stream of plasma constantly flowing away from the Sun and carrying with it a remnant of the Sun's magnetic field, the Interplanetary Magnetic Field (IMF).

As the solar wind blows, it shapes the magnetospheres by compressing the dipole-shaped magnetic-field lines on the dayside (the side facing the Sun), while on the nightside field lines stretch out into a long magnetic tail. Through solar-wind interaction combined with processes internal to magnetospheres, mass (plasma) and open magnetic flux builds up, tail field lines get more massive, and stretch until they snap in a process called reconnection. During reconnection oppositely directed field lines can come into close contact, break, and then merge to form new field lines. This process results in the release of huge amounts of stored-up energy and the closure of open magnetic flux that had built up in the magnetotail. My research involves observing this explosive release by using orbiting spacecraft and also working to understand how the aurora can be used as a remote proxy for changes in the magnetic-flux content of the magnetosphere.

Most of my work to date has concerned Saturn's magnetosphere, and this is owing to the wealth of data that we have available from the highly successful *Cassini* mission, in orbit around Saturn since 2004. By using data from the *Cassini* magnetometer instrument, we can observe reconnection as it is going on in the magnetotail. As the spacecraft flies through regions where the field is moving from a stretched configuration to a more dipolar configuration following magnetic reconnection, we see deflections in the north/south field direction. We can ascertain where the spacecraft is in relation to the point where reconnection started (the 'X' point). Planetward of this X-point, field lines which were stretched regain their dipolar shape and are rapidly accelerated back towards the planet. On the other side of the X-point, the ends of the stretched field lines are broken off to form disconnected blobs of plasma called 'plasmoids'. These plasmoids are no longer attached to the planet and are free to escape down the magnetotail and leave the magnetosphere.

I have used *Cassini* magnetometer data to uncover many of these plasmoid structures in Saturn's magnetotail, and I am currently working to decipher the morphology of plasmoids and to understand how they evolve as they move tailward, achieving pressure balance with their surroundings. In this presentation I show examples of how we can use data from a single spacecraft flying through a plasmoid structure to calculate how much magnetic flux is closed in any given reconnection event (*i.e.*, how much energy is lost through reconnection).

As a complement to *in-situ* magnetometer measurements, remote observations of planetary aurorae can provide direction information about the flux content of the magnetosphere. In general, the main auroral emission takes the form of an oval, inside of which is a dark region called the 'polar cap'. The processes which lead to the main auroral-oval formation at Earth and Saturn are similar, and in these cases the area of the polar cap corresponds to the amount of open flux contained in the magnetosphere. Thus changes in this area (expansion/contraction of the main oval emission) tell us how much open flux is being added to or removed from the magnetosphere through reconnection.

There are also smaller-scale auroral features related to specific reconnection events. Following reconnection, the rapid motion of dipolarized field lines back towards the planet sets up a system of field-aligned currents which maps into the region close to where the main auroral oval forms. This results in the appearance of discrete spots close to the main oval, corresponding to individual reconnection events, and these spots have so far been observed at Earth, Jupiter, and Saturn.

While studying any planetary magnetosphere in our Solar System is without doubt a fascinating task in itself, it is even more rewarding to compare and contrast the character of reconnection events in different magnetospheres. Factors such as the proximity of the planet to the Sun (the solar-wind influence), and the strength of the planetary field (the size of the magnetospheric cavity) can have a huge influence on the time-scales and magnitudes of reconnection events. Applying the same physics in vastly different environments can have dramatically different results. As such, an important new avenue for my research is comparison of reconnection processes at the gas-giant planets Jupiter and Saturn with the case of Mercury, a 'mini-magnetosphere', where reconnection can happen over timescales of order one minute, compared to an hour or more at the larger planets.

The future for magnetospheric science within our Solar System is a bright one. The NASA *MESSENGER* mission arrived at Mercury in 2011 March and it will orbit for at least one year. Meanwhile *Cassini* will continue to sample Saturn's environment until 2017, and Jupiter awaits the arrival of the *Juno* mission. And of course we have a plethora of Earth-orbiting spacecraft which relay information about the terrestrial magnetosphere. The processes which cause reconnection and its global effects remain an intriguing puzzle, and I look forward to continuing my research in this area with the support of the Royal Astronomical Society.

The Vice-President. Thank you very much for a very nice presentation. You mentioned the extension of the *Cassini* mission to 2017: what is the expectation for new science there?

Dr. Jackman. In terms of the deep tail, not that much, because I think the furthest that the spacecraft is going to go out on the night-side is about 30

Saturn radii. This is probably inside of where tail reconnection happens (at the X-line), so we are probably not that likely to see many more plasmoids. However, we may see newly closed field lines after reconnection as well as the auroral counterparts of magnetotail processes. Also for people who are interested in looking at rings or the inner moons, or for those who want to study the internal field of the planet, it's going to be amazing, so we are very pleased that *Cassini* has been extended.

Dr. G. Q. G. Stanley. With the reconnection you see, do you find a correlation between the northern and southern aurorae?

Dr. Jackman. Yes. Depending on the season of Saturn, the planet may be tilted one way or another, so that if we are observing with *HST*, for example, we can only see either north or south, and the images that I have shown you are of the southern aurora, taken during southern-hemisphere summer on Saturn. But we do also have a number of instruments on *Cassini* that are able to look at the aurora *in situ*; *Cassini* has an ultraviolet imaging spectrograph and an infrared instrument. So if the spacecraft is sufficiently far down the tail, it can see the northern and southern aurorae at the same time. But studies of that have only really just started in recent years. Owing to the viewing geometry, you are seeing the aurora quite edge-on, and so it's difficult to get a good projection. Overall the morphology of the northern and southern aurorae is similar, although there is a magnetic anomaly which means the main auroral ovals are slightly offset from one another.

Dr. Stanley. With the Earth, you are on the planet rather than outside the planet, and you could measure it from the planetary surface.

Dr. Jackman. On the Earth there are ground-based magnetometers and all-sky cameras and we are completely spoiled with the ability to observe the auroral regions quasi-continuously. On Saturn we do what we can with *Cassini* and *HST*, when we have observing time allocated.

Professor D. Lynden-Bell. How energetic are the most energetic particles you get out of a single reconnection event? How many MeV?

Dr. Jackman. Gosh! I would have to check.

The Vice-President. It can be worked out! A final question?

Professor M. Mendillo. In your rubber-band analogy, when a plasmoid is ejected away from the planet, and the rubber band connects, is there an equivalent plasmoid heading towards the planet? And is that ejected?

Dr. Jackman. Probably a more appropriate term would be a dipolarization, because the feet of those field lines are still connected to the planet; and while the plasmoid is entirely disconnected from the planet, on the other side the newly closed field lines still have their feet in the north and south. The most appropriate picture would be of field lines that were stretched and have both ends connected to the planet. Following reconnection these field lines want to shorten and thus relax to a lower-energy state.

The Vice-President. Good; let's thank Caitriona once more! [Applause.]

The final speaker for this session is Dr. John Monnier of the University of Michigan. I believe he came here to participate in the meeting earlier today on high-resolution imaging, and he will tell us about, 'Imaging surfaces of stars and the mysterious case of Epsilon Aurigae.'

Dr. J. Monnier. When the *Hubble Space Telescope* (*HST*) stares into a 'blank' piece of sky, it is found to be filled with galaxies — some at the outskirts of the observable Universe. Indeed, the Universe is filled with galaxies — a typical galaxy is only about 40 galactic radii away from the next closest one. You might expect the nearest galaxy to subtend approximately 1 degree of arc (as

Andromeda does). The situation for stars is quite different — typically nearest neighbours to the Sun are roughly 40 *million* stellar radii away. Consequently, even the nearest stars are ≪ 1 degree in angular size, closer to about 5 milli-arcseconds (0·000001 degrees). Indeed, while the small 2·4-m size of the *HST* is enough to resolve even distant galaxies, it would take a 300-m telescope to have enough angular resolution to resolve just the closest stars.

We report a breakthrough in imaging by using the *Michigan Infrared Combiner* (*MIRC*) instrument on CHARA, a six-telescope optical/infrared interferometer with the world's largest baselines (maximum 330 metres). When the light from the CHARA telescopes are combined with *MIRC*, we can use the technique of aperture-synthesis imaging (developed by radio astronomy) to make model-independent images at better than milli-arcsecond angular resolution.

The first main-sequence star to be imaged (besides the Sun) was the nearby, rapidly-rotating, hot star Altair (α Aquilae). We found the star to be elongated due to centrifugal forces at the equator causing the star to spin up to an oblate shape. Even more interesting, the equator is seen to be visibly darker than the rest of the star, the first clear detection of an effect called 'gravity darkening', first hypothesized in 1924 (by von Zeipel). We have gone on to image four more rapid rotators, finding strong deviations from the theory of gravity darkening.

In addition to stars, our angular resolution is so high we can actually image interacting binary stars for the first time. In an interacting binary system, the stars are so close that the gravitational field of the one distorts the shape of the other and can often lead to mass transfer, one star stealing mass directly from the surface of the other. We can now see a movie of this for the first time, as in the case of the famous visual star β Lyrae. One can see the swollen Roche-lobe-filling star that continues to 'donate' some of its mass to its very close-by companion.

Lastly our *MIRC* combiner has captured an amazing series of images of the ε Aurigae system. This system has puzzled astronomers for 200 years — a 27-year binary system with repeating 1·5-year-long-duration eclipse. Each eclipse has been greeted by new technologies and this time around we have made resolved images showing the silhouette of a thin, dark disc moving across the surface of the star. Our work has settled the debate and revealed the nature of the disc responsible for the partial eclipse that naked-eye observers around the world have enjoyed for centuries.

Dr. A. Whiting. How faint can you image these stars?

Dr Monnier. With this technique, the faintest thing we have done is around $H = 5·5$. One thing to keep in mind when you are imaging stellar surfaces is that we're not limited by the brightness of the star, but really by the resolution. So for anything hotter than 3000 or 4000 K, as the stars get fainter, they subtend a smaller angle because they are just getting further away. For young stellar objects we are trying to do this now: imaging planet-forming discs around young stars, which have a much lower temperature, where the surface brightness is much lower; and for that project we really need more sensitivity. But for this type of work, for main-sequence stars above 3–4000 K, it actually turns out we are limited by our baseline, not by the flux. We do want to improve our flux sensitivity in order to do additional work on young stars, discs, and dust emissions.

Dr. R. C. Smith. I notice on Altair you have a hot spot which is not at the pole; is that real?

Dr. Monnier. You mean the bump on the surface-brightness distribution? That's at the 2–3-σ level, if you look at the fluctuations and the reconstruction.

We do not expect this is real because this spins every 8 hours, something like that; this is derived from multiple nights of data, and so it would not have stayed in the same place if it were real. And we see it on both nights, which suggests that it's more of an imaging artifact coming from the fact that we don't have a large number of telescopes. This work is very challenging; the *Very Large Array*, say, has 27 telescopes, but when you only have six telescopes, you can have problems.

Mr. M. F. Osmaston. Just a caveat on using line broadening for determining rotation rates: Gerard Kuiper, 60 years ago I think, pointed out that there is a sharp drop in line broadening and inferred rotation rate in the mid-F-type stars. He was questioning how stars could suddenly lose their angular momentum that was implied by this conclusion. I would just ask whether that drop in apparent line width is still present in the mid-F stars, and if so, perhaps you have to be very careful in how you interpret that as a sudden drop in rotation?

Dr. Monnier. In the mid-F stars you start to have a thicker convective envelope and chromospheric activity. So yes, there is a sudden drop-off in average rotation rates as you get to around 8000 K, below which there is chromospheric activity, spot activity, signatures of magnetic fields, and correspondingly you have low rotation; the rotation rate goes down with age if you look at the population.

What's interesting in some of these objects, like Altair for instance, is that the pole is hot enough that you don't expect any magnetic fields or chromosphere, but the equator is actually below this threshold. So the atmosphere is sort of in-between an F star and an A star — there is over 1000 K difference, and so you actually span an entire spectral type between the pole and the equator. Altair emits some X rays, which often means there is a companion, a lower-mass companion, but Altair doesn't seem to have a companion; one idea is that there is a little bit of magnetic and chromospheric activity at the equator that's generating something local. On the star-spot question, we do want to have a project where we take some data and we look to see if we can see any evidence of spots going around just at the equator, but we really need a lot more data than we currently have to look for that.

Mr. Osmaston. That's the best indicator?

Dr. Monnier. Yes.

The Vice-President. Thank you very much for your interesting talk. [Applause.]

May I remind you of our usual drinks reception in the RAS library immediately following this meeting, and I give notice that the next meeting of the Society will be on Friday, December 9.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2011 December 9 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

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

The President. The first three talks of this programme are from the thesis prize winners. The first is by Duncan Forgan from ROE, on 'Probing self-gravitating discs using smoothed particle hydrodynamics and radiative transfer'.

Dr. D. H. Forgan. Today I will talk about the work undertaken during my PhD, which focussed on the protostellar discs that surround young stars. In particular, I was interested in the very earliest phases of their existence, usually known as the 'self-gravitating phase'. Self-gravitating discs have masses comparable to the central star, and astronomers must account for this self-gravity when modelling them.

Self-gravitating discs can exhibit quite striking behaviour. If the disc is sufficiently cool and massive, then gravitational instability can set in. This produces spiral arms, which sweep through the disc, heating it through shocks. These shocks will tend to push the disc away from instability. If the disc can find a way of cooling after being heated by these shocks (for example by radiating), then we can see a balance being struck. The shock heating and radiative cooling modulate the gravitational instability, allowing the disc continually to produce spiral waves that repeatedly grow and decay before being replaced by new waves.

If the disc cooling rate is high, then it needs to produce very 'strong' spiral waves to maintain this uneasy balance. Theoretical work has shown that spiral waves can only produce a certain amount of stress before the wave begins to fragment into smaller objects. This process has been touted as a means by which giant planets and low-mass stars have been formed, although it has been shown by many authors (and confirmed in my thesis) that this process only tends to work at large distances from the parent star.

As we can see, understanding the cooling rate is extremely important if we are to understand gravitationally unstable discs. My PhD focussed on developing radiative-transfer methods for smoothed particle hydrodynamics (SPH), a method of hydrodynamic simulation that uses particles rather than grid cells to simulate a fluid. I then applied these radiative-transfer methods to problems associated with gravitationally unstable discs.

Firstly, how well does α-disc theory approximate self-gravitating protostellar discs? The theory assumes that turbulence is active in the disc, and models it using a pseudo-viscosity. By using a dimensionless α-parameter, we can afford ourselves a reasonable amount of ignorance about what is causing the turbulence.

Self-gravitating discs do produce a form of turbulence due to the stochastic nature of their spiral structure (it is sometimes referred to as gravito-turbulence). Previous work had shown that, in general, this α-approximation was liable to fail (as the spiral structures could become so intense they would dominate the gravitational potential at long distances, invalidating the approximation). But this work had been less clear as to under what circumstances such failures might occur. By running a series of disc simulations, we were able to quantify where α-disc theory failed in the self-gravitating case, and see clearly what caused the failure.

In a second application, I looked at the outcome of binary encounters on protostellar disc systems. There were two aspects to this investigation: (*i*) Do perturbations caused by the binary companion stimulate disc fragmentation? (*ii*) What happens to the accretion rate of the primary star, and is the effect similar to an FU Orionis outburst event?

Perturbations generally do not cause disc fragmentation, as the approach of the binary heats up the disc by producing a tidal arm inside it. Equally, there are certain circumstances in which an encounter might partially destabilize the disc (if the encounter was distant), but not produce a fragment.

FU Orionis outbursts are observed in young stars, where the luminosity of the star rises on short timescales (a few years), and decays on longer timescales (decades). It is believed that FU Ori events are due to enhanced accretion by the star, and a burst of disc instability might be the cause. The passage of the secondary close to the protostellar disc results in angular-momentum exchange through the tidal arm, which allows matter to flow onto the star at increased rates. In fact, both stars experience bursts of accretion, with well-defined signatures. A key feature of FU Ori events is that they are repeatable — bursts such as those produced by the encounter are repeatable if the secondary is bound, and the period of the bursts will be equal to the period of the secondary's orbit.

However, I showed that the time-scales on which the bursts evolve is not the same as for FU Ori, and the frequency with which bursts of this type should be seen (extremely infrequently) does not tally with the observed frequency of FU Ori events. We therefore concluded that this was a different type of outburst, which is unlikely to be detected.

Finally, I displayed results from what was developing work at the end of the thesis, regarding Monte Carlo Radiative Transfer (MCRT). MCRT follows the path of individual photons inside a medium, allowing them to be absorbed and scattered. Monte Carlo methods are stochastic, *i.e.*, they rely on drawing values at random from statistical distributions. Given the properties of the disc (density, temperature, composition), we can produce a statistical distribution for the distance any photon can expect to travel before interacting with the medium.

This allows us to model individual interaction events stochastically, tracking a photon's progress as it is emitted from the central star (or the disc), scattered throughout the system, and finally captured on an image plane, just as real photons are captured on a CCD in a telescope. This allows us to build up synthetic images of numerical simulations, so we can compare theory directly with observations.

My contribution was to develop a 'native' MCRT algorithm for SPH. Particle-based simulations are usually smoothed onto a grid of cells before MCRT is used, which results in information being lost at small scales. My algorithm allows MCRT to act directly on an SPH simulation without the need for gridding, preserving the information content. We are now working closely with observers to put this algorithm to good use.

The President. A few questions?

Dr. G. Q. G. Stanley. In your simulations, how many particles were you actually using?

Dr. Forgan. For the simulations I have presented, about half a million particles.

Dr. Stanley. And with the angular-momentum transport, how were you letting those leave the grid — how did the angular-momentum transport work?

Dr. Forgan. The point mass at the centre is a specific type of particle that, within a certain radius can accrete particles, so it basically scratches them off. It turns them from being gas particles to being dead particles. That's how the accretion process works. And their mass and angular momentum are added to the protostar.

Dr. Stanley. So the angular momentum is transported out of the disc as well?

Dr. Forgan. Yes — we can resolve the angular-momentum transport out, and the mass will then transport in, as the main result. And that's why you get the accretion onto a separate star.

Mr. M. F. Osmaston. On the question of angular momentum, you were saying that the prograde encounters were more efficient, but that doesn't relate to the spins of the orbits you are creating. In our planetary system we have six out of the eight planets with prograde spins, and the vorticity in a Keplerian disc is retrograde, so you've got a problem!

Dr. Forgan. When I say prograde, I mean in the loosest, global sense. I am talking about the general rotation of spiral structure in one direction. I am not talking about details. But it's a fair point.

Mr. Osmaston. What you are doing does not apply to our planetary system?

Dr. Forgan. This doesn't apply to our Solar System in any way. All that it applies to is the physics of the disc, at the point of fragmentation. What happens next is not really for me to say. It's not something I can resolve in my simulations.

Mr. M. Ince. Following on from that, when you do this sort of run, roughly how much time are we talking about?

Dr. Forgan. In these cases here, tests were run for about 10 000 years' simulation time. Within that period the disc can accrete something like half its mass. It's very rapid — you don't expect to see many self-gravitating discs because this phase is accreting so rapidly.

Mr. Ince. And a stellar fly-by would be pretty weak.

Dr. Forgan. Yes, you've got a variable time-frame for an encounter to happen, the disc is still self-gravitating. That's why it's so rare, and that's why we're not seeing these outbursts.

The President. What was the mass of the encountering object?

Dr. Forgan. The mass of the encountering object here is a $0{\cdot}5$-$M_\odot$ star with a $0{\cdot}1$-$M_\odot$ secondary.

The President. One more question.

A Fellow. Is the radiative transfer very different from the sort of disc you get around white dwarfs in a binary situation, because you have dust involved?

Dr. Forgan. I imagine that's because the temperature régimes you are talking about are different. Our codes could be applied to white dwarfs to some extent but, as I said, they are very basic codes, you use simple opacity laws, and the really interesting grain chemistry and things like that are left out. In principle we could do those, but in this case the chemistry is quite different: it's mainly dust and ices that are playing a rôle here. But I can imagine in a white-dwarf primary you might have higher temperatures that might have more molecular opacity playing a rôle, for example.

The President. We should wrap it there; thank you very much!

The next speaker is Dr. James Verdon from the University of Bristol. The title of his talk is 'Microseismic monitoring and geomechanical modelling of CO_2 storage in subsurface aquifers'.

Dr. J. Verdon. Carbon capture and storage (CCS) is one of a number of developing technologies with the potential to reduce anthropogenic CO_2 emissions that contribute to global climate change. CO_2 can be captured at large point-source emitters (such as coal-fired power stations) and transported to suitable sedimentary basins, where it is injected and stored in saline aquifers or depleted hydrocarbon reservoirs.

For CCS to become politically acceptable and economically viable, we must be able to guarantee that storage reservoirs will not leak CO_2 back to the surface. Assuming that the cap-rock integrity was intact prior to injection, the most likely leakage pathways are through wells drilled into the formation, and through faults and/or fractures in the sealing rock, created by geomechanical deformation.

When CO_2 is injected, it increases the pore pressure in the reservoir, which in turn generates geomechanical deformation. If this deformation exceeds a certain threshold, it is accommodated by brittle failure — the formation of fractures or the reactivation of faults, which have the potential to act as leakage pathways from the reservoir. When fractures are formed, or faults reactivated, seismic energy is released. This process is analogous to earthquakes, except that the magnitudes of events are significantly smaller, so they are termed 'microseismic events'. By placing geophones in boreholes near reservoirs, microseismic events can be detected, and the location where brittle failure has occurred can be identified. Microseismic-event locations can be used as a tool to image and understand the deformation of a reservoir during CO_2 injection, and thereby to characterize the risk of CO_2 leakage through faults and fractures.

We demonstrate this by using microseismic events detected during CO_2 injection at the Weyburn oil field, Saskatchewan, Canada. This mature field has been under production since 1955. To increase oil recovery, CO_2 injection was initiated in 2000, at a rate of 3MT per year (similar to the emissions from a mid-size coal power plant, or 500 000 cars). As well as increasing oil recovery, the CO_2 is stored in the reservoir. In 2003 a microseismic monitoring array was installed, 6 months prior to a new part of the field being opened up to CO_2 injection in 2004 January. CO_2 is injected through a vertical well, which is placed in between four lateral production wells trending NE–SW. The vertical monitoring array of six geophones is sited 50 m to the east of the injection well.

Before injection began, a low rate of microseismicity was observed. During injection, approximately 100 events were detected over five years of monitoring, implying a low and manageable rate of deformation. The most notable features of the event locations is that the majority of events are located near to the lateral production wells, rather than the injection point. This is a surprising result — conventional injection-induced seismicity theory, where geomechanics are not taken into account, suggests that seismicity should occur at the injection wells, as this is where pressure is highest, and therefore effective normal stress is lowest.

Furthermore, many of the events are located in the overburden, above the reservoir. Without a geomechanical model of injection-induced deformation, it is not clear why these events have occurred. If these events represented CO_2 migration from the reservoir, or even pore-pressure communication through the overburden (a potential indication of future leakage), then we would expect the events to be located above the injection point, where the greatest concentrations of CO_2, and the highest pressures will be located.

To address these uncertainties, we created a geomechanical model of injection at Weyburn. The pore pressure computed by fluid-flow simulation was used as the loading for a finite-element geomechanical model. We examine the modelled evolution of stress during injection, using the proximity of the *in-situ* effective stress to the Mohr Coulomb failure limit as a proxy for the likelihood of microseismicity (termed 'fracture potential'). We find that inflation of the reservoir around the injection point, along with continued deflation of the production areas, serves to increase the modelled fracture potential in the overburden above the producing wells. This geomechanical explanation provides the most suitable interpretation of the observed microseismic events. The presence of CO_2 above the reservoir does not signify unwanted migration of CO_2.

To verify further our geomechanical model, we also compared the modelled stress changes with observations of seismic anisotropy from the field. Field observations of azimuthal variations in seismic-reflection amplitude indicate that above the lateral production wells the maximum horizontal stress is NE–SW parallel to the wells, while above the injection well, the maximum horizontal stress is rotated to NW–SE, perpendicular to the trend of the lateral wells. These stress rotations match the *in-situ* stress predicted by the geomechanical model. The match between observed and predicted seismic anisotropy adds further weight to our conclusions regarding the cause of the microseismicity.

In conclusion, the observation of microseismic events has enabled us to characterize the geomechanical deformation induced by CO_2 injection at Weyburn. The relatively low rate of microseismicity implies that deformation is relatively small and manageable. Although some events occur above the reservoir, they represent a mechanical transfer of stress from reservoir to overburden, not a migration of CO_2. To maximize the information provided by microseismic monitoring, we linked our observations with a finite-element geomechanical model. It is by linking together these different disciplines that we are able truly to understand and evaluate the leakage risk posed by deformation. Our approach is of relevance for a range of subsurface processes, including management of mature hydrocarbon fields, mining, shale-gas extraction, and even volcanic unrest.

The President. Fascinating stuff!

Professor I. W. Roxburgh. Have there been any measurements of vertical ground movement using InSAR, as has been performed at In Salah, which is another large CCS project in Algeria?

Dr. Verdon. At Weyburn, the ground motion itself can't be used because farming, permafrost, snow cover, and so on, obscure the signal by changing the ground-surface height by far more than the actual signal. One way to get around this is to use man-made reflectors (such as pylons or farm buildings, for example), but there simply weren't enough of those to make InSAR measurements worthwhile at Weyburn. InSAR measurements worked so well at In Salah because the field is in the middle of the Sahara desert with lots of bare rocks that provide immobile reflection points.

Mr. H. Regnart. To give just one example, the last time I made a comparison, lubricating oil costs more than ten times as much as petrol. So surely fossil fuels are simply too valuable to be burnt! My 12-kW capacity photovoltaics are now in operation, and your quotation from Wallace could surely be a motto for the Green Party! [Laughter.] I invite your response!

Dr. Verdon. Indeed, it would make a great motto for the Green Party! Unfortunately I deleted it from my slide, because I was a bit worried about running over time, and wanted to talk more about the science than the politics. But one of my first slides actually shows the International Energy Agency predictions for how we're going to achieve our climate targets, and renewables are a huge slice of that. But with just renewables alone, when you think about how many coal-fired power plants China and India are building, all of us in this room could go away and put solar panels on, but that still wouldn't stop global warming unless we find a way to stop the emissions from those Chinese and Indian power stations reaching our atmosphere in the next 50 years.

Dr. G. Morgan. To continue the provocative theme of it, if you're getting to bury the CO_2, isn't it always at risk of leakage? Indefinitely?

Dr. Verdon. I guess, it's something of a counter-intuitive thing, in that generally when we start with something new — a new computer, a new car — when it's new it works great, but through time the doors fall off, the screen breaks, the batteries fail, *etc.*, *etc*. The converse is true for a CCS reservoir. It's at greatest risk of leaking during the injection phase, because pressures are high, you're forcing more and more in there, and it all exists as a free phase, like a bubble or super-crystal of CO_2. Through time, the CO_2 dissolves into the residual water that's present in the reservoir. So once it's dissolved into the water it can't leak anywhere, and obviously with the dissolution and lack of injection, the pressures drop through time, again, reducing the chances of leakage. And on very-long-term time-scales, the CO_2 is mineralized by reactions with carbonate rocks and it's then stored very, very securely. If you were to do a schematic plot of risk of leakage through time, it drops off through time. And whilst I guess you can never say there is zero chance of it never leaking, because ultimately in 50 million years' time there is going to be another continental collision to lift everything back up again, on any kind of reasonable time scale, if it survives during the injection period, it's got a good chance of surviving indefinitely.

The President. You hinted that the regulatory measures to control seismic consequences were set at a level which is well below that which would realistically affect anything; is that what you intended to convey?

Dr. Verdon. For the Bowland Shale off Blackpool?

The President. Yes, exactly!

Dr. Verdon. Well, a magnitude-2·5 earthquake sounds like a truck going past your house. And that's if you're right at the epicentre. It's classed as a non-damage-causing earthquake. But the rule they've laid out is that, if they get anything above a 1·7, during fracking processes, they have to stop injection immediately, pull all of the fluids out, and aggressively de-pressurise the well, to stop bringing further seismicity.

The President. And what's the geophysics assessment of that ruling? If the geophysics community were asked, what consequences do you think really are appropriate to take into account — is 1.7 the right level — what would the advice be?

Dr. Verdon. I think, based on the lack of knowledge we have so far with the Bowland Shale, it's a very precautionary but very sensible measure to take at this stage.

The President. Thank you! [Applause.]

Our third speaker is Dr. Mikako Matsuura from UCL and the title of her talk is '*Herschel* detects a massive dust reservoir in SN 1987A'

Dr. Mikako Matsuura. Dust grains have been found in the Milky Way, nearby galaxies, and distant galaxies. However, it is still unknown what is the origin of dust in the interstellar medium (ISM) of galaxies.

Our current understanding is that dust goes through a constant cycle of formation and destruction around stars and in the ISM. The life cycle of dust starts with star formation in molecular clouds. Stars evolve, and eventually low- and intermediate-mass stars evolve into the asymptotic-giant-branch (AGB) phase. The circumstellar envelopes of AGB stars have warm and relatively high-density gas, and they are considered to be an ideal site for dust formation. These types of stars lose gas and dust through slow stellar winds, and eventually gas and dust are fed back into the ISM. In contrast, high-mass stars end their lives as supernovae (SNe), and eject gas in a very explosive event. AGB stars and SNe synthesize elements in their stellar interiors. Some of the elements are condensed into dust in the circumstellar envelope of AGB stars and possibly SNe.

These types of stars are considered to be important sources of dust in the ISM, but actual quantities are still unknown, particular SN dust. Some of the dust might be destroyed by shocks in the ISM. There is a suggestion that certain types of dust grains can grow in the molecular clouds, but this is still uncertain. Dust grains go through this cycle of formation and destruction processes in the circumstellar and interstellar medium, and are constantly replenished within a galaxy. My work focusses on dust formed in stars at the end of their evolution, particularly in supernovae.

A supernova (SN) explosion was detected about 25 years ago in a neighbouring galaxy, the Large Magellanic Cloud, at the distance of 50 kpc. This galaxy is nearly face-on, so that astronomers can see the objects within it very well. SN 1987A, a type-II supernova, was the nearest supernova detected in the last 300 years. It provided astronomers with a unique opportunity to observe the evolution of a SN with modern telescopes at all wavelengths.

This supernova was located just at the edge of a star-forming region, the Tarantula Nebula, which was fortunate because it has relatively little ISM dust along the line of the sight.

Dust formation in SN 1987A itself was detected in the early phase. The optical light-curve of SN 1987A followed the theoretical light-curve calculated by energy deposition from cobalt decay up to about 600 days. After about 600 days after the explosion, the brightness declined faster than the theoretical expectation. This difference is due to dust extinction, after dust was formed at about 600 days.

In the 2000s, mid-infrared observations with *Gemini* and the *Spitzer Space Telescope* detected dust in SN 1987A. There is a distinct bright ring seen in the optical images, and also in the mid-infrared, which traces the thermal emission of dust. The spectral-energy distribution of SN 1987A had been already obtained up to 40 microns before *Herschel*'s launch. A model fitted to this shows that the infrared flux drops sharply towards longer wavelengths, so that the initial prediction was that SN 1987A would not be detected with *Herschel*. Once *Herschel* was launched in 2009, fortunately and unexpectedly, we detected SN 1987A at far-infrared and submillimetre wavelengths.

The HERITAGE (HERschel Inventory of The Agents of Galaxy Evolution) project maps the LMC from 100 to 500 microns, covering about a 5° × 5° area of this galaxy. While *Herschel* was mapping the parent galaxy, by chance SN 1987A was detected. *Herschel* detected SN 1987A from 100 to 350 microns, which makes this object a very-well-isolated point source.

We have detected SN 1987A in the far-infrared, but there are three possible sources: thermal dust emission, synchrotron radiation, and fine-structure lines. We have examined all three possibilities. It is easy to remove the possibility of synchrotron radiation because it follows a power law from radio to far-infrared, and its far-infrared flux is significantly lower than the *Herschel* flux. The second possibility is far-infrared fine-structure lines. Theoretical predictions are that less than one per cent of the *Herschel* in-band flux can be due to lines, hence excluding this possibility. The only possible source to explain the far-infrared emission is dust.

Even though far-infrared emission is due to dust, there are again three possibilities for the origin of the dust: it may be formed before the explosion, when the star was in the red-supergiant phase, it might be formed from elements ejected by the supernova, and dust originating in the ambient ISM. To distinguish from these three possibilities, we analyse the temperature and mass of dust.

We estimate the dust mass, using a single species of dust composition. To fit the far-infrared emission, we require about 0·4 to 3 $M_\odot$ of dust, depending on the dust species.

Now, going back to the three possible cases of the dust origin. The first possibility is ISM dust. This has been found quite often in old supernova remnants. When a supernova explodes, it sweeps-up the dust in the surrounding interstellar medium. Dust is detected towards the supernova remnant, but it is not really formed in the supernova, itself. In the case of SN 1987A, the supernova is so young that the volume which has been swept-up is small. The estimated swept-up ISM dust mass is only about 10^{-6} $M_\odot$, whereas the required dust mass to explain the *Herschel* fluxes is 0·4 $M_\odot$ or larger. So it is very unlikely that far-infrared emission is due to ISM dust.

The second possibility is dust formed before the supernova explosion. When the star was in the red-supergiant phase, dust can be formed. The current theory predicts that about 8 $M_\odot$ of gas had been lost from the star during the red-supergiant phase. Considering the gas-to-dust mass ratio, it suggests that about 0·03 $M_\odot$ of dust should have been ejected from the progenitor. However, the required dust mass to explain *Herschel* emission is 2·4 $M_\odot$ for a silicate composition. This is because the red-supergiant forms mainly silicate dust. It is very unlikely that the dust detected by *Herschel* had been formed during the red-supergiant phase.

The only possibility is that dust observed by *Herschel* was formed in the ejected material from the supernova, using elements synthesized in the star and supernova.

Our big surprise was that we detected enormous amounts of dust made by SN 1987A. Its maximum possible dust mass is limited by the available elemental mass. Considering the elemental abundance from current theoretical models, the current dust mass is estimated to be about 0·4–0·7 $M_\odot$.

If supernovae, in general, can make such significant masses of dust, and if dust formed in supernovae can survive subsequent shocks, supernova dust production could actually explain the amount of dust found in the interstellar medium. In the Large Magellanic Cloud, there is about 10^6 $M_\odot$ of dust present in the ISM. We have measured the dust mass ejected from low- and intermediate-mass stars, AGB stars, but it seems to be much lower than the measured 10^6 $M_\odot$. However, if supernovae can make a significant amount of dust, as found in SN 1987A, supernovae can generate the required amount of dust measured today in the LMC's ISM.

Dust in supernovae can also help our understanding of the dust evolution in high-redshift galaxies. SDSS J114816·64+525150·3 is one of the furthest galaxies found, and it has been detected at sub-mm wavelength. This galaxy has about 10^8 $M_\odot$ of dust. The age of the galaxy is about 0·4–0·8 Gyr, which gives a short time for the stars to evolve and produce dust in large quantities. If high-mass stars can produce a large amount of dust, we can explain the origin of dust in such high-redshift galaxies, because they evolve faster. To explain such a high dust mass in high-redshift galaxies, models predict about 0·1 $M_\odot$ of dust produced per average-mass supernova. This condition can be fulfilled if average-mass supernovae can make dust in similar quantities that *Herschel* has detected in SN 1987A.

Mr. M. Hepburn. Surely, observationally, the precursor star was a white, or more or less white, giant.

Dr. Matsuura. Yes, just before the explosion, it was a luminous blue variable. It first went through the red-supergiant phase and then became a luminous blue variable, so just before the explosion it was a very luminous blue star.

Professor P. G. Murdin. The progenitor for SN 1987A was a member of a small cluster of stars. There are perhaps, half a dozen, maybe ten stars in the immediate vicinity. Is it possible this dust is the sum of dust from each of these stars added up?

Dr. Matsuura. I haven't really calculated that case, but I think unless there are a lot of elements ejected from these stars in the cluster already, it is actually very difficult to explain such a large amount of dust. It's not completely excluded, but it's very difficult, I guess.

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

This has been quite a week for astronomy in the news. The day before yesterday the story was of a supermassive black hole, and John Humphrys said something on Radio 4, and yesterday the *Extremely Large Telescope* was on the *Today* programme, but neither of these were this week's biggest astronomy-news story. That has surely to go to Kepler 22b, a habitable planet. I was particularly impressed by this when I got out of the taxi at Munich airport on Wednesday morning and I was faced with a taxi driver with a folded-over newspaper. On the front there was a picture of Kepler 22b, and he immediately started quizzing me about it! So it just shows what kind of impact those stories have on the general public. This is a particularly timely story for us as we had a specialist discussion meeting today on 'Is the Earth special', which has also given us the opportunity to invite Professor James Kasting, from Pennsylvania State University, to be our final speaker today, on 'What has *Kepler* taught us about the prevalence of habitable planets?'.

Professor J. Kasting. The pace of discovery in exoplanet science is breathtaking. Measurements made using ground-based radial-velocity (RV) and other techniques have now identified over 700 planets, including 94 multiple-planet systems. Most of these planets are much more massive than Earth and are probably ice- or gas-giants like Neptune or Jupiter, but some of them — those with masses less than 10–15 Earth masses — may be rocky planets like Earth. The space-based *Kepler* telescope, which has been in orbit around the Sun for the last 2·5 years, has now identified another 2300 or so 'planet candidates' by using the transit method. The data are not yet published, but the results have been summarized in a press release. These are classified into five categories based on the planet's radius relative to Earth's radius, R_E. The most populous group, with 1181 objects, is the Neptune-size category (2 – 4 R_E), but super-Earths (1·25 – 2·0 R_E) and Earths ($< 1{\cdot}25\ R_E$) are also well represented, with 680 and 207 objects, respectively. Smaller planets are harder to detect than large ones, so these data are likely consistent with Earth-size planets being the most abundant objects. This conclusion is consistent with the RV results, which show that the number of planets increases towards low planet masses.

Although all of these new discoveries are exciting, the big payoff will come when we are able to find and characterize Earth-like planets around other stars. To do this we will need to build large, space-based telescopes such as NASA's proposed *Terrestrial Planet Finder-Coronagraph* (*TPF-C*) or ESA's proposed *Darwin Infrared Interferometer*. Occulters, or star-shades, can also be used at optical/UV wavelengths. If one of these missions were to be launched, it could, at the same time, search for evidence of life by looking for various biosignature gases. This would be a paradigm-changing discovery, regardless of whether the result is positive or negative.

To design a *TPF*-type mission, one must make several assumptions. First, we need to decide what exactly we are looking for. Biologists argue among themselves about what it takes to be alive. One definition that is nearly universally accepted is the following: 'Life is a self-replicating chemical system capable of undergoing Darwinian evolution'. But this definition is more useful for biologists staring through a microscope than it is for astronomers gazing through a telescope. Remote-life detection on extrasolar planets necessarily involves measuring the by-products of metabolism. On Earth, these by-products include gases such as O_2, CH_4, and N_2O, all of which are much more abundant in Earth's atmosphere than they would be on an abiotic planet. These gases are produced by carbon-based life forms operating in an environment that is rich in liquid water. So, it is conservative to base our mission design on those requirements. Furthermore, the reason that life is able to modify Earth's atmosphere to such a great extent is because organisms are able to exist near Earth's surface where they can use the bountiful energy of sunlight to drive their metabolisms. A biosphere subsisting on subsurface liquid water, such as might conceivably exist on Mars or Europa, would be virtually impossible to detect from afar. This suggests that we should restrict our search to planets within the habitable zone of their parent star, defined as the region in which liquid water can exist on a planet's surface. Fortunately, the habitable zone is thought to be relatively wide as a consequence of stabilizing feedbacks between atmospheric CO_2 and climate; hence, this requirement may not be overly restrictive.

In designing a *TPF*-type mission, it would also help greatly to know the frequency with which Earth-like planets occur around other stars. Exoplanet hunters define the parameter $\eta_{\rm Earth}$ as the fraction of stars that have at least one rocky planet within their habitable zone. If $\eta_{\rm Earth}$ is high, then fewer target stars need to be examined in order to have a good chance of finding an Earth. We need look at only the closest stars, so the angular resolution, θ, of the telescope does not have to be as great. Because θ is typically some small multiple of λ/D (the wavelength of the observation divided by the diameter of the telescope), a high value of $\eta_{\rm Earth}$ implies that one can get by with a smaller telescope. The *TPF-C* telescope, mentioned above, was designed with an $8 \times 3{\cdot}5$-m mirror that would operate at $4\lambda/D$ at $\lambda = 500$ nm. This gave it an inner working angle of ~ 50 mas, allowing it to search roughly half of the habitable zones of 60 nearby stars. For $\eta_{\rm Earth} = 0{\cdot}1$, the default value used in that study, this yielded an expected value of three Earths that ought to be detected. Equivalently, this mission would have had a 95 per cent chance of detecting an Earth-like planet, if such planets exist.

The new *Kepler* and RV results suggest, but do not yet demonstrate, that $\eta_{\rm Earth}$ may be significantly higher than 0·1, as discussed below. The latest release of RV data from the *HARPS* group, which currently has the greatest sensitivity to small planets, indicates that over 50 per cent of solar-type (FGK) stars have a planet of any mass in an orbit of < 100 days. Some of these planets are gas- or ice-giants, but others are almost certainly rocky. (One can only derive the density of the planet if it also happens to transit, allowing a measurement of its radius.) Most of these planets are well inside the inner edge of their star's habitable zone; however, a straightforward extrapolation of these results suggests that the habitable zone should also be populated.

The new *Kepler* data contain significantly more information about small planets, as summarized earlier. They do not yet allow a direct measurement of $\eta_{\rm Earth}$, as three years or more of data would be required to find the Earth around the Sun. (Typically, three transits are needed to be confident that a planet exists. Even then, certain false positives, notably background eclipsing binary stars, need to be eliminated before one can be sure that a planet exists.)

The two years of data represented in the December (2011) data release have yet to be thoroughly analyzed. However, two separate estimates of η_{Earth} were published earlier this year based on the February (2011) data release, which contained only the first four months of data. Catanzarite & Shao estimated that $\eta_{Earth} = 0{\cdot}01 - 0{\cdot}03$, whereas Traub estimated $\eta_{Earth} = 34 + 14$ per cent. The difference between the two calculations was almost entirely caused by their inclusion or exclusion of planets with orbital periods > 42 days. Catanzarite & Shao included such planets in their analysis, whereas Traub ignored them on the grounds that the data were incomplete in this region of parameter space. As there were only 138 days of data, and as three transits are required to be confident in seeing a planet, it seems likely that Traub's estimate for η_{Earth} is closer to the truth. But it is clearly based on a very large extrapolation, and so one should recognize that it might change as the dataset is extended. The *Kepler* team themselves have held off on publishing estimates of η_{Earth}, preferring to wait until the data are more complete. In any case, by two years from now, *i.e.*, by 2014 January, we should have a pretty good idea of what the value of η_{Earth} really is. And that will be welcome news for those of us interested in building *TPF*. We will then need to tackle the even more difficult question of how to find enough money to pay for it. [Applause.]

The President. Thank you very much; questions?

Mr. M. F. Osmaston. I may have misunderstood you, but you seem to be saying that hydrocarbons, like methane, are hallmarks of biogenic origin. But if you do that, you have problems with the exoplanets, increasingly in the Uranus – Neptune area, of finding hydrocarbons going up to acetylene, that's C_2H_2, with people even suggesting higher ones than that. It's very difficult to fit that into a biogenic frame.

Professor Kasting. The outer planets are not within the habitable zone, so methane on a planet within the habitable zone is a better biomarker, although it's not as good a biomarker as oxygen. The best biomarker actually is the simultaneous presence of both methane and oxygen. If you saw a planet that was within the habitable zone and it had methane, you might think that it had life, but there are known abiogenic processes for producing methane. So I think we would also want to see other biomarker gases.

Mr. R. Stripe. When you get the density of these planets, will you be able to tell whether they have liquid cores?

Professor Kasting. Probably yes, if it's a rocky planet, and you can get its density, the models can distinguish between a pure silicate planet and a planet with an iron core. That's not that hard to do, if your limits on the radius are tight enough. You're thinking of the importance of the magnetic field?

Mr. Stripe. I was thinking in terms of the paper that was delivered this morning about life being detected in these hot vapour zones that are deep down in the Earth.

Professor Kasting. As I said, sub-surface life is not that interesting to me, because I think it's almost impossible to detect remotely. So I think we should be focussed on surface life.

A Fellow. If we're out at 600 light years for Kepler 22b, how is the targeting on *Kepler* being prioritized? I guess it's not fuel efficient to look at the nearest ones first, is it?

Professor Kasting. *Kepler* just stares at one patch of sky, they don't target at all, except for that patch.

The Fellow. So it's not being slewed around at all.

Professor Kasting. It's not moved, no.

Rev. G. Barber. Of course, even gas giants could be suitable, in the sense that they have habitable satellites?

Professor Kasting. Sure, and that's been thought of. It's certainly possible. It's actually really difficult to follow-up on, though, because how do you directly image a moon around a giant planet? I don't really think it can be done.

Dr. M. M. Dworetsky. You started to mention that there were tricks to get around the problem of the difficulty of measuring small radial-velocity variations. I just saw an abstract in a collection of exoplanet abstracts just the other day, and the first one on the list was a method of using the star's own photometric variations to compensate for that in some way, to make an approximation for the star's sort of spotty radial-velocity variations, and remove that, to look for even smaller radial-velocity variations caused by the planets.

Professor Kasting. Well, I am glad; you know astronomers are clever, and so there may be ways to reduce the noise. You can certainly beat down the systematics. There are instruments called 'laser combs' that are being developed now for calibration, which are essentially exact. The systematic errors can be dealt with, that is, you can get your calibration standards to be very good, but then the stars themselves are still noisy. The abstract that you mention sounds like a technique for trying to beat down the stellar noise.

Dr. Dworetsky. This is really still just in preprint stage; I think the paper has been accepted, so call them up for something about that.

Professor Kasting. I hope that it can be done.

Dr. Dworetsky. The authors claim that they can.

Mr. Hepburn. Surely this notion of a habitable zone is a remnant of the past — it's the idea of a blackbody which is in equilibrium with solar radiation or stellar radiation, and its own emission. The single example of Venus shows that it's a very bad predictor of the surface temperature of a planet. If Venus is as hot as it is, it's because it's got a 100-bar pressure. If you had a 100-bar pressure out at the radius of Uranus or Neptune, you would expect to get much higher temperatures than the blackbody hypothesis suggests. In consequence I say that it's wrong to say that there is a habitable zone. It comes from the past, it's got no validity!

Professor Kasting. You know, I didn't get a chance, because of lack of time, to explain the logic behind our habitable zones. It's not just a static blackbody temperature that we're calculating. These are planets that have very active feedback processes, so as the planet gets closer to its star we assume it has an ocean like the Earth. That ocean gets vaporized progressively, and so the greenhouse effect increases. You get a very dense water-rich atmosphere on the inside, giving you a runaway greenhouse. That defines the inner edge of the habitable zone. And on the outer edge, we assume that planets were like the Earth: they have a carbonate–silicate cycle that replenishes the CO_2 in the atmosphere, and the farther you move the planet out, the more CO_2 accumulates in the atmosphere. So, it's far from a static concept; rather, it's a very dynamic planet that we're considering, a planet that has dynamics like the Earth does.

Mr. Hepburn. But that doesn't answer my challenge that at the Neptune level, Neptune and Uranus are not gas giants, in the sense that Jupiter and Saturn are. It's quite a plausible hypothesis that they're essentially made of liquid or solid water, and the greenhouse effect on them is enhanced, not because of CO_2 but because of hydrocarbons, as another commenter just said. I do not believe that you can say with certainty that there is such a thing as a habitable zone which is limited on the outside, and I've never seen any convincing claim to refute what I've just suggested. [Laughter.]

Professor Kasting. I don't think that there's any solid surface on Neptune or Uranus, on which life could get going.

Mr. Hepburn. You would expect that there would be either a little bit of a solid surface — they're just not big enough to be gaseous throughout.

The President. We have lots of other people who want to ask questions, so could you continue later?

Mr. Hepburn. I'm sorry!

Dr. Giovanna Tinetti. Just one comment: you were talking about the need for exoplanet detection to see the atmosphere of the planet. I'm like you, very interested in seeing these atmospheres. But, with the transit technique we can very effectively get to the habitable zone of late-type dwarfs, relatively easily. So we don't have to wait for a very big experiment for that.

Professor Kasting. Yes, I know, you have a mission proposed, *EChO*, that would do this. I'm sceptical myself, because I've talked to the people on the *James Webb Space Telescope*, which is a bigger telescope which they hope to use for transits, and they're iffy as to whether they think they can get transit spectra of an Earth in the habitable zone.

Dr. Tinetti. That's because *JWST* is a fantastic telescope but it's a telescope that is optimized for background observations, not for viewing an extrasolar planet. We need for that a photon-noise-limited type of telescope. The *JWST* is not ideal for extrasolar planets.

Professor Kasting. That's news to me! If *EChO* outperforms *JWST*, then I'm very interested, of course.

Mr. Regnart. Forgive me if I'm asking a question you may have answered in different terms. Could you comment on the suggestion that if a rocky planet could be detected surrounded by an atmosphere in thermodynamic disequilibrium, that would be indicative of life?

Professor Kasting. There were actually two papers published in 1963; one was by Jim Lovelock, and there was another paper by Joshua Lederberg, which came out earlier that year, and he proposed exactly this criterion that you just mentioned: the extreme thermodynamic disequilibrium in a planetary atmosphere. Methane and oxygen is a specific example of that. But you have to be careful with this because all planetary atmospheres are in some state of disequilibrium. I've heard Lynn Margulis make this argument incorrectly, I think. She has said, "Look at Mars and Venus, their atmospheres are in equilibrium, whereas Earth's is out of equilibrium". Nothing could be further from the truth, because those atmospheres are themselves well out of thermodynamic equilibrium. Take Mars' atmosphere: it's trying to equilibrate at the local surface temperature of 200 K, but it's also getting hit by photons from the Sun that are coming in with an effective temperature of 6000 K, along with short-wavelength radiation. Thus, atmospheres are inherently out of equilibrium to begin with. Another thing that one should keep in mind is this: think about the early Earth before the rise of oxygen. Then what we think you might have been able to detect in its atmosphere is methane. In that case though, the organisms would be driving the atmosphere towards equilibrium, because they would be taking CO_2 and hydrogen, which are in disequilibrium, and making methane, which is closer to equilibrium at planetary temperatures. So what you're really looking for with biosignatures are things that you can explain in the presence of life, that you can't explain in the absence of life. And you have to be careful about it.

Dr. J. Z. Kolendowicz. I've got a question related to two previous questions, one about the habitable zone, and one about liquid-core/iron-core planets. We generate our own magnetic field, and our magnetic field protects us from solar radiation to a certain extent. Now, would you look for that in a planet that is in the habitable zone? Because obviously, if it didn't have that protection, the likelihood of more advanced life would be reduced!

Professor Kasting. This is one of the points that is brought up in the book, *Rare Earth* by Peter Ward & Don Brownlee, and I happen to think that point is overstated. So, why is that? One of the arguments is that planets will lose their atmospheres if they don't have a magnetic field. Mars is cited as an example because a lot of Mars' atmosphere has apparently been stripped away by solar-wind interactions. But Venus doesn't have an intrinsic magnetic field, but it still has about a 100-bar atmosphere. And Venus has a stronger interaction with the solar wind than Mars does because it's closer in. In this case, size matters: the small planet without the magnetic field loses its atmosphere, whereas a larger one doesn't. The other way that magnetic fields are typically invoked is as protection against cosmic rays. During magnetic-field reversals, however, the Earth's magnetic field goes down to, I think, 10% or less of its normal strength, and it has absolutely no correlation with the biological record on Earth, because most of the cosmic rays are absorbed high up in the atmosphere anyway. So basically, I think we shouldn't worry as much about magnetic fields as people have.

The President. Thank you very much again, Jim! [Applause.]

I have two brief announcements: you're invited to a seasonal drinks reception at the Library immediately after this meeting, and the next open meeting of the Society is on Friday January 13th.

VISUAL PHOTOMETRY: COLOUR AND BRIGHTNESS SPACING OF COMPARISON STARS

By Alan B. Whiting
University of Birmingham

A significant amount of data on the historical and current behaviour of variable stars is derived from visual estimates of brightness using a set of comparison stars. To make optimum use of this invaluable collection one must understand the characteristics of visual photometry, which are significantly different from those of electronic or photographic data. Here I show that the dispersion of estimates among observers is very consistent at between 0·2 and 0·3 magnitudes and, surprisingly, has no apparent dependence on the colour of comparison stars or on their spacing in brightness.

Introduction: visual photometry

Measuring the brightness of an object is one of the most basic of operations in astronomy and doing it accurately is one of the most important. For most of the history of the science it has been accomplished by the human eye, supplemented for about the past century by photographic and electronic methods. These are certainly more accurate and objective than visual estimates, but they have not entirely supplanted the eye for two major reasons. First, many historical records are only visual in nature; the best CCD in the world today cannot measure η Carinae in 1835. Second, even in this age of large-scale, rapid surveys, visual estimates may be the only way of getting the desired temporal coverage. Observers from the American Association of Variable Star Observers (AAVSO), for example, are routinely called upon to alert professional astronomers to outbursts or other behaviour of objects in support of observing campaigns. In this way an amateur with a small telescope may trigger the use of the *Hubble Space Telescope* and visual data may be combined with far more sophisticated measurements. As a recent example, Martin *et al.*[1] used visual estimates of η Carinae in conjunction with instrumental photometry and spectroscopy from a variety of telescopes.

But the human eye is not a simple detection and measuring system. In a sense, there is no such thing as raw visual photometric data; everything is heavily processed before it can be recorded. While this automatic processing is no doubt useful in a terrestrial environment it is occasionally annoying to the photometrist. Known effects include the tendency for red stars to appear brighter if stared at for a time and for a star placed vertically above a similar one in the visual field to appear brighter.

To understand better the workings of the visual photometry system, here I concentrate on two features of comparison stars: their colour (compared to the variable) and their spacing in brightness. The hypothesis to be tested in the first case is that, when the colour of the variable and the comparison star are very different, the dispersion of visual estimates will increase, as an uncorrected 'colour term' comes into play. In the second case, the hypothesis asserts that when there is a large difference between comparison-star brightnesses, observers will disagree to a greater extent on where to place the variable. Support for the latter is given by a table in Webb[2], comparing various extrapolations of the six naked-eye magnitudes to telescopic stars. Four famous 19th-Century observers disagreed at the half-magnitude level by magnitude 6·5, a full magnitude by 8·5, and worse at fainter levels.

The sample and processing

While there are several organizations worldwide of dedicated variable-star observers, the largest and the one with the most accessible data is the American Association of Variable Star Observers, whose data were used exclusively for this study. For the investigation we need variable stars with many observations (to give a good delineation of the spread of estimates) and a variety of comparison stars. These conditions essentially limit us to bright Mira-type variables. In principle the results could be different for other types; but the colour analysis is simplified, since no comparison stars will be redder than the variable. Nine stars were chosen from the AAVSO list fitting the requirements and I downloaded from the website one to two periods of visual observations each[3]. For each observation the Julian Date and time, the reported brightness, and the brightness of the two reported comparison stars were extracted.

I initially tried to fit a polynomial to produce a smooth curve for reference (as did Price, Foster & Skiff[4]), but found no number of terms that would reproduce well the overall form without adding artifacts due to unfortunate or badly-placed odd observations. In the end I used a top-hat-smoothed version, its width depending on the density of observations. For each observation I

TABLE I

Summary observational data on the stars analysed

Star	*Starting JD*	*No. of Observations*
R Leonis	2455472·0	756
R Aquarii	2455032·8	148
R Bootis	2455231·5	622
R Canum Venaticorum	2455223·5	319
R Hydrae	2455170·8	67
S Coronae Borealis	2455301·2	407
T Ursae Minoris	2455400·5	414
R Ursae Majoris	2455404·5	344
T Ursae Majoris	2455400·2	468

determined, from AAVSO photometry, the $B - V$ colour of the comparison star nearest in brightness. Also for each observation, I recorded the difference in magnitude between the bracketing comparison stars. In subsequent displays time is presented in days from the first observation used. Table I gives the starting Julian Date and number of observations for each star analysed.

R Leonis will serve as an example of the method of analysis. First the observations were combined with a smoothed curve, shown in Fig. 1. The curve generally follows the centreline of the observations, but by the nature of smoothing underestimates the brightness at maximum and overestimates that at minimum. Those two possible problem areas are kept in mind during subsequent work.

Next, the difference in magnitude between each observation and the smooth curve is plotted against the colour of the closest comparison star (in brightness and in the sky, where two comparison stars are listed as the same brightness). Here in Fig. 2 we see at $B - V \sim 0{\cdot}743$ a clear displacement toward positive differences, a result of the smoothed-curve error at maximum, and at

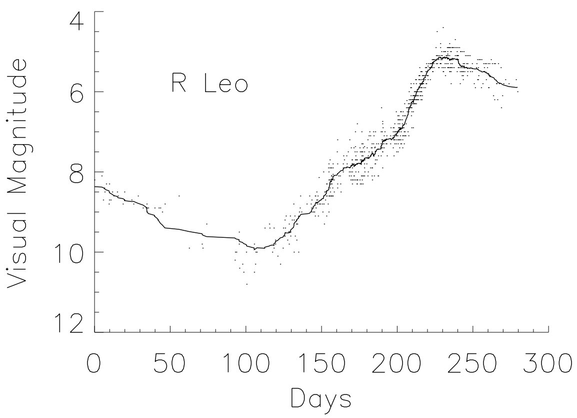

FIG. 1

Visual estimates of the brightness of R Leonis over the period analysed in this study, along with a curve generated by top-hat smoothing.

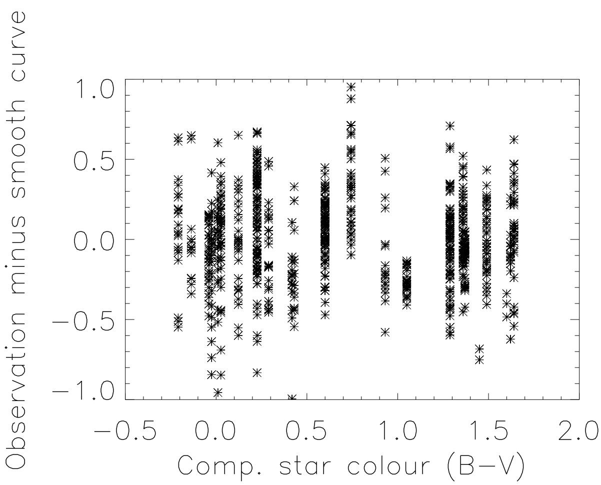

FIG. 2

Deviations of visual-magnitude estimates of R Leonis about the smoothed curve, as a function of the $B-V$ colour of the closest comparison stars. The systematically high residuals at $B-V = 0{\cdot}743$ are due to the failure of the smoothed curve to trace the maximum accurately, and the systematically low residuals at 1·05 come from the minimum.

$B-V \sim 1{\cdot}05$ a displacement toward negative differences from a similar effect at the minimum. It is not clear from this plot that there is any systematic difference between the estimates based on very blue comparison stars (left of the plot) and those based on red stars (on the right), though the overlap of plotting symbols can hide a great deal.

For that reason the data were binned (in the obvious bins, though at times neighbouring colours were combined in order to have enough observations) and the standard deviations calculated about the mean, which gets rid of the systematic offsets at maximum and minimum. The resulting plot of standard deviation against colour appears in Fig. 3. Formal error bars are calculated as $\Delta\sigma = \sigma/\sqrt{n}$, with n being the number of observations in the bin, and are probably optimistic as a measure of actual uncertainty. The outstanding feature of this plot is its featurelessness: there is no clear trend of dispersion of estimates with colour. (A slight trend downward to the right can be imagined, but it is not significant.)

Postponing a discussion of this result until the data on the other variables are presented, we turn to the question of the magnitude gap between comparison stars. The differences between the observations and the smoothed curve are shown in Fig. 4 plotted against the corresponding gap. We note that there are comparatively few observations with a gap as large as a full magnitude, though some show up at $2^{m}{\cdot}7$. Again, the piling up of symbols in columns makes interpretation unclear, so as before the standard deviations of the bins (sometimes combined) are plotted in Fig. 5. And once more we note a lack of any apparent correlation, and again we postpone discussion until the results of all the variables have been collected.

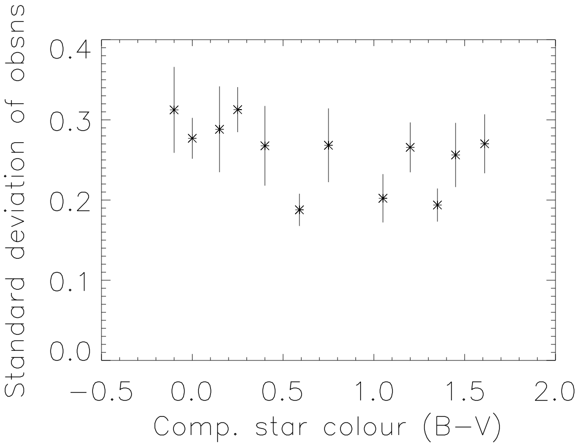

FIG. 3

Standard deviation of observations of R Leonis about the average, plotted against the colour of the closest comparison star. Error bars are assigned based on the number of observations in each bin.

Results

The combined results for all nine stars are presented in Fig. 6 and Fig. 7, produced as in the last section. For several variables the observations were much sparser than for R Leonis, resulting in fewer bins. In each plot there are a handful of points well above the general trend; these (all above about $\sigma \sim 0{\cdot}4$) can be traced to an individual or a few wild points, where an obvious mistake has been made in star identification or perhaps in entering a Julian Date. (Some come from the failure of a smoothed curve to bridge a long gap between observations accurately.)

There is, very obviously and firmly, *no* trend of dispersion among the observations with either comparison star colour or spacing of comparison star magnitudes. This is very surprising. Consider what it means. First, with colour: for all the known problems with red stars and the known differences among people in colour perception, it does not seem to matter whether an M Mira variable is compared with another M giant or a B star; the variation in estimate among observers will be the same. This result appears to be in flat contradiction to that of Price *et al.*[4], who found a strong difference in the standard deviation of visual estimates among stars of various spectral classes. But their work classified by the colour of the *variable*, not the comparison stars; there are in addition other differences in their treatment which make a direct comparison with this work impractical.

Second, it makes no apparent difference to the dispersion of estimates among observers how far apart the comparison stars are in brightness. This is very surprising to an observer. One certainly has a *feeling* of being on firmer ground when placing one's variable on a stepladder of several stars 0·1 magnitude apart, rather than reaching into the wide spaces of whole magnitudes. But this doesn't appear to be true, at least when comparing the estimates of several observers. Up to a gap of 2·7 magnitudes, visual estimates do not suffer the same dispersion by interpolating that they seem to do when extrapolating. For comparison,

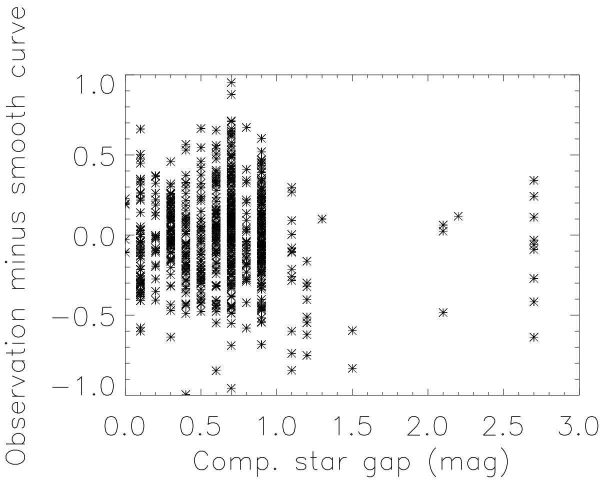

FIG. 4

Differences between observations of R Leonis and a smoothed curve plotted against the magnitude gap between the comparison stars.

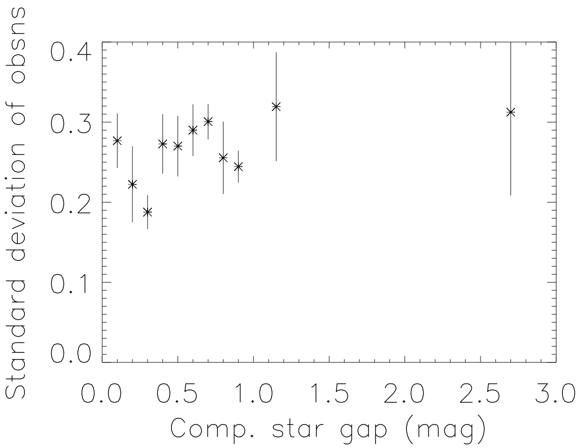

FIG. 5

Standard deviation of the difference between observations and smoothed observations of R Leonis in bins based on the magnitude gap between comparison stars. As before, error bars are based on the number of observations in each bin.

consider that an electronic detector, if limited by shot noise in the comparison star, will have twice the uncertainty if the comparison is made 1·7 magnitudes fainter, and three times the uncertainty for a 2·7 magnitude drop. The situation is not, of course, directly comparable — which is indeed the point.

Perhaps the best way to sum up these results is that the human eye–brain

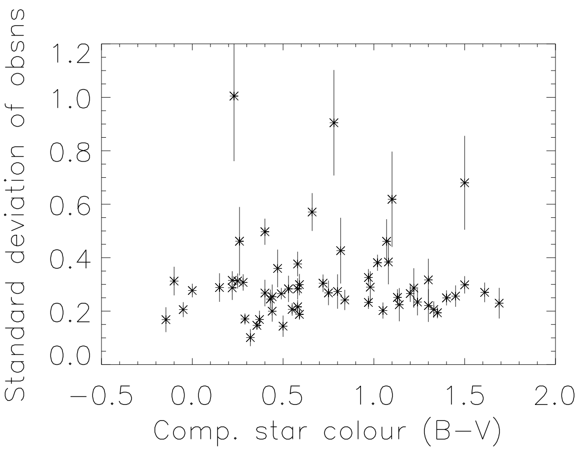

FIG. 6

Standard deviation of brightness estimates for all nine variable stars as a function of $B-V$ colour of the nearest comparison star.

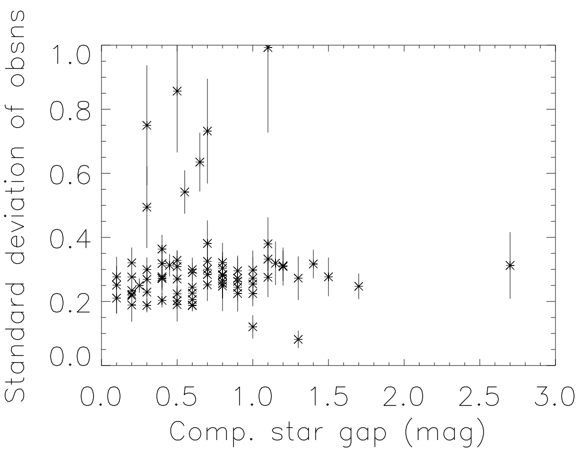

FIG. 7

Standard deviation of brightness estimates for all nine variable stars as a function of the gap between comparison-star brightness.

system does *not* work like any easily-modelled detector when performing photometry.

Implications

It should be borne in mind that these surprising results apply only to the observations of several or many observers taken together; there is both anecdotal and more systematic evidence[4] that observers taken individually are significantly more reliable (with some offset) than the 0·2–0·3 magnitude dispersion found here. But consider the implications. Given a ladder of stars reliably measured as magnitude 8·1, 8·2, 8·3, and 8·4, a set of observers will put them in any order with roughly equal probability. More to the point here, an observer can estimate a variable as being *simultaneously* brighter than a $9^{m}\cdot1$ comparison star and fainter than a $9^{m}\cdot2$ comparison star. Not only will this dent the confidence of a new observer, it can puzzle an experienced one: what number should be reported?

On the other hand, the dispersion appears to be immune to the obvious problems one might expect from inconvenient comparison stars. Perhaps this result will encourage more observations of variables now regarded as difficult and under-observed! A deeper matter is the source of the dispersion. Where does it come from, and how does it behave? There is a great deal of work yet to be done on visual photometry.

Acknowledgements

This study relies on the observations of many dozen AAVSO volunteer observers, whose dedication is gratefully acknowledged though it is impractical to list them here.

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SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 224: HD 180660, HD 183791, BD +57° 2161, AND BD +34° 4216

By R. F. Griffin
Cambridge Observatories

The four stars are all in the same region of the sky, between 19 and 21 hours in right ascension. HD 180660, a K star probably of luminosity class II, is the primary component of a wide visual double-star system; it is not clear whether it is a physical binary. HD 180660 itself is certainly a spectroscopic binary, with an orbit having a modest eccentricity and a period of 180 days. Its visual companion is V342 Aql, a poorly documented eclipsing system with a very large (~$3^{m}{\cdot}0$) *V* amplitude. HD 183791 is a G2 II star in an orbit with a period of about 2½ years and an eccentricity of 0·4. If its $v \sin i$ of 13 km s^{-1} is pseudo-synchronized with the orbital revolution, the implied projected stellar radius is 115 $R_{\odot}$. It has a large mass function that makes it very likely that the unseen companion, if not itself a double system, is an A-type main-sequence star. BD +57° 2161 is a carbon star with large abundance excesses of *s*-process elements; it has an orbit with a period of 541 days and a small but distinctly non-zero eccentricity. BD +34° 4216 is a composite-spectrum system, whose components are probably about K0 III and B9 V, in a moderate-eccentricity orbit having a period of 629 days. Its appearances in the literature mainly stem from its proximity to the β Lyr-type variable CG Cyg, for which it has often been used as a comparison star.

Introduction

HD 180660 and 183791 were selected for orbit determination at Cambridge in 2002, after a listing of Haute-Provence (OHP) radial velocities was made accessible through the Centre de Données Stellaires by de Medeiros & Mayor, who had previously announced[1] that those stars were spectroscopic binaries. BD +57° 2161 was placed on the observing programme in 2004, in response to a request from Dr. L. Zăcs, who (with co-authors) was writing a paper[2] on the spectroscopic nature of that star. BD +34° 4216, too, was put on the programme in 2004, when the observer belatedly came across a paper[3] referring to it as a composite-spectrum binary (its nature had, however, been discovered previously) and giving radial velocities measured from five plates and appearing to show major changes on a fairly short time-scale.

HD 180660 (ADS 12259 A)

This 8^{m} star is to be found in Aquila, some 8° preceding Altair. It is the brighter component, usually by half a magnitude, of the wide visual double star ADS[4] 12259. The system was first listed by F. G. W. Struve in a catalogue[5] published in 1843; from the shorthand designation of that catalogue and the serial number that it accords to the system of present interest, the latter carries the 'discoverer's designation' OΣ 370*. (Such designations were useful before any general catalogues (even the *BD*) had been compiled.) The visual companion, about 19″ north-following, is HD 180639, and is also known by its variable-star designation V342 Aql; it was discovered as an Algol-type eclipsing system by Hoffmeister[7] in 1935, and was officially gazetted as a variable star and dignified with its constellation designation[8] in the following year. The position angle of the companion with respect to HD 180660 is, and always *has* been (ever since the pair was first measured in 1844 by Struve[9]), about 14°. It is, therefore, an irregularity that the companion was entered in the *Henry Draper Catalogue*[10] with the number 180639 — *earlier* than HD 180660 and listed with a right ascension a tenth of a minute of time earlier — although the star is actually about five seconds of arc (about 0·3 seconds of time) *later*. That is despite the fact that the *Henry Draper Catalogue* positions (except for comparatively bright stars) were derived simply by updating the positions found in the *BD*. In *that* catalogue[11] the stars are correctly designated in order of right ascension as +9° 4047 (HD 180660) and 4048 (HD 180639), the latter following the former by 0·5 seconds of time. The *BD* identities of the stars are inverted in error in the ADS[4] catalogue.

The magnitude and colour indices of HD 180660 have been given by Hall & Weedman[12] as $V = 8^{m}\cdot22$, $(B-V) = 1^{m}\cdot37$, $(U-B) = 1^{m}\cdot37$, and by Fernie[13] (albeit in a table concerning which he offered the advice *caveat emptor*!) as $8^{m}\cdot22$, $1^{m}\cdot35$, and $1^{m}\cdot32$, respectively. Hall & Weedman also gave the corresponding magnitudes of the visual secondary, as $8^{m}\cdot72$, $0^{m}\cdot33$, and $-0^{m}\cdot04$. Otto Struve, despite his disclaimer of accurate colour estimation on p. 156 of the substantial (French) introduction to his work[9], propounded for the colours of the pair (in Latin, following his father's convention) *subrubra* and *subcaerulea*, which we have to agree are in admirable accord with the now-observed colour indices.

Stephenson[14] classified the spectra of the visual pair as K4 II and A4 II, and gave their absolute magnitudes as $-2^{m}\cdot3$ and $-1^{m}\cdot8$; he also listed their projected linear separation as 19000 AU, implying a distance of just 1 kpc and thus a distance modulus of 10^{m}, so he must have allowed for half a magnitude of visual absorption. It is of course also implicit that he supposed that the two objects do constitute a physical system. Disappointingly, that cannot be checked by reference to *Hipparcos*, even though both stars feature in that catalogue: they are listed there with parallaxes of –6 and –7 milliseconds of arc, with uncertainties to match, and even their proper motions and *their* uncertainties are also of the same order. The re-reduction[15] of the *Hipparcos* data has not improved the situation at all. The waters have been further muddied by a re-classification of the two stars by Abt[16], who gave their types as K0 IV and "A2 IIp (3760 OIII st)", which he said meant that their luminosities differed by $5^{m}\cdot9$ while their apparent Δm_V was $0^{m}\cdot6$, so the pair was clearly optical. Confidence in his results is, however, hardly enhanced by his comment that "The classifications … include … a strange spectrum (ADS 12259B) that needs confirmation.".

V342 Aql (ADS 12259 B), which is of concern in this paper as a possible physical companion to its visual primary, HD 180660, is the subject of a very

* The anomaly whereby the catalogue[5] that was published under the name of F. G. W. (Wilhelm) Struve, whose conventional designation is Σ, is seemingly attributed by the double-star fraternity to his son Otto (OΣ) was outlined by the writer[6] in Paper 42 of this series thirty years ago, in what may be regarded as a sympathetic paraphrase in English of the situation outlined in French on (mostly) p. III of the original publication[5].

conflicting and confusing literature. Although there has been quite a number of reports of times of minimum light and even some discussions of the (in)constancy of the period (which is listed in the *GCVS*[17] as 3·390882 days), there seems never to have been a proper discussion or plot of the light-curve. The successive editions of the *GCVS* have noted that the total duration of the eclipse is 14% of the orbital period, *i.e.*, 11 hours, and the duration of totality is 3·2% of the period, or 2·6 hours. The only actual plots of the light-curve[18,19] are restricted to the immediate vicinity of totality. In such plots, Diethelm, Locher & Peter[18] illustrated their contention that at one epoch the eclipse had become less deep and there was no phase of constancy at totality at all. (That contention does not seem entirely compelling; the visual estimates have considerable scatter, and it is hard even to recognize from the sketch of the field the identities of the comparison stars, whose own magnitudes were in any case unknown.) In another case, a flat-bottomed (at $12^{m}\cdot 7$) visual light-curve was contributed by G. Samolyk to an article[19] by Baldwin (the then Chairman of the AAVSO's Eclipsing Binary Committee). The photometric amplitude of V342 Aql is huge, and is reported in the *GCVS* and elsewhere as $3^{m}\cdot 4$ in the 'photographic' region, implying immediately that the surface brightness (in the blue) of the body that is eclipsed at primary minimum would have to be at least 23 times that of the eclipsing body, even if the eclipse were central and only momentarily total, which is far from being the case. The *Hipparcos* 'epoch photometry', however, shows an extreme range of only $0^{m}\cdot 8$ — not very much more than is shown for HD 180660, which has never been suspected of variability at all; the photometric entries for both stars are flagged in the *Hipparcos* catalogue with a note saying "duplicity possibly causing spurious variability". It appears that, although *Hipparcos* obtained 79 observations of V342 Aql at times that were inevitably quasi-random with respect to the phasing of the variation, yet it managed completely to miss eclipse epochs! The eclipse period certainly seems to have changed, by amounts of the order of one unit in the fourth decimal of a day. The changes have been represented as discontinuous by Baldwin & Robinson[20] and again, rather clearly, by Baldwin[19], but comparatively recently Erdem *et al.*[21] have tried to represent them as cyclic and to attribute them to motion in a 45-year orbit opposite an unseen (though surprisingly massive) companion.

Several abstracts[22–24] stemming from meetings of the American Astronomical Society have referred to V342 Aql. The abstracts have reported, *inter alia*, that variations in the λ7774-Å O I line and in Hα show V342 Aql to be an 'active' system[22] which has on-going mass transfer[23,24]. They have also said, first[22], that the primary star is not of type A4 II (as it has generally been listed since Stephenson[14] so classified it), but is a late-B or early-A main-sequence star; later, they[24] said that *IUE* spectra show that the primary is earlier than B8. Radial-velocity curves yielding a mass ratio have also been reported[24]. The writer has not been able to find much of the work adumbrated in abstracts written up in actual papers. V342 Aql is obviously a system that deserves attention.

By comparison with its enigmatic companion, HD 180660 itself has a very sparse literature, most of which has already been indicated above. When de Medeiros & Mayor[1] announced the star as being a spectroscopic binary, they noted that they had just two, mutually discordant, observations. When, three years later, they sent a listing of the individual observations to the Centre de Données Stellaires, there proved to be six of them; the 'extra' ones were obtained much later than the first two, but still before the paper[1] had been submitted. The six observations appear at the head of Table I here, with the

TABLE I

Radial-velocity observations of HD 180660

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

Date (UT)	*MJD*	*Velocity* $km\ s^{-1}$	*Phase*	*(O – C)* $km\ s^{-1}$
1986 May 31·10*	46581·10	−10·0	$\overline{32}$·564	−0·5
1987 June 14·05*	46960·05	−6·1	$\overline{30}$·664	−0·7
1997 Aug. 29·88*	50689·88	−23·9	$\overline{9}$·330	−0·1
30·94*	690·94	−23·8	·336	−0·4
Sept. 18·84*	709·84	−16·7	·440	−0·3
1998 Aug. 15·90*	51040·90	−28·4	$\overline{7}$·275	−0·5
2002 June 2·06	52427·06	−20·2	0·955	−0·6
July 14·03	469·03	−34·2	1·187	0·0
15·01	470·01	−33·7	·193	+0·2
21·01	476·01	−31·3	·226	+0·2
26·07	481·07	−29·5	·254	−0·1
Aug. 12·99	498·99	−22·2	·353	−0·1
17·01	503·01	−20·4	·376	+0·2
27·88	513·88	−16·8	·436	−0·1
Sept. 1·88	518·88	−14·8	·464	+0·2
8·98	525·98	−12·7	·503	0·0
22·86	539·86	−8·7	·580	+0·1
Oct. 4·89	551·89	−6·3	·647	−0·3
12·81	559·81	−4·4	·690	+0·2
23·81	570·81	−3·4	·751	+0·1
Nov. 12·75	590·75	−6·8	·862	0·0
Dec. 4·71	612·71	−25·1	·984	0·0
2003 May 12·12	52771·12	−6·8	2·861	0·0
June 19·06	809·06	−36·6	3·071	+0·2
28·05	818·05	−37·4	·121	0·0
July 14·02	834·02	−32·9	·210	−0·2
27·94	847·94	−27·0	·287	0·0
Aug. 2·96	853·96	−24·2	·320	+0·3
17·03	868·03	−18·9	·398	+0·2
Sept. 10·97	892·97	−11·1	·536	−0·2
24·86	906·86	−7·1	·613	+0·2
Oct. 14·84	926·84	−3·7	·724	+0·1
27·77	939·77	−3·9	·796	−0·1
Nov. 3·75	946·75	−4·6	·834	+0·5
24·74	967·74	−19·0	·951	−0·2
26·71	969·71	−20·9	·962	0·0
Dec. 7·71	980·71	−32·1	4·023	−0·2
2004 May 31·07	53156·07	−26·8	4·994	+0·3
June 17·06	173·06	−37·3	5·088	+0·2
28·02	184·02	−36·8	·149	−0·4
Nov. 13·76	322·76	−13·5	·918	−0·3
26·72	335·72	−26·4	·990	−0·1
29·75	338·75	−29·8	6·006	−0·5
Dec. 5·73	344·73	−34·3	·039	−0·2
2005 May 8·10	53498·10	−9·3	6·889	+0·2
12·12	502·12	−12·6	·912	−0·3
15·13	505·13	−14·7	·928	+0·2
28·05	518·05	−27·9	7·000	+0·3

TABLE I (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2005 June 27·04	53548·04	−35·7	7·166	−0·2
July 22·00	573·00	−25·5	·304	+0·2
Sept. 20·90	633·90	−6·4	·642	−0·2
25·84	638·84	−5·1	·669	+0·1
Nov. 12·74	686·74	−15·8	·934	+0·1
19·76	693·76	−23·0	·973	+0·2
29·72	703·72	−32·4	8·029	+0·4
2006 Apr. 12·17	53837·17	−3·6	8·768	−0·1
2007 July 25·02	54306·02	−21·1	11·366	+0·2
Aug. 3·02	315·02	−18·0	·416	0·0
Oct. 29·78	402·78	−11·0	·902	0·0
2008 Oct. 31·76	54770·76	−16·9	13·941	+0·1
2010 Oct. 27·81	55496·81	−21·1	17·963	+0·2

*OHP measure[1] from CDS, weight ½ — see text

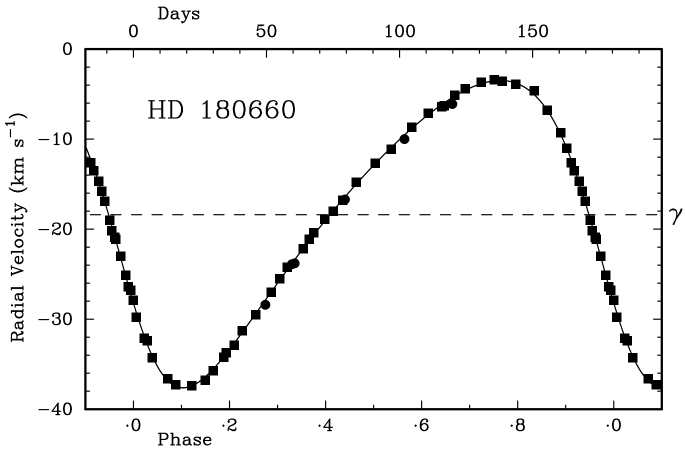

FIG. 1

The observed radial velocities of HD 180660 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. Squares plot the measurements made with the *Coravel* at Cambridge, circles those made with the one at Haute-Provence (OHP), which were half-weighted in the solution of the orbit.

usual adjustment of +0·8 km s^{-1} applied to them. In comparison with the 55 observations made with the Cambridge *Coravel* in 2004–2011, they were given half-weight in the solution of the orbit, which is illustrated in Fig. 1. The listing of orbital elements is delayed until the other objects treated in this paper have been described, whereupon they are all presented together in Table VI. It is a bit unpleasant that Table I shows all six of the de Medeiros & Mayor measurements to have negative residuals; they average −0·4 km s^{-1}. To bring the two data sources fully into agreement, a relative offset of 1·4 km s^{-1} would be necessary, rather than the 0·8 that has already been introduced. With stars of earlier types it has frequently proved sensible to apply negative offsets to the Cambridge data, but for stars as red as HD 180660 they have not been needed. Not wishing to 'improve' the data by too much empiricism, the writer has elected to let the current solution stand. He notes, however, that making a relative offset as large as the seemingly needful 1·4 km s^{-1} would alter some of the orbital elements by amounts at least comparable with their formal standard errors (in particular, the period would become 180·504 days, nearly 2σ up from the value in Table VI), so it must be accepted that the true uncertainties of the elements may be larger than the formal ones. It is of course possible that the extra 0·6-km s^{-1} discrepancy that concerns us is evidence of a genuine change in the γ-velocity of HD 180660, arising from motion in a long-period orbit with an unseen third component, but in the absence of more compelling evidence of higher multiplicity it will be prudent to favour a more mundane explanation of the offset.

If HD 180660 is really of luminosity class II, then it may be a bit surprising that it has an orbit of considerable eccentricity at a period of only six months. The mass function of about 0·08 $M_{\odot}$ is large enough to be significant, but before we could assert what its significance actually *is* we would need to have an idea of the mass of the primary star. For a normal giant of perhaps 2 $M_{\odot}$, that mass function would demand a minimum of about 0·9 $M_{\odot}$ for the secondary's mass, whereas a genuine Class II star might have a mass more like 4 $M_{\odot}$, which would require a secondary of no less than 1·3 $M_{\odot}$. Such secondaries could be main-sequence stars which (in either case) could be so faint in relation to the primary that it is not surprising that no evidence of them has been apparent. The relationship between the observed colour indices does not strongly suggest that there is much admixture of the primary's light with that of a hotter secondary; the indices do, however, suggest that if the primary is a Class II star its type is likely to be more like K2 than K4.

HD 183791

HD 183791 is another star in Aquila, less than 5° away from HD 180660, being about 5½° south-preceding Altair and little more than 1° south-preceding the 4^{m} star μ Aql. Its photometry, like that of HD 180660, but in a different table that lacks any *caveat*, has been provided by Fernie[13], as $V = 8^{m}{\cdot}04$, $(B-V) = 1^{m}{\cdot}15$, $(U-B) = 0^{m}{\cdot}78$. The star happened to feature in the Case study of the Galactic luminosity function (LF), in which it was identified[25] by the designation LF1 +6° 146. The study was made on objective-prism plates taken with the *Burrell Schmidt* telescope while it was still at Cleveland*, and among its results was a classification of the spectrum of HD 183791 as G2 Ib (wrongly recorded in the 'measurements' section of the *Simbad* bibliography as G2 II). The latter type was actually the one found[27] by Miss E. Pisani from two slit spectra obtained with a 1-prism spectrograph giving a reciprocal dispersion of 75 Å mm^{-1} at Hγ on the Lick 36-inch refractor. The spectra formed part of a large project (more than 800 stars, selected from those that were listed in the *Henry Draper Catalogue* as having photographic magnitudes in the narrow range from $8^{m}{\cdot}5$ to $8^{m}{\cdot}6$); the spectra were taken in 1928–37 by Moore and Paddock, whose names appear as the authors of the paper concerned[27], which appeared only in 1950, and it is clear from a footnote that although much of it was organized by Moore it was completed and submitted by Mayall after the former's demise.

* A potted history of the instrument will be found as an extensive footnote in Paper 167[26] of this series.

Eggen[28] gave some photometric parameters for a number of stars, of which HD 183791 was one, but did not conclude anything about it. In a second paper[29] he listed exactly the same parameters again, and went so far as to promise that they would be "calibrated in terms of reddening, abundance and luminosity" in a future paper. He did in fact redeem that promise two years later: in Table 16 of the paper concerned[30] (which is not retrieved by *Simbad*), he listed the star of interest as having $M_V = -3^{m}{\cdot}90$ according to its *DDO* indices and $-3^{m}{\cdot}55$ according to "4-color" photometry interpreted through an empirical recipe that he proposed in the associated text and that would apply only to very luminous stars. Thus the high luminosity that was recognized from spectra[25,27] appears to have been corroborated by photometric means. HD 183791 was not observed by *Hipparcos*, so no actual parallax is available. Eggen[30] considered that its 'V_0' magnitude (corrected for interstellar absorption) would be $7^{m}{\cdot}24$; the absolute distance modulus therefore appears to be about 11^{m}, so the parallax ought to be well under a single millisecond of arc.

The paper[27] under Moore & Paddock's names gave the mean radial velocity from their two spectra as +15 km s^{-1} with a 'probable error' of 2·6 km s^{-1}, from which it might be deduced that the individual values differed from that mean by ±3·9 km s^{-1}; but since their dates are unknown they cannot be included in the present investigation — not that the velocities would be of a precision to assist with the orbit in any case. De Medeiros & Mayor[1] reported that they had five velocities, obtained with their *Coravel* and having a mean of +6·01 ± 4·16 km s^{-1}; their implied r.m.s. 'errors' of 9·31 km s^{-1} certainly identified the star as a spectroscopic binary, and (as related in the *Introduction* above) it was after those data were provided individually through the Centre de Données Stellaires that HD 183791 was placed on the observing programme of the Cambridge *Coravel*. There are 45 Cambridge observations, listed in Table II below the five de Medeiros & Mayor measures; the latter have received the usual adjustment of +0·8 km s^{-1} in the interest of systematic homogeneity with the Cambridge observations, which in this case is satisfactorily achieved. The two data sources have been given equal weight in the solution of the orbit, which is illustrated in Fig. 2; the uniformity of phase coverage is assisted by the period being near a half-integral number of years (it is 2·59 years), so seasonal gaps in coverage in one cycle can be made good in the next. The orbit was therefore practically completed in two cycles, in 2002–06, but a few measurements have been made subsequently to patch slight *lacunae* in coverage.

It is, perhaps, a bit surprising that the orbit of such a large star with such a modest period should have an eccentricity as high as the observed 0·4. The most striking thing about the elements is, however, the mass function. If one were to hazard a guess that the observed star has a mass of 4 $M_{\odot}$, then the secondary would have to be at least 2·4 $M_{\odot}$. It therefore could not be a white dwarf — in fact the same is true ($m_2 \gtrsim 1{\cdot}4\ M_{\odot}$) for all primary masses above about 1·5 $M_{\odot}$, which surely must be exceeded in the case of HD 183791. It could be a binary pair itself, or else a main-sequence star of type no later than about A3. In the latter case it can be at most five magnitudes fainter than the primary in V, and much less at short wavelengths. In fact the $(U-B)$ colour index of $0^{m}{\cdot}78$ *could* be held to be a bit 'blue' in comparison with the $(B-V)$ one of $1^{m}{\cdot}15$ — the corresponding values for α Aqr (G2 Ib), for example, are $0^{m}{\cdot}74$ and $0^{m}{\cdot}98$. But in view of the existence of 'cosmic scatter', and the fact that interstellar absorption reddens $(B-V)$ somewhat more than $(U-B)$, the discrepancy cannot be said to be compelling evidence that the companion is a single hot star.

TABLE II

Radial-velocity observations of HD 183791

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

Date (UT)			MJD	Velocity $km\ s^{-1}$	Phase	(O–C) $km\ s^{-1}$
1986	May	31·11*	46581·11	+15·7	$\bar{6}$·502	−0·3
1987	June	14·08*	46960·08	−10·1	$\bar{6}$·903	−0·2
1989	May	14·12*	47660·12	+9·1	$\bar{5}$·642	−0·8
		15·10*	661·10	+9·7	·643	−0·2
		29·11*	675·11	+9·5	·658	+0·4
2002	June	2·07	52427·07	+7·7	0·680	−0·2
	July	15·01	470·01	+5·7	·725	+0·6
	Aug.	14·97	500·97	+3·1	·758	+0·3
	Sept.	10·88	527·88	+0·7	·786	+0·1
		26·88	543·88	−0·1	·803	+0·7
	Oct.	12·82	559·82	−2·0	·820	+0·3
		23·82	570·82	−3·7	·831	−0·3
	Nov.	12·76	590·76	−5·5	·853	−0·2
	Dec.	4·72	612·72	−7·9	·876	−0·3
		18·70	626·70	−9·8	·890	−0·9
2003	Apr.	19·15	52748·15	−2·9	1·019	−0·4
	May	6·10	765·10	+1·5	·037	−0·4
	June	19·07	809·07	+11·7	·083	0·0
		29·03	819·03	+13·2	·094	−0·2
	July	13·02	833·02	+15·8	·109	+0·5
	Aug.	3·99	854·99	+17·8	·132	+0·1
		24·88	875·88	+18·9	·154	−0·3
	Sept.	22·89	904·89	+20·2	·184	−0·3
	Oct.	14·85	926·85	+21·1	·208	0·0
	Nov.	26·72	969·72	+21·4	·253	0·0
2004	Apr.	23·12	53118·12	+19·2	1·410	+0·3
	June	19·03	175·03	+17·6	·470	+0·5
	Aug.	13·00	230·00	+14·6	·528	−0·4
	Sept.	25·92	273·92	+13·4	·574	+0·2
	Oct.	25·80	303·80	+11·9	·606	+0·2
	Nov.	13·77	322·77	+10·7	·626	−0·1
2005	Aug.	17·97	53599·97	−10·6	1·919	+0·5
	Sept.	7·90	620·90	−11·8	·941	+0·2
	Oct.	1·87	644·87	−11·2	·966	+0·2
		25·85	668·85	−8·3	·992	0·0
	Nov.	9·80	683·80	−5·1	2·008	0·0
		29·75	703·75	0·0	·029	+0·1
2006	June	11·06	53897·06	+21·3	2·233	−0·1
	July	13·00	929·00	+21·3	·267	−0·1
	Aug.	15·97	962·97	+21·0	·303	−0·1
		28·98	975·98	+20·7	·316	−0·2
	Sept.	26·91	54004·91	+20·8	·347	+0·4
	Oct.	24·83	032·83	+20·4	·376	+0·7
2007	Oct.	4·92	54377·92	+3·8	2·741	−0·2
2008	July	22·07	54669·07	+5·3	3·049	+0·5
	Aug.	3·06	681·06	+7·5	·061	−0·1
		10·98	688·98	+9·3	·070	0·0

TABLE II (*concluded*)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O – C) km s⁻¹*
2009 July 20·06	55032·06	+17·9	3·432	−0·4
Sept. 12·94	086·94	+15·7	·490	−0·7
2011 Oct. 14·86	55848·86	+21·3	4·295	+0·1

*OHP measure[1] from CDS, weight 1 — see text

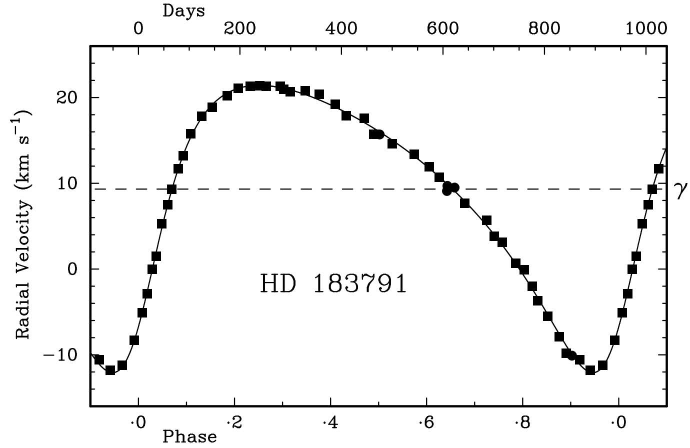

FIG. 2

As Fig. 1, but for HD 183791. In this case the OHP velocities were given full weight.

Radial-velocity traces of HD 183791 show dips that are very noticeably broadened, no doubt in the main by axial rotation of the star. The apparent projected rotational velocity is routinely quantified in the same process that performs the reduction of the traces for radial velocities. The values repeat very well from one trace to another, the r.m.s. spread of the values from individual traces being little more than 1 km s^{-1}. The mean is 13·06, with a formal standard error of only 0·16 km s^{-1}, but owing to the neglect of differences between stars in respect of sources of line-broadening other than rotational velocity the mean value is never claimed to be accurate to better than 1 km s^{-1}. To that accuracy it agrees with the number given by de Medeiros & Mayor[1], 12·3 ± 1 km s^{-1}, from their much smaller number of observations.

Few late-type stars exhibit much in the way of rotational velocity, and in cases where they do the reason is usually 'captured' rotation in a binary-star system. It is tempting to relate the observed $v \sin i$ value to the size of the star and the period of axial rotation. At the eccentricity of HD 183791's orbit, the pseudo-synchronous rotation period is[31] shorter than the orbital period by a factor of 2·1, making it 450 days. To give a projected equatorial velocity of 13 km s^{-1} for rotation in such a period would require the star to have a projected radius of about 115 $R_{\odot}$, which may seem at first thought to be unbelievably gargantuan. If the *DDO* absolute magnitude is accepted, however, then the star has a V luminosity that is $8^{m}{\cdot}7$ (a factor of 3000 times) brighter than the Sun, requiring a surface area *that* much larger, or a radius of $55/\sin i\ R_{\odot}$, if the surface brightness per unit area were equal to the solar value. The latter *proviso* is not met: although the spectral types of the *HD* star and the Sun are both G2, supergiants have lower temperatures than dwarfs of the same spectral type — and no plausible amount of de-reddening would reduce the HD 183791 colour index of $1^{m}{\cdot}15$ to the solar value of $0^{m}{\cdot}67$ — so some very appreciable increase on a 55-$R_{\odot}$ minimum radius would be necessary to offset the lower surface brightness. Thus 115 $R_{\odot}$ does not appear to be entirely beyond the bounds of possibility.

BD +57° 2161

It is somewhat unusual for papers in this series to treat stars that are too faint to possess *HD* numbers, but on this occasion there are two such objects. The sky positions of *BD* stars are partly given away by the fact that their designations include their (epoch 1855) declinations, but their right ascensions are not so apparent. The object described in this section is in the northernmost part of Cygnus, about 1½° south of the 4^{m} star 33 Cyg. Photometry (of not quite the standard of most photoelectric work) was carried out by the then graduate student Vandervort[32], on the 26-inch refractor of the Leander McCormick Observatory, which is on a hill in the outskirts of Charlottesville, Virginia, in observing conditions that were probably less than ideal. His observing programme consisted of a lot of carbon stars that had been discovered on Case objective-prism survey plates by Vyssotsky. For BD +57° 2161 he found $V = 9^{m}{\cdot}60$, $(B-V) = 1^{m}{\cdot}75$. The spectral type, which may have been provided by Vyssotsky, was listed as R0. Such a spectrum is supposed to be basically analogous to that of a late-G giant but with the addition of strong Swan[33] bands of C_2. The colour index of BD +57° 2161 was much redder than the mean for the 20 R0 stars on the programme, by more than three times the 'probable error' of the mean (actually four times, judged from the numbers given). For no better reason than that its colour was like that of certain late-R stars that were also photometrically variable, Vandervort, who made only one observation of it, suggested that BD +57° 2161 might be a variable star, and it has accordingly featured in catalogues of suspected variable stars to this day! No significant variation was noticed in 120 passes by *Hipparcos*, although the mean V and $(B-V)$ values derived from the *Hipparcos/Tycho* photometry, at $9^{m}{\cdot}75$ and $1^{m}{\cdot}61$, differ considerably from those given by Vandervort. (It will be noticed that the discrepancy is actually in V only, and not in B.)

Yamashita[34] included BD +57° 2161 in a table of 'CH-like stars' and assigned it a spectral type of 'C4,1ch'. He was using a classification scheme put forward by Keenan & Morgan[35] in preference to the one used by Vandervort. This is obviously not the place to launch into a major description of carbon stars and their classification, and moreover the writer is not sufficiently versed in the subject himself to do that with any authority, but a little background in the matter may be useful in allowing some comprehension of the development of the literature on the star of immediate concern here. For proper reviews of the subject, with descriptions of many individual objects and progressively improved understanding as time elapsed, the reader may like to consult Bidelman[36], Wallerstein[37], and McClure[38], in addition to the papers cited below. In the 'new' classification scheme[35] the initial C stands for carbon star. The ensuing numeral represents a monotonic temperature classification (which the earlier R/N scheme had proved not to do[35]), wherein C0 to C7 parallels normal types ranging from G4–6 to M3–4; the second numeral is an indication of the strength of the carbon bands*. The concluding 'ch' was intended to represent Yamashita's own contention that BD +57° 2161 is a 'CH-like' star, meaning that the carbon bands are not overwhelmingly strong, the lighter metallic elements are not seriously under-abundant as they are in genuine CH stars, but the heavier elements have the CH-star type of overabundance. There was also a note saying "Ba II λ4554 greatly enhanced". In a subsequent publication[40] by Yamashita plus five co-authors, line-strength indices derived objectively by means of a multi-channel spectrophotometer were tabulated for a number of spectral features; the classification of BD +57° 2161 was slightly changed to C3,1, and the barium enhancement appears not to be so great as the earlier paper[34] had seemed to suggest.

Bartkevičius & Lazauskaitė[41] included BD +57° 2161 in a large list of stars classified through their 'Vilnius photometric system'; they gave its type as C1,1CH, with [Fe/H] = –0·1 and $M_V = -2^{m}{\cdot}4$. They quoted the quantities $E(B-V) = 0^{m}{\cdot}15$: and $M_V = -2^{m}{\cdot}6$ from Sleivytė & Bartkevičius[42].

The *Hipparcos* catalogue gives the parallax of BD +57° 2161 as 0·62 ± 0·87 mas. Knapp, Pourbaix & Jorissen[43] tried to do better by starting afresh from the original data, and reckoned that the best value was 1·10 mas with lower and upper bounds of 0·42 and 2·49 mas. Most recently, the complete re-calculation of all the parallaxes by van Leeuwen[15] gave a result of 0·81 ± 0·84 mas. What the naïve user makes of all that is that the star is a long way away, most probably something of the order of a kiloparsec away, but its parallax is too small to have been measured at all satisfactorily by *Hipparcos*. At such a distance, with allowance for interstellar absorption, its absolute magnitude would be near -1^{m}.

The paper referred to in the Introduction above, by Zăcs *et al.*[2], illustrates parts of the spectrum of BD +57° 2161, and demonstrates not only how the spectrum is cut up by molecular lines of carbon compounds, but also that such abundances as could be determined for the heavier metallic elements, starting from Sr, are systematically enhanced over the solar values by factors ranging from 10 up to nearly 100. The $^{12}C/^{13}C$ ratio is about 10, which in comparison with the Solar System ratio of 89 demonstrates that a lot of material that has been processed in a stellar core has been mixed into the photosphere of the *BD* star. It is perhaps not immediately obvious, however, whether the processing has occurred within the star that is now observed or whether (as in barium stars) it has come from a companion that has now run its evolutionary course and is a white dwarf. Since barium stars in general do not show greatly enhanced carbon abundances although they *do* show lowered $^{12}C/^{13}C$ ratios, the principle of economy of hypotheses ('Occam's razor') points to the photospheric peculiarities of BD +57° 2161 being *sui generis*.

Zamora *et al.*[44], in a paper on the chemical composition of R-type stars, remind the reader that McClure[45] had noted that no R star had been found

*Characteristically, Keenan later became dissatisfied with his own scheme, and substituted another[39]. When photoelectric detectors had extended routine observations into the near infrared, it became apparent that temperature types based on the conspicuous variations in the apparent continuum slope in the blue and visible parts of the spectrum could be falsified by the extraordinarily wide and intense molecular bands found in carbon stars. In Keenan's latest scheme, which includes CH stars, the basic 'carbon' nature of the spectrum is recognized by an initial C, followed by a dash and then one of the letters R, N, or H, assigning the object to one or other of the three major families of carbon-enhanced stars, and then a numeral that represents the temperature type and bears the same relationship to temperature across all three families. Then a luminosity class (if it can be estimated) can follow, and finally a C index that represents a numerical description of the strength of the carbon bands. Unfortunately there does not seem to be any such classification of BD +57° 2161.

to belong in a binary system. At first sight BD +57° 2161 represents an initial counter-example to that 'rule', but in fact it is disqualified from classification as an R-type star (at least in the sense in which that classification was understood by McClure) because of its massive enhancement of *s*-process elements; the elemental idiosyncrasy of R stars is — or at least was — supposed to be largely limited to excessive carbon abundance. Unsatisfactory as it may seem, therefore, it is not immediately clear *what* the classification of BD +57° 2161 ought to be on the latest scheme[39] proposed by Keenan. Almost by a process of elimination of the C-R and C-N possibilities, we are left with C-H, which indeed could be regarded as corroborated by Yamashita's classification of BD +57° 2161 as a 'CH-like' star. Such stars share a characteristic of R and of CH stars in their great over-abundance of carbon, and at the same time share the principal characteristic of barium stars in their great over-abundance of *s*-process elements. The latter attribute prevents their being called R stars except by a deliberate relaxation of the definition of that category, while their excessive carbon abundance disqualifies them as barium stars in the received sense. Furthermore, the 'CH-like' stars cannot be considered to be 'ordinary' CH stars, which as a class exhibit a notably large velocity dispersion, because the former belong to a low-velocity population. It could be objected, therefore, that Keenan's C-H class embraces at least two types of object, kinematically entirely distinct. Keenan's scheme, however, provides for additional characteristics to be appended to the principal classification, so a type could probably be ascribed to BD +57° 2161, without necessarily infringing any 'rule', by a sufficiently well-informed or over-confident operator, although the present writer (abjuring, for different reasons, membership of either category) refrains from hazarding a particular proposal here.

Zăcs *et al.*[2] published the nine radial-velocity measurements of BD +57° 2161 that are listed at the head of Table III. Eight of them were obtained with the Vilnius *Coravel*-type spectrometer[46], apparently used alternatively at its 'home' 1·65-m *Moletai* telescope in Lithuania or else at the Steward Observatory's 1·5-m on Mt. Lemmon in Arizona. They were all obtained in the second half of the year 2003 and do not appear, by themselves, to establish beyond cavil the spectroscopic-binary nature of the star. They are, however, discordant with the velocity measured from the single spectrum, obtained in the previous year, that underlies the principal discussion in the Zăcs *et al.* paper. The rest of the 54 observations shown in Table III were made by the writer in 2004–11 with the Cambridge *Coravel*. There is one previously published radial-velocity measurement of BD +57° 2161 in the literature, given by Yamashita[47]; since no date is given, it cannot be included here, but even if its date were known it could not contribute to the solution of the orbit and would have in any case to be rejected, because it is far outside the range of the velocities admitted in Table III.

In the solution of the orbit, the published measurements[2] were weighted ½ in comparison with the Cambridge ones, and an adjustment of +0·8 km s^{-1} was made to them in an effort to place them on the zero-point[48] normally adopted in this series of papers. The resulting orbit is shown in Fig. 3, and its elements have been added to Table VI below. The eccentricity of the orbit is seen to be some 3½ times its own standard deviation, so its non-zero nature seems quite secure. That is reinforced by a calculation along the lines of the second test described by Bassett[50], which is based on a comparison of the sum of the weighted squares of the residuals of the orbit calculated with the eccentricity (*a*) left as a free variable and (*b*) fixed at zero, which are 6·23 and 8·08 (km $s^{-1})^2$, respectively.

TABLE III

Radial-velocity observations of BD +57° 2161

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

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2002 Nov. 20·00*	52598·00	−55·4	0·827	+0·5
2003 July 19·01*	52839·01	−65·1	1·272	+0·5
19·99*	839·99	−65·0	·274	+0·7
Aug. 12·88*	863·88	−67·4	·318	−0·7
13·90*	864·90	−65·9	·320	+0·8
18·91*	869·91	−66·7	·329	+0·2
Oct. 18·11*	930·11	−67·4	·440	−0·1
Nov. 6·16*	949·16	−66·7	·476	+0·2
10·11*	953·11	−67·3	·483	−0·5
2004 Nov. 12·89	53321·89	−61·8	2·165	0·0
26·77	335·77	−62·0	·190	+0·8
2005 May 28·06	53518·06	−66·1	2·527	−0·2
June 28·05	549·05	−63·8	·585	+0·5
July 18·04	569·04	−62·6	·622	+0·4
Aug. 15·06	597·06	−61·2	·673	−0·1
Sept. 8·96	621·96	−59·0	·719	+0·3
12·99	625·99	−58·6	·727	+0·4
Oct. 27·80	670·80	−55·8	·810	+0·5
Nov. 29·85	703·85	−54·3	·871	+0·7
Dec. 17·80	721·80	−54·6	·904	+0·2
2006 May 17·09	53872·09	−62·8	3·182	−0·3
June 23·08	909·08	−64·9	·250	+0·1
July 21·11	937·11	−66·6	·302	−0·2
Aug. 16·00	963·00	−67·3	·350	−0·1
Sept. 10·98	988·98	−67·2	·398	+0·3
Oct. 24·92	54032·92	−66·8	·479	+0·1
Nov. 9·92	048·92	−66·5	·509	−0·2
Dec. 9·77	078·77	−65·0	·564	−0·1
2007 May 31·09	54251·09	−55·2	3·882	−0·3
July 19·05	300·05	−54·8	·973	+0·4
Aug. 6·98	318·98	−55·4	4·008	+0·5
30·05	342·05	−57·3	·051	−0·1
Sept. 14·97	357·97	−58·4	·080	−0·1
Oct. 4·96	377·96	−60·0	·117	−0·2
21·95	394·95	−61·1	·148	0·0
Dec. 5·85	439·85	−64·5	·231	−0·1
2008 Jan. 17·75	54482·75	−66·9	4·311	−0·3
July 22·08	669·08	−61·6	·655	+0·2
Aug. 11·03	689·03	−61·0	·692	−0·7
Sept. 12·00	721·00	−58·5	·751	−0·4
Oct. 1·90	740·90	−57·1	·788	−0·2
31·84	770·84	−56·2	·843	−0·7
Nov. 25·84	795·84	−54·7	·889	+0·1
Dec. 26·72	826·72	−55·4	·947	−0·5
2009 Aug. 21·04	55064·04	−67·7	5·385	−0·2
Sept. 25·92	099·92	−67·3	·452	−0·1
2010 June 27·08	55374·08	−54·9	5·958	+0·1
July 30·07	407·07	−56·3	6·019	−0·1
Aug. 30·04	438·04	−58·2	·077	0·0

TABLE III (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2010 Sept. 14·99	55453·99	−59·3	6·106	0·0
Oct. 4·97	473·97	−61·3	·143	−0·4
Nov. 10·84	510·84	−63·6	·211	0·0
2011 Sept. 12·97	55816·97	−57·5	6·777	−0·2
Oct. 15·89	849·89	−55·8	·838	−0·2

*Measure by Zăcs *et al.*[2], weight ½ — see text

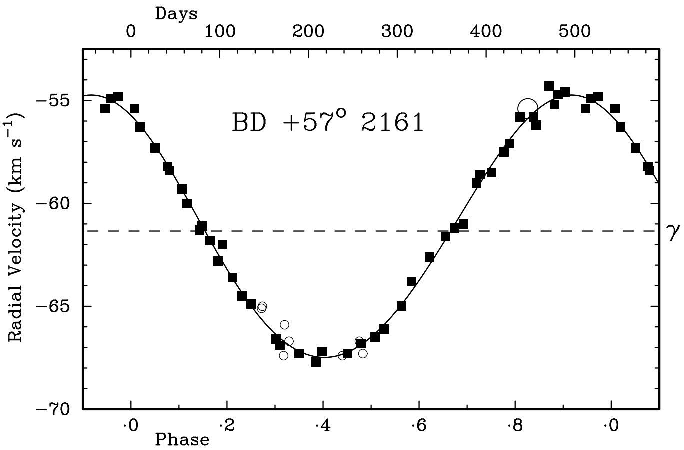

FIG. 3

As Fig. 1, but for BD +57° 2161. Here there are no OHP velocities; the open circles plot those published by Zăcs *et al.*[2]. The small circles denote those that came from their *Coravel*-type spectrometer, while the single large circle shows the velocity that was derived from their observation of the complete spectrum; all were given half-weight. An early version of this diagram, with zero eccentricity, was displayed in a poster paper by Zăcs *et al.* at a conference in 2008; the poster does not appear in the conference proceedings[49] but is accessible on the Web.

The former number shows that the 48 degrees of freedom left after fitting six orbital elements to the 54 observations cost 0·13 (km s^{-1})^2 each, while the extra two that are gained by 'freezing' e and ω cost 0·925 each. The ratio 0·925/0·13 of those costs yields $F_{2,48} \sim 7{\cdot}1$, which is well beyond the 1% level of significance ($F \sim 5{\cdot}08$) — in fact its probability of chance occurrence for a truly circular orbit is about 0·2%; moreover, the circular fit to the points *looks* to be bad in a systematic way.

The orbit of BD +57° 2161 could very well pass for that of a barium star, with its moderate period and eccentricity, and the star certainly has the sort of abundance excesses of *s*-process elements that distinguishes barium stars. It differs from their category, however, in its additional large carbon over-abundance, which warrants our sympathy with Yamashita's characterization of it as a 'CH-like' star. It would be of particular interest to know whether its unseen companion — which need not have a remarkably low absolute luminosity to be invisible, either directly (at the order of 1 arc-ms angular separation!) or spectroscopically, against the glare of the carbon star — is a white dwarf or just a lower-main-sequence star awaiting its own turn to shine as a leading light on the evolutionary stage.

BD +34° 4216

This star, being slightly brighter than ninth magnitude, seems a bit unlucky not to have been honoured with an entry in the *Henry Draper Catalogue*. It is to be found about 3° north-following ε Cyg, a 3^m long-period binary system that is continuing to follow quite well the orbital-velocity ephemeris that was suggested[51] for it some years ago on the basis of very fragmentary information.

BD +34° 4216 was recognized as a composite-spectrum system by Stephenson & Nassau[52] from an objective-prism spectrogram, which is actually illustrated directly in their paper. The recognition of composite spectra, and certainly the classification of the components, largely depended on the ability to see the spectrum both in the blue and in the ultraviolet beyond the *H* and *K* lines and past the Balmer limit. Stephenson & Nassau were at the Warner & Swasey Observatory of the Case Institute of Technology (as it was then — now Case Western Reserve University) and must have been familiar with the advantages of having spectra going far down towards the atmospheric cut-off from experience with their own *Burrell Schmidt*[26], which had a 'Vita glass' corrector plate and a 4°·5 UV-transmitting objective prism giving a dispersion of 580 Å mm^{-1} at Hγ. But possibly on account of dissatisfaction with the resolution of their own telescope, which was reported — *after* its mirror had been re-figured — to have suffered from astigmatism, they came to an agreement with the Hamburg Observatory for a joint project to survey the northern Milky Way fields for high-luminosity and other interesting stars. The Case observers undertook to search the region of Galactic longitude 46°–80° on objective-prism plates taken by Hamburg observers with *their* 80-cm Schmidt with its UV-transmitting optics. It took the present writer a little while to realize why the circumstances just outlined seemed not to explain how BD +34° 4216, at $l \sim 35°$, came to be discovered by Case observers on Hamburg plates; the answer lies in the alteration, at just that time, in the definition[53] of the zero-point of Galactic longitude so as to put the zero at the Galactic Centre, which had the effect of reducing all such longitudes numerically by about 33°.

The several objective-prism spectra illustrated in Stephenson & Nassau's paper bring home to the reader the degree of familiarity and expertise that must be necessary to classify anything from them! It is clear from the accompanying text that those authors did in fact feel quite at home with such spectra and felt able to classify them with some confidence — even ones where two spectra were superposed, as in the case of present interest, where they gave the types as B8 and G2. Seventeen years later than Stephenson & Nassau's paper[52], BD +34° 4216 featured also in another list of "peculiar stars", noticed among spectra obtained with an 8° objective prism on the 70-cm Maksutov at Abastumani (166 Å mm^{-1} at Hγ) by Radoslavova[54], who gave the impression that it was a fresh discovery and classified it as A + G. Later still, Naftilan & Milone[3] wrote a whole (if short) paper entirely dedicated to the star. It starts straight out by saying, "The star BD +34° 4216 was noted by Milone *et al.* (1979) as having a composite spectrum", which (in the absence of any reference either to Stephenson & Nassau or to Radoslavova) the naïve reader would take as implying that they *discovered* it.

The only interest that Milone *et al.*[55] actually had in BD +34° 4216 was as a photometric comparison star for the variable star CG Cyg, a 0·63-day β Lyr eclipsing system only about 11′ away from it. All that they said about BD +34° 4216 is that Milone[56] had obtained *UBV* photometry* of $V = 8^{m}{\cdot}969$, $(B-V) = 0^{m}{\cdot}744$, $(U-B) = 0^{m}{\cdot}231$, and they remarked *en passant* that it "possesses an interesting composite spectrum", although they did not indicate how they knew that, and the colour indices would be quite appropriate to a single G5 V star. Naftilan & Milone[3] obtained six spectra of BD +34° 4216, and also a spectrophotometric scan of the star and analogous scans of certain single stars for comparison. After concluding from the spectra that the types were approximately B9·5 V and K0 III, they showed plots of flux against wavelength. One of them showed that in the red and near-infrared the flux of the composite system matched that of HR 5616 (whose type of K2 III is repeatedly misprinted as KS III), but shortward of λ4400 Å they *said* that it was a good match with θ Vir (A1 V), although, from the plot, that is not at all apparent to the writer. After explaining that their spectra were hardly up to the task of determining radial velocities, they said that the spectral lines were apt to appear double, the pairs being of unequal strength. They listed the average velocities from the main lines for five plates — the first being at a dispersion of 120 Å mm^{-1}, then two at 30 Å mm^{-1}, and finally two (image-tube) ones at 15 Å mm^{-1}. In four cases they gave also a second velocity from the weaker components of pairs of lines. Their results are included at the head of Table IV here. It seems a bit unlikely, however, that the strongest lines in spectra so dissimilar as those supposed to constitute BD +34° 4216 would be the same (corresponding) lines in both spectra and therefore appear double in spectra taken at phases away from conjunctions.

Out of 23 papers listed for BD +34° 4216 in the *Simbad* bibliography, most refer to the star only as a comparison star for CG Cyg. In fact no fewer than 14 of them actually have 'CG Cyg' explicitly in the title, and five others (mostly referring to *groups* of variable stars) are in truth interested only in CG Cyg and not in BD +34° 4216. There are only four papers in the bibliography that are really concerned with the latter object itself; in the references here they are nos. 3, 52, and 54 (refs. 55 and 56 are not retrieved by *Simbad*), and the only one to which reference has not been made so far is another paper by Radoslavova[57], reporting another classification of BD +34° 4216 as A0 + G as well as the previous A + G.

It is the rule rather than the exception for classifications made directly from composite spectra to place the late-type component much too early, so there is no hesitation in following more nearly the results of the spectrophotometric flux observations[3]. Table V illustrates (with colour indices from Schmidt-Kaler[58]) how a combination of a late-type giant that has an absolute magnitude of $-0^{m}{\cdot}5$ and the colour indices of K0 III, plus a B9 V star, yields colour indices comparable with those of BD +34° 4216, allowing for a certain amount of reddening. The apparent distance modulus implied by the combined absolute magnitude in conjunction with the observed magnitude of the *BD* star is nearly ten magnitudes; after correction for the absorption implied by the reddening allowance in Table V, the distance should be about 800 pc — a parallax of about 0″·0012.

*The quantities of $0^{m}{\cdot}002$ or $0^{m}{\cdot}003$, to which they refer as 'mean square errors of the means' are presumably *root*-mean-square uncertainties.

TABLE IV

Radial-velocity observations of BD +34° 4216

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

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
1973 June 14·36*	41847·36	–16·0	$\overline{18}$·516	–12·9
14·36†	847·36	+13·0	·516	—
1977 July 27·29*	43351·29	+7·0	$\overline{16}$·911	–3·7
27·29†	351·29	–12·0	·911	—
Oct. 20·19*	436·19	–18·0	$\overline{15}$·046	–14·8
1978 Aug. 17·28*	43737·28	–26·0	$\overline{15}$·526	–23·2
17·28†	737·28	+27·0	·526	—
Oct. 11·71*	792·71	–18·0	·614	–18·5
11·71†	792·71	–4·0	·614	—
2004 Aug. 19·97	53236·97	+2·8	0·657	+0·6
Oct. 25·89	303·89	+6·5	·764	–0·2
Dec. 11·07	350·07	+9·4	·837	–0·2
2005 May 28·07	53518·07	–8·9	1·105	–0·1
June 23·05	544·05	–10·3	·146	+0·2
July 18·05	569·05	–11·4	·186	–0·4
Aug. 15·08	597·08	–10·1	·231	+0·7
Sept. 16·96	629·96	–9·7	·283	+0·2
Nov. 17·83	691·83	–8·0	·382	–0·6
Dec. 10·79	714·79	–6·0	·418	+0·3
2006 July 13·09	53929·09	+7·3	1·760	+0·8
Aug. 10·06	957·06	+8·7	·804	+0·3
Sept. 8·99	986·99	+10·1	·852	0·0
Oct. 5·00	54013·00	+10·6	·893	–0·2
Nov. 3·84	042·84	+9·9	·941	+0·3
23·79	062·79	+6·4	·972	–0·5
Dec. 2·79	071·79	+5·2	·987	0·0
9·78	078·78	+3·9	·998	+0·2
16·78	085·78	+1·9	2·009	–0·2
28·78	097·78	–0·9	·028	–0·2
2007 Jan. 10·76	54110·76	–2·8	2·049	+0·7
July 8·08	289·08	–8·5	·333	+0·2
25·07	306·07	–7·6	·360	+0·4
Sept. 22·95	365·95	–4·9	·455	+0·2
Oct. 20·93	393·93	–3·5	·500	+0·2
2008 Jan. 7·77	54472·77	+0·2	2·625	–0·7
July 13·09	660·09	+11·0	·924	+0·7
Aug. 11·04	689·04	+7·4	·970	+0·2
Sept. 13·96	722·96	+0·1	3·024	+0·2
Oct. 8·92	747·92	–5·4	·064	–0·1
18·94	757·94	–6·9	·080	0·0
27·89	766·89	–8·4	·094	–0·3
Dec. 9·81	809·81	–10·7	·162	+0·1
2009 Aug. 12·01	55055·01	–1·9	3·553	–0·1
Sept. 4·00	078·00	–1·1	·589	–0·6
Oct. 8·91	112·91	+1·7	·645	0·0
Nov. 7·87	142·87	+3·5	·693	–0·1
23·78	158·78	+4·2	·718	–0·5
Dec. 6·77	171·77	+5·8	·739	+0·2

TABLE IV (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)* *km s⁻¹*
2010 Jan. 5·74	55201·74	+7·5	3·787	−0·1
June 23·08	370·08	−4·1	4·055	+0·1
Aug. 6·05	414·05	−10·1	·125	−0·3
Oct. 4·98	473·98	−11·6	·220	−0·7
2011 Nov. 27·87	55892·87	+10·4	4·887	−0·3
2012 Jan. 12·73	55938·73	+7·4	4·960	−0·7

* Measure by Naftilan & Milone[3], weight 0
† Ditto, but of other component — see text

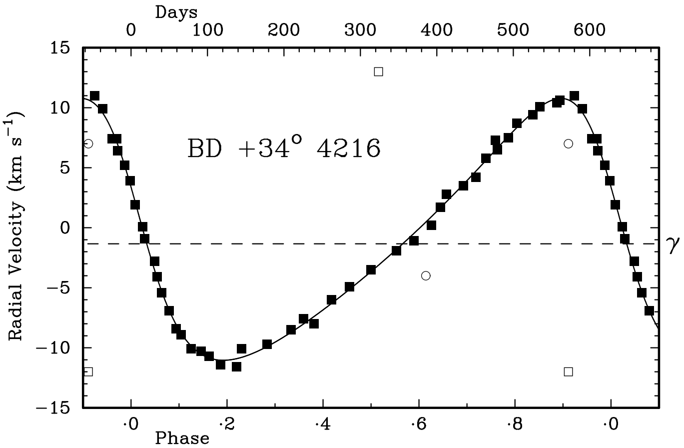

FIG. 4

As Fig. 3, but for the composite-spectrum system BD +34° 4216. Here the open symbols represent radial velocities (zero-weighted in the orbit) published by Naftilan & Milone[3]. The circles are the ones that might be expected to fall near the velocity curve, which refers to the late-type component of the system; there are in principle five of them but only two are within the range of velocities included in the diagram. The open squares should represent the velocities of the early-type companion and lie near a velocity curve that is like the plotted one mirrored in the γ-velocity line but with probably a mildly different amplitude.

TABLE V

Photometric model (absolute magnitudes, colour indices) for BD +34° 4216

Star	M_V *m*	*(B–V)* *m*	*(U–B)* *m*	M_B *m*	M_U *m*
Model { K0 III	−0·5	1·07	0·90	0·57	1·47
Model { B9 V	0·5	−0·07	−0·20	0·43	0·23
Model { K0 + B9	−0·86	0·66	0·13	−0·20	−0·07
+34° 4216 (observed)	8·97*	0·74	0·21		

**Apparent* magnitude

The only radial velocities to be found in the literature for BD +34° 4216 are those of Naftilan & Milone[3]. The other 45 observations in Table IV represent Cambridge measurements of, doubtless, the late-type component of the system. The 'equivalent widths' of the dips in radial-velocity traces, in comparison with those given by single early-K giants, suggest that approximately half of the light from the system, in the wavelength region (~*B*) utilized by the *Coravel*, comes from the late-type star. That accords with what one might judge from Naftilan & Milone's Fig. 1A, that illustrates the spectrophotometric flux scans of the composite spectrum and of single stars that may approximate to the spectral types of the individual components.

The orbit is readily solved from the Cambridge observations. It is shown in Fig. 4, and its elements appear in the last column of Table VI. Most of Naftilan & Milone's radial velocities fall outside the confines of Fig 4 and bear little relationship to the orbit. Only the second pair of their velocities looks at all plausible. It is clear from their paper, however, that those authors themselves had very little faith in their velocities. Their only real conclusion from them was that it "is clear that real velocity variations do indeed exist, confirming the binary nature of BD +34° 4216." Even *that* might now seem to be an over-statement, because the mean-square deviation of their five measures of the principal lines from the now-known velocities of the late-type star at the relevant times is almost as great as the mean-square deviation of the five velocities from their own mean.

TABLE VI

Orbital elements for the four stars

Element	*HD 180660*	*HD 183791*	*BD +57° 2161*	*BD +34° 4216*
P (days)	180·483 ± 0·011	946·3 ± 0·4	541·0 ± 0·8	627·8 ± 0·6
T (MJD)	52976·64 ± 0·25	52730·3 ± 1·4	54315 ± 25*	54707·9 ± 2·1
γ (km s^{-1})	−18·39 ± 0·04	+9·33 ± 0·06	−61·34 ± 0·05	−1·34 ± 0·07
K_1 (km s^{-1})	17·07 ± 0·05	16·75 ± 0·10	6·37 ± 0·07	10·91 ± 0·09
e	0·2831 ± 0·0025	0·408 ± 0·004	0·043 ± 0·012	0·342 ± 0·007
ω (degrees)	116·6 ± 0·6	227·1 ± 0·8	32 ± 17	71·2 ± 1·6
$a_1 \sin i$ (Gm)	40·63 ± 0·13	199·0 ± 1·2	47·4 ± 0·6	88·5 ± 0·8
$f(m)$ ($M_\odot$)	0·0822 ± 0·0008	0·351 ± 0·006	0·0145 ± 0·0005	0·0702 ± 0·0019
R.m.s. residual (wt. 1) (km s^{-1})	0·23	0·37	0·34	0·39

*For BD +57° 2161 T_0 = 54266·1 ± 1·0

Without knowing a good deal more about the system than we do at present, it is not possible to interpret the mass function with any confidence. If the late-type component of the binary is indeed something like a K0 III star it might be expected to have a mass near 2 $M_\odot$, and then the mass function would merely set for the other star a minimum mass of a little over 0·8 $M_\odot$ — something that is already obvious. Then, since the companion star is thought to be a late-B object, which could be expected to have a mass near 3 $M_\odot$, at first sight the function could be used to estimate the orbital inclination. But that calculation would introduce the *non sequitur* of supposing that the star that was evolving is less massive than its supposedly main-sequence companion, implying either that it has lost a lot of mass already in the course of its evolution, or else that we must have been mistaken over the mass of the late-type star, which must be considerably more massive than was originally estimated. There does not appear to be much future in discussing the issue further in the absence of a more comprehensive investigation of the BD +34° 4216 system than can be reported here.

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COOL DWARFS IN WIDE MULTIPLE SYSTEMS

PAPER 2: A DISTANT M8·5V COMPANION TO HD 212168 AB

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DENIS J222644·3–750342 is a nearby, very-low-mass, M8·5V-type star that has been repeatedly targeted by kinematic and activity studies. Thought to be an isolated star, it is actually a wide common-proper-motion companion at 265 arcsec from the bright G0 V *Hipparcos* star HD 212168. The third member in the trio is CD –75° 1242, a poorly investigated K dwarf. We confirm the physical binding of the triple system, which we call Koenigstuhl 5, by compiling common radial velocities and proper motions and measuring constant angular separations and position angles between components A, B, and C, over very long time baselines (A–B: 114 yr; A–C: 22 yr). With about 0·095 $M_{\odot}$, the M8·5 V star at 6090 AU to the G0 V primary is one of the least-bound, ultracool dwarfs in multiple systems.

Koenigstuhl 5 ABC: two Sun-like Hipparcos stars with an M8·5 V companion

We continue the series of works devoted to investigating cool dwarfs in wide multiple systems started by Caballero[1]. Here we present a new system containing a pair of relatively bright Sun-like stars and a very-low-mass M8·5 V star 4′·4 southeast. It was found during a kinematic investigation of ultracool dwarfs in young moving groups[2], but has not been formally reported yet.

The central pair, formed by the stars HD 212168 and CD –75° 1242, was discovered by Dunlop[3] in 1826 and received the *Washington Double Star Catalogue* (*WDS*[4]) code and number Dun 238. Although the *WDS* remarks that "proper-motion or other technique indicates that this pair is physical" and Raghavan *et al.*[5] added that the two stars in the system "[match] proper-motion and photometric distance", there has been no consensus on the actual binarity of the pair. Sinachopoulos[6] found that the B component has Strömgren *uvby* colours that were much too red for its spectral type, thought to be "G0– V", from which Gray *et al.*[7] concluded that "since 'A' is also a G0 dwarf and is about 2^{m}·7 brighter, this is undoubtedly an optical pair".

In Table I, we compile the basic astrophysical data for HD 212168 and CD –75° 1242 from a number of sources (refs. 7–19). While the observed parameters match A being an inactive, early G-type star, non-Strömgren (*i.e.*, Johnson, Geneva) photometry also suggests a later spectral type for B, which we estimate to be an early or intermediate K dwarf (or, more accurately, K3–4: V, where the colon stands for uncertain spectral type). The spectra of both stars available at the NStar Spectra Homepage (http://stellar.phys.appstate.edu/) appear basically the same, except for what seems to be a normalization problem redwards of 470 nm. Gray *et al.*[7] probably made a book-keeping slip

TABLE I

Basic data of components A and B

Datum	*A*	*B*	*Origin: refs.*
Name	HD 212168	CD –75° 1242	*Simbad*
HIP	110712	110719	8
α (J2000)	$22^h\ 25^m\ 51^s \cdot 16$	$22^h\ 25^m\ 56^s \cdot 40$	8
δ (J2000)	–75° 00′ 56″·5	–75° 00′ 52″·8	8
d [pc]	23·0±0·3	18±2 (23·0±0·3)	8
U [mag]	6·82	…	13
B_T [mag]	6·839±0·015	10·134±0·027	9
B [mag]	6·68	9·82	13, this work
V_T [mag]	6·187±0·010	8·845±0·015	9
V [mag]	6·04	8·73	13, this work
R_F [mag]	5·78	8·10	10
I_N [mag]	5·48	7·55	10
J [mag]	5·262±0.276	6·559±0·029	11
H [mag]	4·768±0.021	5·937±0·021	11
K_s [mag]	4·705±0.016	5·809±0·023	11
$b–y$ [mag]	0·374	0·674	14
m_1 [mag]	0·197	0·555	14
c_1 [mag]	0·357	0·116	14
β [mag]	2·614	2·520	14
v_r [km s^{-1}]	+13·2±0·7	+13±2	16, 12
U,V,W [km s^{-1}]	+2, –8, –12	(+2, –8, –12)	18
T_{eff}	5940±80	…	19
log g	4·31	…	19
[Fe/H]	–0·05	…	19
log R'_{HK}	–4·89 to –4·97	…	15, 17
Sp. Type	G0 V	K2-3: V	7, this work
τ(Gyr)	6^{+2}_{-3}	1 to 10	19, this work
M_V	4·22±0·03	6·92±0·03	This work
M [$M_\odot$]	1·10±0·10	0·80±0·10	This work

in observing the star: since the two stars have *Hipparcos* numbers several units apart, they could have observed them on separate occasions, and mistakenly observed the primary twice, obtaining essentially the identical spectral type from both spectra. CD –75° 1242 was classified as type G5 in the *Cape Photographic Catalogue*, where non-Henry Draper types were provided by Margaret Mayall from Harvard objective-prism plates. This usually implies a modern Morgan–Keenan type of K0, and the overlapping spectra probably led to the type being estimated too early. Given the brightness of CD –75° 1242, of $V = 8^{m}\!\cdot\!7$, about $2^{m}\!\cdot\!7$ fainter than HD 212168, it should be easy to obtain a new mid-resolution spectrum from the southern hemisphere to solve this dilemma.

The primary, HD 212168, has no low-mass, close companions, based on deep adaptive-optics imaging with *NACO*[20] and on accurate, long-term radial-velocity monitoring[21], nor flux excess in the mid-infrared[22].

The *Hipparcos*-based parallax of CD –75° 1242 is relatively poor because of interference from the companion (a common problem in the *Hipparcos* catalogue). However, both HD 212168 and CD –75° 1242 have the same radial velocity within the uncertainties. But the definitive confirmation that they form "undoubtedly a *physical* pair" comes from a simple astrometric analysis consisting of measuring the angular separation between the two components with a very long time baseline. We compile all available astrometric epochs in Table II. We derived angles from refs. 8, 9, 11, and 23 and took observed angles from the original van Albada-van Dien works[24–26]. The remaining epochs were

TABLE II

Astrometric observations of the AB pair (Dun 238 AB)

Epoch	ρ *(arcsec)*	θ *(deg)*	*Origin*
1826	14	90	3
1835·73	25	83·9	27
1836·24	18·09	82·9	27
1882·785	19·2	83	35
1892·78	20·134	82·6	23
1893·707	19·9±0·5	80·4±0·2	23
1894·73	20	85	28
1894·74	19·713	79·9	23
1917·92	20·12	81·2	29
1940·60	20·2	79·8	30
1947·80	20·532	80·8	31
1956·19	20·501	81·4	32
1958·567	20·419	80·25	33
1971·49	20·478	80·1	34
1975·514	20·48±0·01	80·15±0·04	24
1976·462	20·451±0·017	80·30±0·07	25
1983·640	20·695±0·007	79·7026	
1991·250	20·6±0·4	79·6±0·2	3
1991·500	20·5979·8	9	
1999·930	20·75±0·13	79·6±0·2	11
2007·0	20·71	80·3	36

either tabulated by the *WDS* catalogue (refs. 3, 23, 27–34) or kindly provided by the anonymous referee (refs. 35 & 36). For some unknown reason, two Deep Near-Infrared Southern Sky Survey (DENIS[37]) astrometric epochs in 1999·81 smeared the results and were not used.

A total of 114·2 years elapsed between the first and last reliable astrometric epochs in Table II (1892·78 and 2007·0). If only the most accurate measurements after 1945 are taken into account, the mean angular separation and position angle turn out to be $\langle\rho\rangle = 20''\cdot56 \pm 0''\cdot11$, $\langle\theta\rangle = 80^\circ\cdot2 \pm 0^\circ\cdot5$, showing CD –75° 1242 to be fixed relative to the primary. The large scatter in position angle is because some authors (*e.g.*, van Albada-van Dien[24–26]) reported measurements at-epoch (*i.e.*, not precessed to a common equinox), rather than for J2000, which is the case for the other measurements shown. Besides, small time variations of the order of what is observed in ρ and θ are expected from two Sun-like stars separated by slightly less than 500 AU. Our astrometric follow-up covered approximately 1·5% of a seven-millenia orbit.

The third component in the system, DENIS J222644·3–750342, was discovered and first investigated by Phan-Bao *et al.*[38,39]. The star turned out to be an M8·5 dwarf with faint Hα emission[40,41,42,43], relatively large rotational velocity[42], and apparently single and isolated from any other star. See Table III for a compilation of astrophysical parameters of the ultracool dwarf.

While the radial velocity of DENIS J222644·3–750342 measured by Reiners & Basri[44] matches within 1σ with the one measured for HD 212168, reported heliocentric distances are significantly less than for the G0 V star, for which there is a parallax measurement by *Hipparcos*. In particular, different distances in the interval from 15·9 to 19·4 pc have been estimated from spectral-type–magnitude relationships by virtually all authors that have investigated the ultracool dwarf[39–43,45,46], with most estimations around 17 pc. Being a companion to HD 212168 AB translates into being located significantly further, at 23·0 ± 0·3 pc.

TABLE III

Basic data of component C

Datum	*C*	*Origin*
Name	DENIS J2226443–750342, Koenigstuhl 5 C	19, this work
α (J2000)	$22^h 26^m 44^s{\cdot}41$	11
δ (J2000)	$-75° 03' 42''{\cdot}5$	11
d [pc]	15·9 to 19·4 (23·0±0·3)	Various authors
$\langle R_F \rangle$ [mag]	18·7	10
$\langle i \rangle$ [mag]	15·21	37
I_N [mag]	15·05	10
J [mag]	12·353±0·023	11
H [mag]	11·696±0·027	11
K_s [mag]	11·246±0·023	11
v_r [km s^{-1}]	+14·7±3·0	24
M_J	10·54±0·03	This work
Sp. type	M8·5 V	Various authors
pseudoEW(Hα)	−0·6 to −7·1	Various authors
M [$M_\odot$]	0·095±0·005	This work

Does DENIS J222644·3–750342 have the same proper motion as HD 212168? First, we have to solve a mess related to proper motions of the three stars involved, as illustrated by Table IV: (*i*) As seen above, HD 212168 and CD –75° 1242 have (roughly) the same proper-motion, in spite of the large proper-motion uncertainties in the re-reduced *Hipparcos*[8] data and, especially, in Tycho-2[9], USNO-B1·0[10], and the original *Hipparcos* data[47], which we do not list in order to avoid confusion. (*ii*) The reference for the proper motion of DENIS J222644·3–750342 that Faherty *et al.*[46] used for their Brown Dwarf Kinematics Project is the *NLTT Catalogue*[48], but they actually took it from Phan-Bao *et al.*[39]. (*iii*) *Simbad* lists only the proper motion of DENIS J222644·3–750342 from Schmidt *et al.*[41], which has very large uncertainties and is quite different from the one from Phan-Bao *et al.*[39]. (*iv*) The proper motion from Phan-Bao *et al.*[39] resembles the ones from USNO-B1.0 and Positions and Proper-motions-Extended Large (PPMXL[49]), which in turn look like the re-reduced *Hipparcos* proper-motion of HD 212168.

We used 2MASS, DENIS, and the SuperCOSMOS[50] digitizations of the photographic plates from the *United Kingdom Schmidt Telescope* (*UKST*) and European Southern Observatory red plate (ESO Red) as in Caballero[51] for measuring a new accurate proper motion of DENIS J222644·3–750342. In particular, we used seven astrometric epochs spaced by 22·15 yr (the ones listed in Table V plus another DENIS epoch on 1999 August 28). In contrast, previous

TABLE IV

Proper-motions of components A, B, and C

Component	$\mu_\alpha \cos\delta$ (*mas yr*$^{-1}$)	μ_δ (*mas yr*$^{-1}$)	*Reference*
A	+57·79±0·50	+12·25±0·39	3
B	+67·78±5·59	+9·09±4·82	3
C	+46±10	+22±10	10
C	+60·6±9·3	+16·6±9·3	29
C	+48±19	+14±19	19
C	+20±55	+46±24	21
C	+53·4±0·9	+6·5±0·7	This work

TABLE V

Astrometric observations of the AC pair (KO 5 AC)

Epoch	*ρ (arcsec)*	*θ (deg)*	*Origin*
1977 Oct. 13	264·9±0·2	128·8±0·2	UKST Blue
1984 Oct. 12	264·8±0·2	128·9±0·2	ESO Red
1993 Sep. 25	264·8±0·2	128·8±0·2	UKST Infrared
1996 Oct. 10	264·7±0·2	128·9±0·2	UKST Red
1999 Oct. 24	264·9±0·1	128·9±0·2	DENIS
1999 Dec. 07	264·71±0·13	128·8±0·2	2MASS

authors had used only part of our dataset (*e.g.*, Schmidt *et al.*[41] used *UKST* plates of the Digitized Sky Survey and 2MASS). To sum up, the USNO-B1·0 and PPMXL measurements and ours, (+53·4 ± 0·9, +6·5 ± 0·7) mas yr^{-1}, are consistent with HD 212168 AB and DENIS J222644·3–750342 having a common proper motion (Table IV).

To confirm our assumption, we studied the constancy of the angular separation and position angle between HD 212168 and DENIS J222644·3–750342. As shown in Table V, ρ and θ are indeed constant with r.m.s. deviations of only 0″·10 and 0°·05 over six astrometric epochs across 22·15 yr. The proper-motion of the system is large enough that there would be significant change in ρ and θ over the 22-year baseline of the observations if the two stars were not in fact linked. At this point, we conclude already that the three stars, HD 212168 (G0 V), CD –75° 1242 (K3–4: V), and DENIS J222644·3–750342 (M8·5 V) form a triple system that we called Koenigstuhl 5 AB–C (KO 5), following the nomenclature introduced by Caballero[1,52].

In Tables I and III we provide masses of CD –75° 1242 and Koenigstuhl 5 C derived from photometry and theoretical models[53,54] assuming that they

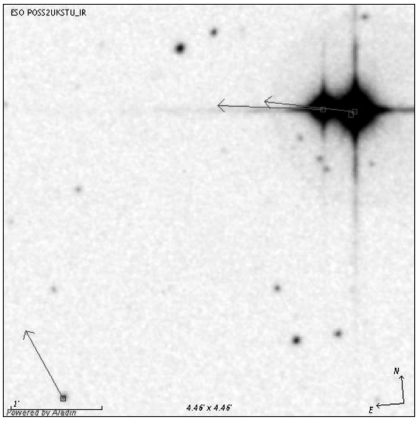

FIG. 1

Colour-inverted *UKST* I_N-band image provided by *The Digitized Sky Survey* from the European Southern Observatory at Garching constructed with *Aladin* showing the components A (HD 212168; top right corner, western star), B (CD –75° 1242; top right corner, eastern star), and C (DENIS J2226443–750342, Koenigstuhl 5 C; bottom left corner). The long arrows indicate the proper-motions as tabulated by *Simbad*. Note the incorrect proper-motion of component C as measured by Schmidt *et al.*[41]. The field of view is 4′·46 square and the orientation is indicated on the image.

are located at the same distance as HD 212168 and have a Sun-like age and metallicity. Although the primary *UVW* Galactic space velocities derived by Holmberg *et al.*[18] are consistent with membership in the Ursa Major moving group ($\tau \sim 0{\cdot}3$ Gyr)[55] and in the young disc ($\tau \leq 1$ Gyr), no features of youth have been identified up to now in any of the three components. See also Table I for the age estimated for the primary by Casagrande *et al.*[19]. Because of the dwarf class of the primary, its solar metallicity ([Fe/H] = −0·05) and low vertical Galactic velocity component ($W = -12$ km s^{-1}), the system cannot be extremely old (*i.e.*, $\tau > 10$ Gyr).

At $d = 23{\cdot}0 \pm 0{\cdot}3$ pc, Koenigstuhl 5 C seems to be over-luminous with respect to the spectral-type–magnitude relationships used previously. The star appears single at the resolution of the 2MASS images, where an equal pair with even ~1″ separation would be obvious. This does not rule out a closer binary.

Koenigstuhl 5 C is located at a projected physical separation to the primary of over 6000 AU. This separation is not extraordinary (see, *e.g.*, recent works by Caballero[56,57] and Shaya & Olling[58]), but it is considerable for an ultracool dwarf of $0{\cdot}095 \pm 0{\cdot}05\ M_\odot$ close to the substellar boundary (Table VI). There are few comparable systems, V1054 Oph ABC + GJ 643 + vB 8 (a group of five M dwarfs including an M7 V star at 1500 AU to the triple primary[59]), η CrB AB–C (an L8 V brown dwarf at 3600 AU of a G1 V + G3 V pair[60]), and Koenigstuhl 3 A–BC (an M8 V + L3 V pair at 11900 AU to an F8 V[61]) being perhaps the most representative ones. Koenigstuhl 5 AB–C, with an approximate binding energy of -54×10^{33} J if the three components are taken into account, is one of the least-bound systems known (see, *e.g.*, Fig. 2 in Caballero[56]). Our triple system continues the Koenigstuhl series of wide multiple systems with ultracool dwarfs that represent a challenge for formation scenarios and stability models of fragile systems as they travel across the Galactic disc. Besides, the hypothetical overluminosity of Koenigstuhl 5 C could be explained by unresolved equal binarity and, thus, our system would be a hierarchical quadruple system with roughly twice the estimated binding energy. Dedicated high-resolution imaging of the system components would be of great interest.

TABLE VI

Basic data of the AB and AC pairs

Pair	*A–B* (*Dun 238 AB*)	*A–C* (*KO 5 AC*)
$\langle\rho\rangle$ [arcsec]	20·56±0·11	264·79±0·10
$\langle\theta\rangle$ [deg]	80·2±0·5	128·84±0·05
d [pc]	23·0±0·3	
s [AU]	476±6	6090±80
M_{total} [$M_\odot$]	1·90±0·14	1·20±0·10
$-U^{\star}_{g}$ [10^{33} J]	3300±300	30±3

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CORRESPONDENCE

To the Editors of 'The Observatory'

Which Law is Hubble's Law?

In her reply to Nussbaumer & Bieri[1], Trimble[2] writes of "the linear velocity–distance (or redshift–magnitude) relationship that we now call Hubble's Law" and of "the redshift–magnitude, velocity–distance, *etc.*, relationships". Perhaps some of the debate over whom the Law should be named after is due to the fact that different people use the term 'Hubble's Law' to mean different things. Harrison[3] discusses the distinction between what he calls the redshift–distance and velocity–distance laws. The former is what Hubble[4] discovered (with, of course, input from others), but the title of his paper mentions "a relation between distance and radial velocity", using apparent magnitude as a proxy for distance and redshift as a proxy for velocity.

As Harrison points out[3], the linear velocity–distance relation is theoretical, involves unobservable quantities and, in a universe described by the Robertson–Walker metric[5,6,7], is exact: it is the only velocity–distance relation possible in a universe which is homogeneous and isotropic at all times. By contrast, the redshift–magnitude relation is observational, involves quantities which can be 'directly' observed, and is approximate both observationally (because of contamination of the cosmological redshifts by redshifts due to peculiar velocities and because of scatter in the absolute magnitudes) and theoretically (since, in general, it holds only in the limit of zero redshift when computed based on the assumption of a Friedmann–Lemaître cosmological model).

The constant of proportionality is, in both cases, the Hubble constant. While Hubble's interpretation of apparent magnitude as distance and of redshift as velocity are both valid only in the limit of zero redshift (at least if the velocity is interpreted as the temporal derivative of the distance), one can nevertheless use this to measure the (same) constant of proportionality for the theoretical velocity–distance relation, which is valid at all velocities and at all distances. As emphasized[3] by Harrison, it is at best confusing even to think about the Doppler effect in this context (though many do so), but Bunn & Hogg demonstrate[8] that this is possible after all if one uses the appropriate definitions of velocity and distance, though I hasten to point out that the velocity involved is one which, as far as I know, has no other use in cosmology.

Ironically, Hubble himself, while never as keen on theoretical interpretation as on the observations themselves, probably doubted that the expansion was real[9].

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Can the Date of Moses' Death be Determined Astronomically?

Manetsch & Osborn[1] attempt to date Moses' death astronomically by quoting "an early Jewish tradition that the Sun darkened on the day of Moses' death", which they interpret as a solar eclipse. This is not a particularly early Jewish tradition; it comes from the Zohar[2]. According to most authorities, the Zohar in its final form dates only from the 13th Century, although it may possibly incorporate some older material[3,4]. While in most astronomical circumstances a 13th-Century reference would be deemed early, the Zohar dates from over 2500 years after Moses' death, and from several centuries after rabbinic sources such as the two Talmuds. Thus anything that can only be sourced to the Zohar and not from an earlier rabbinic source is unlikely to be an authentic early tradition.

Further, the reference to the Sun darkening must be understood in terms of the philosophy of the Zohar. Moses represents the mystical concept of *Tif'eret*, which is symbolised by the Sun and by the written Torah[5]. Thus Moses' death affected the Sun in a metaphysical way, not necessarily a literal one.

A key point in the paper is the authors' claim that "The most accepted period for Moses' life is in the 15th Century BC, although some scholars place this as hundreds of years earlier or later." They source this to the *NIV Study Bible*[6]. In fact, the *NIV* chronology is quite controversial. For example, overwhelmingly scholars fall into two camps for the date of the Exodus: *circa* 1450 and *circa* 1250 BC. The *NIV Bible* states in the time chart at the front that the Exodus occurred in 1446 BC. However, on p. 84 it notes that there is evidence for a date of about 1290, although "there are no compelling reasons to modify in any substantial way the traditional 1446 BC date". On p. 285 it says "Much of the data from archaeology appears to support a date for Joshua's invasion c. 1250 BC … On the other hand, a good case can be made for the traditional viewpoint that the invasion occurred c. 1406 BC." There is no mention of any earlier date. It should be noted that one reason for the *NIV* supporting the traditional 1446 BC date is that it is consistent with Evangelical interpretations of the Bible, and the editors of the *NIV Study Bible* were all Evangelicals.

A detailed discussion of the historical and archaeological evidence is given by Thompson[7]. He notes: "It has been widely held that the Exodus took place about 1440 B.C. ... Most scholars today feel that the weight of evidence is for an Exodus from Egypt about 1280 B.C." It has been claimed that the city of Jericho was destroyed in 1550, implying an earlier date for the Exodus. However, according to Wood[8], "The pottery, stratigraphic considerations, scarab data and a Carbon-14 date all point to a destruction of the city around the end of Late Bronze I, about 1400 B.C.E." Further, there is some evidence of a second destruction in about 1250. Also, there was considerable destruction in many towns near Jericho in about 1250, confirming that large-scale military activity took place around then[9].

These dates imply that Moses died in about 1410 BC or (more likely) during the 13th Century BC . The former is just about consistent with the authors' conclusion that Moses' death was in 1399 BC ; obviously, the latter is not.

In summary, the discussion of these authors is interesting but does not seem to be based on secure foundations.

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Dr. Osborn's Reply

Lisa Budd[1] is to be thanked for providing additional information regarding the source of the conjecture presented in our article[2] that the date of Moses' death might be fixed by an eclipse. We tried to stress in the paper that our findings involved numerous assumptions and uncertainties. I reiterate the statement at the end of the first section that the "… results, which involve an … ancient and … speculative astronomical event, should be treated cautiously." Budd's points exemplify the difficulties inherent in investigations of this type that we noted in the summary statement at the end of our article.

As the astronomer for the project, I note that Budd did not express concerns with the astronomical work. Her criticism relates to those portions of the article based on the rabbinical and biblical material for which Dr. Manetsch was the lead. Sadly, he is no longer with us and cannot respond. In his place I will offer my comments on Budd's points.

Miss Budd is correct that the Zohar, which gives the tradition that the Sun darkened on the day of Moses' death, is not an early source in terms of rabbinical literature. And we are in agreement that the philosophical context of the Zohar must be considered when using its material. Nevertheless, while those factors may significantly minimize the chances that the reference has its origins with a solar eclipse, they do not rule out the possibility.

Most of Budd's comments regard when Moses might have lived. I argue that the paper's statement "the most accepted period for Moses' life is in the 15th Century BC, although some scholars place it hundreds of years earlier or later" accurately represents current knowledge. It does not conflict with possible dates of the Exodus mentioned by Miss Budd. Indeed our Moses' death date is in reasonable agreement with the earlier one. Further, the statement's uncertainty window was carefully chosen to encompass both the *circa* 1250 BC date mentioned by Budd as well as the *circa* 1550 BC one suggested by the Jericho archaeological record. This Jericho destruction date seems to be supported by the preponderance of the data, notwithstanding Wood's analysis[3].

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On the Discovery of the Period of the Crab Nebula Pulsar

The discovery of radio pulsars[1] gave rise to an explosion of knowledge of stars and their evolution, immediately attracting the interest and attention of astronomers world-wide. It also sparked widespread public interest in pulsars. Subsequent discoveries over the next year resulted in discoveries of similar sources, also 'ticking' with periods of about a second, which could be explained by pulsation of a white dwarf. The discovery of the period of the Crab Nebula pulsar with regular emission at a 33-ms period was the first detection of a source with a significantly shorter period, which could readily be explained by a rotating neutron star. The discovery of this pulsar has not been described previously.

Richard Lovelace, then a Cornell graduate student in physics working for Professor Edwin Salpeter, spent the period from 1968 April to December at the Arecibo Observatory, doing research on the newly discovered pulsars. He was assigned by Prof. Salpeter to develop computer codes for searching for pulsars in digital records[2], and had developed a 'fast-folding code', with which he discovered the 0·5579-s period of a new pulsar in Vulpecula[3].

Later that year, in September and October, after discussions with Arecibo Observatory visitors Len Tyler (a research associate at Stanford University) and Cornell Professor Tommy Gold (who was convinced that pulsars were magnetized rotating neutron stars — possibly rapidly rotating — rather than pulsating white dwarfs[4]), Lovelace began work on a new code designed to find pulsars with much shorter periods. The new code was based on a floating-point Fast Fourier Transform (FFT) code written by Tyler.

The Arecibo computer at the time was a Control Data Corporation CDC 3200 which had a magnetic core memory of 32K words of 24 bit length. Lovelace developed an integer-based FFT code using half-words (12 bits each) which accommodated FFTs of $N = 16\,384 = 2^{14}$ signal samples. For a sampling rate of Δt, the FFTs were calculated from spans of data of duration $T = N\Delta t$. This long sample length made possible a fine frequency resolution of $\delta f = f_N/8192$, with $f_N = (2\Delta t)^{-1}$ the Nyquist frequency. These choices were important (*i*) for increasing the output signal-to-noise ratio, (*ii*) for allowing a search over a wide range of pulsar periods — including the shorter periods expected of rotating neutron stars, and (*iii*) for discriminating against local power-line noise. (The latter was required because in 1968 the frequency variations of the local electric power from one span of data to the next were always much larger than the δf of our FFT's!) All 8192 values of the received signal power *versus* frequency were displayed on a line-printer page as a folded raster scan, *i.e.*, side-to-side in rows and from top-to-bottom with values 0, …, 9, and 'X' for values greater than 9. The signal root-mean-square value was used to renormalize the signal so that its average Fourier power was <~1. The integer code ran relatively fast on this CDC computer, and for this reason it was named GALLOP[5,6].

The Arecibo computer was available to Lovelace between midnight and 8 am on weekdays, and essentially full time on weekends, which was convenient since Lovelace lived at the Observatory. It was his good fortune on the overnight computer run on 1968 November 9–10 to find in the FFT raster patterns of 196·5 MHz scans of the Crab Nebula* a highly significant peak at the frequency $f = ((4951+4952)/2)\ f_N/8192 = 30{\cdot}22$ Hz, where the Nyquist frequency was $f_N = 50$ Hz for these data comprising samples at 100 Hz. The corresponding pulse period was $P = 33{\cdot}09$ ms[5]. Part of the line-printer output of the discovery is shown in Fig. 1 of ref. 6.

This was an exciting discovery, but for a day or two it was met with scepticism by the observatory astronomers because the observed repetition frequency from the pulsar was close to the second harmonic of the local 60-Hz electrical power grid. As mentioned above, this was ruled out because the 60-Hz signal was known to change its frequency from one data span to the next. Follow-up measurements of the Crab pulsar established that the pulse period was 33·09 ms.

Observations at a 100-Hz sampling rate were Fourier analyzed using 163·8-s spans of data. Effectively, the Fourier transforms stacked about 5000 periods of the received pulsar signal, resulting in an improvement of the signal-to-noise ratio by a factor of $\sqrt{5000} \approx 70$.

Subsequently, the GALLOP code was applied to drift-scan observations of the Crab Nebula and this established that the pulsar's location was near the centre of the Nebula[6].

A few days prior to our discovery of the period of the Crab pulsar, D. H. Staelin (MIT) and E. C. Reifenstein (NRAO) announced the discovery of dispersed pulses from two sources in the vicinity (±2°) of the Crab Nebula[7]. In the paper describing the discovery[8] they report dispersed pulses from the sources NP 0527 and NP 0532, characterized in the abstract by the statement that "Both sources are sporadic, and, no periodicities are evident." Our pulsar search at Arecibo was independent of the NRAO work, and it was designed

*Scans of the Crab Nebula at 196·5 MHz were digitally recorded on magnetic tape earlier in the week by Lovelace's Arecibo collaborators H. D. Craft, J. M. Sutton, and J. M. Comella, for later computer analysis.

for discovering short-period pulsars. The NRAO observations[8] at 110–115 MHz were made with a 0·05-s time constant which precluded a measurement of the period of the Crab pulsar and no period was reported. Furthermore, at frequencies of 110–115 MHz, interstellar scattering broadens the Crab pulsar pulses to a width much larger than the pulse period. Fortuitously, at the 196·5 MHz frequency of the Arecibo observations the broadening was less than the pulse period. The identification of the Crab pulsar found at Arecibo with NP 0532 found at NRAO was established by the agreement of the dispersion measures observed at the two observatories[6].

The short period of the Crab pulsar made it very unlikely that the star was a pulsating white dwarf. It became clear that the pulsar was a rotating magnetized neutron star as advocated by Gold[4]. Indeed, several weeks after the discovery of the period, the 'spin-down' or expected lengthening of the period was observed[9]; the rotational energy released in the form of magnetized winds was later shown to be sufficient to power the electromagnetic emissions of the Nebula[10].

Yours faithfully,

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'Asteroseismology'

Astrolabe, astronomy, astrometry, astrodynamics, astrophysics, … even astrotheology — there are some ninety such words beginning with prefix 'astro-', of coinage both ancient and modern, in the files of the *Oxford English Dictionary**; the implied rule of construction is both very long-established and universal. So why, now, *astero*seismology†? Can anyone explain this apparently arbitrary garbling of orthography? The overtones are positively botanical! Is this simply a result of the adoption of 'English' as the global lingua franca of the sciences by a community many of whom, in truth, have little command of English?

The point raised, I suggest, is not merely one of lexicographical pedantry: it is surely undesirable that one of the most exciting developments of recent times in the most ancient of physical sciences be encumbered with a name which itself appears to proclaim illogicality and inconsistency. Don't we *astro*nomers stand for consistency and logic? And shouldn't the editors of scientific journals be making a stand over this?

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P.S. Since the above was written, the Managing Editor has drawn my attention to Douglas Gough's earlier letter on the subject (in *The Observatory*, **116**, 313, 1996). While one must necessarily bow to Professor Gough's immense classical erudition, there is surely nonetheless a point to be answered here: when writing the above, I was aware of the Greek root *aster*, having the 1843 edition of Liddell & Scott to hand, but the fact remains that the unvarying practice in modern English has been to omit the 'e' when constructing such compound words — unvarying, that is, until 'asteroseismology' came on the scene a few years ago. It is, I would suggest, more important in using modern scientific English that contradictions and inconsistencies, real *or* apparent, with respect to such well-established practice in English itself be avoided, than that appeal to the ancient Greek be allowed to prevail irrespective of that obvious consideration.

*Omitting non-current words and those unrelated to stars and astronomy; 'astero-', on the other hand, is unique to asteroseismology except for Herschel's 1802 coinage 'asteroid' (private communication Dr. David Shirt, scientific editor *OED*). Asteroid predates most of the compound words relevant to the present case and is now appropriate in making the distinction from 'astroid', that plane curve which is the envelope of the normals to an ellipse (although *that* usage appears to be late 19th Century).

†See *The Observatory*, **131**, 337, 2011, for instance. This spelling would seem to be making a bid to establish itself as the received usage in the literature despite the obviously correct astroseismology having been widely current only a very few years ago.

REVIEWS

How I Killed Pluto and Why It Had It Coming, by Mike Brown (Spiegel and Grau, New York), 2010. Pp. 267, 20 × 13 cm. Price $25 (about £16) (hardbound; ISBN 978 0 385 53108 5).

Well, when I was your age, they were called asteroids because they lived in the asteroid belt, or perhaps minor planets, because their orbits lived at the Minor Planet Center, but this was not always so. Back during the Great War they were often called planets (see, for instance, *Nature*, **94**, 248 from 1915, or **99**, 572 from 1917 on Ambrosie and Albert, respectively). And *New Scientist* (24 November 2011, page 52) has just revived planetoid. Now they are officially dwarf planets, whether in the main asteroid belt or the Kuiper Belt outside the orbit of Neptune. Brown's rôle in the story is that he (*et* perhaps *al.*) discovered Eris, a large KBO, and thereby forced the process that resulted in Pluto also being classified as a dwarf planet.

The book has three intermingled themes: the very extended, exhaustive, and exhausting process that led to the discovery of Eris (and some other far-out KBOs); details of several pre-birth months and the first post-birth month of Brown's daughter Lilah; and a fairly inaccurate description of how the International Astronomical Union operates and of the votes at Prague in 2006 that 'de-planetted' Pluto (the correct decision, says Brown, though arrived at incorrectly).

The hard work consisted of imaging parts of the sky likely to host KBOs, comparing old and new images, first *via* computer programs supposed to identify image-to-image changes, and then *via* visual, human inspection to separate real objects from image flaws (more numerous by far). It probably takes a slightly Aspergerish investigator to keep after this sort of thing for many years (says your faithful reviewer who just spent the nine opening hours of the UCI library hunched over volumes of *Nature* from 1914 to 1919 looking page by page for items reflecting the effects of the Great War on astronomy and astronomers). Then Brown and a couple of supporting students and postdocs still had to determine properties of their discovery (rapid rotation, a moon eventually called Dystonia, *etc.*), write a paper and, unfortunately, fend off an attempted take-over of the discovery.

Perhaps the most important use of the intermingled gestation, birth, and infant narration is to compare it with your own experiences. Do most parents keep a day-by-day, almost hour-by-hour, diary of sleeping, eating, and so forth? It is, anyhow, a happier narration than that of the story of Sedna, the Inuit daughter-goddess, who gave her name to another of the large KBOs.

Now, about the IAU. It is, as Brown notes, charged under international treaty with attempting to bring some order to astronomical coordinate systems, nomenclature, units and constants, and so forth. Is it important that we all agree about these things? Actually, yes — if you want your GPS to work, to be able to find things in the sky, and to tell others about them. Is the process ponderous! Yes — it took several General Assemblies to fold general relativistic effects into the definition of time in the Solar System in agreement with what JPL had been doing for mission tracking anyhow.

Is it secret or exclusive? Not very. An astronomer who has been part of the community for a while (three years post PhD is typical) is welcome to join with no initiation fee or dues. Anyone who is interested and not (yet) a member can ask for an invitation to a triennial General Assembly. And anyone who is there (which does mean paying a registration fee) is welcome to attend not only all the science but also the formal business meetings where voting occurs. But only IAU members may actually vote. For things involving money, voting is by countries with weight proportional to dues paid. For political things, like admitting new members, it is one country, one vote. And for scientific things, like boundaries of constellations, zero-point of galactic coordinates, and nomenclature, it is one member, one vote. This has sometimes been done just by showing of hands. At Prague, members received yellow cards when they entered the auditorium and held them up at appropriate times (Brown's "sea of yellow").

How were the votes counted? From at least the 1985 GA onward, by one or more tellers (appointed at the first part of the GA near the beginning of the two-week fiesta), plus a couple of helpers to run up and down the rows (all with fluent English, fleet feet, and no two from any one country). The chief teller did a bit of quick mental arithmetic and reported totals to the presiding officer (the IAU President at Prague, but not quite always). The only real laugh came after one of the votes on an amendment came very close to a tie, and the lead teller said that anyone who wanted a recount could move to Florida (which had recently had a fairly close count in the 2004 presidential election). A close call for the teller who had almost said "could move to Mexico", which had recently also had a very close election, but if you are going to make fun of some political entity, it had better be your own country, not a good neighbour. How do I know? Prague was roughly the 6th or 7th GA where I had skippered the team of tellers — also the last. I do not now remember all their names, but remain grateful to them, mostly students who had attended a young astronomers' lunch a few days before, and even more grateful to Patricia Whitelock of South Africa who had been part of the team at something like three previous GAs.

So, we now have eight Solar System planets, many hundreds of exoplanets, and also some large number of dwarf planets. So yes, Mike Brown killed Pluto, but he had a lot of help. And anytime he is prepared to join the IAU, its Working Group on Planetary System Nomenclature, and so forth, he will be most welcome. The Union, in any case, is now considering some system that will allow electronic voting for members who cannot get to the General Assembly. Meanwhile, your faithful reviewer still has about ten more days of library duty to finish the 11 relevant volumes of *Nature*, then on, she hopes, to German journals from the same period. — VIRGINIA TRIMBLE.

A Vertical Empire: History of the British Rocket Programme, 2nd Edition, by C. N. Hill (Imperial College Press, London), 2011. Pp. 383, 25 × 16·5 cm. Price £57 (hardbound; ISBN 978 1 84816 795 7), £29 (paperback, ISBN 978 1 84816 796 4).

Today, the UK is a world leader in satellite design and construction, but it is largely forgotten that, for a brief period, the government and the aerospace industry also had ambitions in the field of rocketry. This volume gives the full story of how the British work on rockets and satellite launchers blossomed and then withered to nothing during the late 1950s and the 1960s. This largely forgotten endeavour began with a desire to give Britain an independent nuclear deterrent at the height of the Cold War.

With the United States and the Soviet Union already developing long-range ballistic missiles, the UK government decided to develop its own missile system. The largest programme was Blue Streak, whose sole purpose was to launch hydrogen bombs at Soviet targets. Blue Steel was an air-launched missile designed to deliver megaton warheads, while the Black Knight sounding rocket was used for research into re-entry vehicles for nuclear warheads. Blue Streak was cancelled long before it could become operational, with Britain turning to the United States for its submarine-based Polaris deterrent. However, the vehicle's technology had become sufficiently mature for it to form the basis of a prototype European launcher known as Europa. Other, smaller, civilian projects which subsequently came to fruition were Black Arrow — the only UK-built launcher to place a satellite in orbit — and the Skylark sounding rocket.

The technical evolution and never-ending political debates associated with those projects are presented in great detail in this well-written and meticulously researched volume. However, it is difficult to read this history without wondering if Britain squandered its opportunity to participate in the development of a world-leading launcher such as Ariane. As the author comments, "the systems that were built and tested in the 1960s, and then abandoned, could have been commercially successful in the 1980s and 1990s. It was, perhaps, a penalty paid for being too early in the field." The pioneering work on rocketry and satellite launchers ceased as a consequence of lack of funding, political vacillation, and a perceived lack of the benefits to be derived from space research. Perhaps there are some lessons in this story for the Britain of today. — PETER BOND.

Ambartsumian's Legacy and Active Universe, edited by H. Harutyunian, D. Sedrakian, A. Kalloghlian & A. Nikoghossian (Springer, Heidelberg), 2012. Pp. 216, 24 × 16 cm. Price £90/$129/€99·95 (hardbound; ISBN 978 1 4614 0181 0).

Viktor Amazapovich Ambartsumian (1908–1996) unquestionably lived in interesting times, in the sense of the prototypical Chinese curse, and indeed contributed toward making them interesting, both astrophyscally and politically. Most readers will find that the most palatable part of this volume (with its four Armenian editors and nine additional authors, two Russian and seven Armenian) is the biographical material at the beginning, but I would urge anyone who takes that introduction as definitive to compare it with material written by Adriaan Blaauw (a strong Ambartsumian supporter) in his *History of the International Astronomical Union* and article in the *Biographical Encyclopedia of Astronomers*, and by Josef Shklovsky (definitely not an Ambartsumian fan) in his autobiographical *Five Billion Vodka Bottles to the Moon*.

Each of the 11 chapters deals with a topic about which Ambartsumian wrote at length over his long career. These included stellar dynamics, radiative transfer, star formation and flare stars, active galactic nuclei, and super-dense stars. His views on all of these were, to varying extent, conditioned by his firm conviction that astronomical objects and substances began in some very dense state (not known in the lab) and expanded outward to give rise to the stars, galaxies, and nebulae we see. It was evident, at least until very recently, every time one encountered an astronomer with roots at the Byurakan Observatory and Yerevan State University that his younger associates all endorsed this view of cosmic history, and it permeates most of the book chapters. I do not know whether adhering to this paradigm was a requirement for astronomers working in Armenia, or even in Soviet astronomy in general, where his influence seems to have been considerable. But as late as 1978, Shklovsky in writing a popular book on stellar astronomy thought it necessary to devote many paragraphs to proving that planetary nebulae come at the ends of stellar lives, not at the beginning.

On the political side, Ambartsumian headed (1961–64) what was probably the most nationally diverse Executive Committee in the history of the International Astronomical Union, and that during a period of considerable international tension. His team of rivals consisted of one man each from the US, Japan, Canada, Mexico, Czechoslovakia, South Africa, the UK, Holland, and France. And nobody left in a huff (at least as visible from outside). In most of our minds a quintessential Armenian, Ambartsumian was in fact born and received his elementary and secondary education in Tbilisi (Georgia), graduated from the University of Leningrad in 1928, and was employed first at Pulkova Observatory and then at Leningrad until 1943 (with war-time relocation to Yelabuga in Tatarstan, rather than Kazan where the Moscow physicists went). Yes, that put him at Pulkova at the time of the 1937 slaughter of astronomers, and indeed he was accused of "wrecking the department". But, of his colleagues, Kozyrev was imprisoned for a decade and Gerasimovich (the director) executed, as were about eight other IAU members, whom the Executive Committee decided (in Stockholm in 1938) not to ask about, lest they make the situation worse rather than better. Interesting times indeed, and an interesting person, whom I never met except on paper recording his words and those of his colleagues. We had, however, many friends (like Adriaan Blaauw) and acquaintances (like Shklovsky) in common, and also the very special Beatrice M. Tinsley. — VIRGINIA TRIMBLE.

The Astronomy Revolution: 400 Years of Exploring the Cosmos, edited by Donald G. York, Owen Gingerich & Shuang-Nan Zhang (CRC Press, Boca Raton, Florida), 2012. Pp. 426, 26 × 18·5 cm. Price £49·99 (hardbound; ISBN 978 1 4398 3600 2).

Conferences usually concentrate on rather narrow and specific topics, the attendees often being well known to each other and rivals and collaborators in the same field. The conference that spawned the book under review was, however, entirely different. The sky was the limit. The theme was the 400th birthday of the astronomical telescope, and celebrated its 1608 invention by Hans Lipperhey in Middelburg, Holland, a town famous for the production of clear optical glass. The conference was in Beijing in 2008 October.

Twenty four papers from this conference have been gathered together to form this book. The range of topics is vast. The quality is first class. The end product is a boon to astronomers of all ages, and especially to the final-year undergraduate students confronted with an essay project. They will revel in the readability, the celebrity of the authors, the apposite illustrations, the fulsome referencing, and the erudite approach.

There were essays delving into the mysteries of dark matter, dark energy, supernovae, and gaseous nebulae. Others dealt with such disparate topics as searching for Earth-like planets, detecting life on those planets, understanding the formation and evolution of galaxies, the physics of black holes, the production of energetic cosmic rays, the reasons for choosing either a universe or multiverse cosmos, and why the scientific culture of the West differed from that of the East. There were a few essays that I expected — a discussion of the impact of the telescope on the development of astronomy, recent advances in adaptive optics, reasons for campaigning for telescopes with ever-larger primary mirrors, the advance of radio astronomy, and telescopes in space. And then there was a fascinating set of specifically Chinese essays, on topics such as Chinese astrology and calendars in pre-telescopic times, the influence of the telescope on Chinese society, and the relationship between the celestial and terrestrial in Chinese philosophy.

This book is like a 'pick and mix' sweet shop. There is something for everyone, and many extremely enjoyable treats. And behind it all was a nagging and obviously unanswered question: revelling in 400 years of telescopic and astronomical progress, what will the next 400 years bring? — DAVID W. HUGHES.

Can We Do Without the Big Bang?, by Tom Gehrels (University of Arizona Book Stores, Tucson), 2011. Pp. 147, 25 × 17·5 cm. Price $16 (about £10) (paperback; ISBN 978 0 9832658 0 1).

Tom Gehrels was part of the Lunar and Planetary Laboratory of the University of Arizona for more than 37 years (as far back as my collection of AAS directories goes) and built his reputation as an infrared astronomer looking primarily at Solar System bodies. This volume is, however, designed to lead the reader to the answer "Yes" to the title question. The author can also, happily, do without inflation and string theory. Instead he puts forward a new theory, called the 'Chandra multiverse', in honour of the most inspiring of his teachers at the University of Chicago in the 1950s. In this version, all universes have the same physics (values of h, c, G, H, *etc*.).

Large portions of the text present basic physics and astronomy of the sort you would find in a standard popularization, but more clearly written than most, following the model of one of Gehrels' heroes, Darwin. He also expresses explicit admiration for Eisenhower, Nehru, and Sakharov. The most charming pages deal with his teen years in the Netherlands under German occupation, making his contribution to opposing it.

Given that his work has tended more and more toward unconventional cosmologies for the past decade and more, he has had, and recounts, numerous difficulties with referees and editors. He also recounts, and apologizes for, having rejected a book chapter in 1971, which he thought excessively speculative at the time: Samuel Herrick suggesting the digging of a second Panama Canal by guiding an impact by asteroid Geographos.

Do I think the new model has a large chance of being correct? No. But I wouldn't bet real money on string theory either, and am pleased to be on the list of people Tom Gehrels thought open-minded enough receive a copy of his book. Given the remarkably low price, I think you ought to make sure there is a copy available somewhere at your institution to show to people who complain that the scientific community refuses to listen to anything except main-stream ideas. — VIRGINIA TRIMBLE.

The Mythology of the Night Sky, by D. E. Falkner (Springer, Heidelberg), 2011. Pp. 251, 23·5 × 15·5 cm. Price £31·99/$34·95/€34·95 (paperback; ISBN 978 1 4614 0136 0).

For a science that operates at the cutting edge of technology, astronomy retains a touching attachment to the archaic past. Characters from ancient Greek mythology live on among the patterns of the constellations; the planets bear the titles of Roman gods; and many bright stars have names that originated with the Arabs over a millennium ago. Anyone who has ever wondered about the stories behind these names will want to take a look at this book by David Falkner, an amateur astronomer from Minnesota.

He begins on a practical note with a few pages of advice on how to photograph the constellations with a simple digital camera, in an attempt to encourage his readers to go out and look at the celestial figures he is about to describe. After providing some handy family trees of gods and goddesses, who were almost as confusingly inter-related as the royal families of present-day Europe, he embarks on a mythological tour of the sky, season by season, starting with the glittering stars of winter. Falkner describes the constellations visible in each season and the objects of interest they contain, illustrated with diagrams printed out from the popular STARRY NIGHT software. This brief survey is followed by an extensive retelling of the associated legends. Only Ptolemy's 48 constellations are covered, so anyone interested in the remainder, including those of the far south, will have to look elsewhere. Falkner concludes with a substantial section on the planets and their moons.

As with other books in the Springer *Practical Astronomy* series, this one contains a dismaying number of typos and minor errors that betray a lack of editing and proof-reading. For example, readers might be puzzled to find Piscis Austrinus mis-named both as Pisces [*sic*] Austrinus and Pisces Australis. Pyxis was not one of the divisions of Argo, nor was it originally known as Malus; that was a subsequent name change proposed by John Herschel which was not adopted. Among the myths, Falkner claims that the mother of Orion was a Gorgon, Euryale (whose name he mis-spells Euryane); in fact she was a different Euryale, daughter of King Minos of Crete. On the positive side, though, Falkner corrects the long-standing mistranslation of Betelgeuse — anyone who still thinks it means 'armpit' should take note.

The myths and legends of the constellations are a good way to introduce newcomers to the sky. Falkner's book serves a useful purpose and I wish it success. — IAN RIDPATH.

Galileo: Selected Writings, a new translation by William R. Shea & Mark Davie (Oxford University Press), 2012. Pp. 431, 18·5 × 13 cm. Price £10·99/$15·95 (paperback; ISBN 978 0 19 958369 0).

Galileo Galilei had a lot going for him. He was a pioneering, dogmatic, argumentative, acerbic, hardworking genius, born at the right time and in the right place. But also, close to his 45th birthday (around 1609), he obtained a new-fangled 'telescope' which he was technically able to improve and turn to the skies. Being ambitious, and capable, he took the maximum advantage of this revolutionary instrument, and unlike others (the reticent Englishman Thomas Harriot, for example) he speedily published his observations, ensuring that his findings and opinions reached a continental-wide readership. Everlasting fame followed.

Galileo was at the forefront of most of the contemporary research topics in physics and astronomy. Since reading Kepler's work he had become a committed heliocentrist. The appearance of the Ophiuchus supernova in 1604 had turned him strongly against the Aristotelian view of cosmic perfection. Pendula, tides, sunspots, falling bodies, and mathematical instruments all occupied his time. Unfortunately Galileo's insistence that the Earth moved, and orbited the Sun, dragged him into theological waters. And in claiming that he had observed sunspots before Christoph Scheiner he annoyed the Jesuits too. He was eventually tried by the Roman Church and ended his life under house arrest.

In my opinion Galileo's three greatest books were *Sidereus Nuncius* (1610, Venice), written in Latin — a perfect example of how to publicise new astronomical results quickly and forcefully; *Dialogo* (1632, Florence), the *Dialogue Concerning the Two Chief World Systems — Ptolemaic and Copernican*, published in Italian, a masterpiece of rhetorical polemic; and *Discourse on Two New Sciences* (1638, Leiden), the foundation stone of Newtonian physics.

In *Galileo: Selected Writings* we are presented with over 390 close-printed pages containing the major highlights of Galileo's publications. William Shea (an expert on the 17th-Century scientific revolution and a retired professor of the history of science) has translated the Latin, and Mark Davie (the retired head of Exeter University's School of Modern Languages) has translated the Italian. Both have converted Galileo's words into clear, flowing, highly readable, modern English prose. Not only do we get the vast majority of the science, we also are presented with the key documents from Galileo's trial before the Inquisition, and a brief introduction to his life and philosophical and scientific background.

This book is an absolute joy, and I have a strong feeling that my collection of older Galilean translations will soon be collecting dust. — DAVID W. HUGHES.

Quantum Enigma: Physics Encounters Consciousness, 2nd Edition, by B. Rosenblum & F. Kuttner (Duckworth, London), 2011. Pp. 287, 21·5 × 13·5 cm. Price £10·99 (paperback; ISBN 978 071563979 5).

I confess that when I finally got around to reading this book I wondered whether I had made the right decision in agreeing to review it for this august journal. The subtitle, 'Physics Encounters Consciousness', set alarm bells ringing a bit, as I feared this might turn out to belong to the same category of vacuous New-Age twaddle as, *e.g.*, Danah Zohar's *The Quantum Self*.

In fact this book is much better than I originally gave it credit for, and in the end I'm rather pleased I got the chance to read it. It is for the most part a breezy and engaging but essentially straightforward popular-level exposition of quantum theory. The historical narrative begins with Newton and the rise of classical mechanics to set the stage for the radical developments that took place at the beginning of the 20th Century, covering the basic theoretical concepts — rather well, in my opinion — and rightly emphasizing the enormous practical utility of quantum mechanics in electronic devices and elsewhere. It then discusses the paradoxical nature of the quantum world and the various philosophical approaches that have been deployed to try to make sense of what the theory says or does not say about reality, given the tremendous successes quantum mechanics has achieved in predicting the outcomes of experiments. I was less impressed with this discussion of the 'metaphysics' of quantum mechanics, but it's a difficult subject to tackle at this level without being either, on the one hand, trite or, on the other, impenetrable. It's not bad, but doesn't really do justice to a number of important physical concepts, such as the rôle of de-coherence, and some of the philosophy is very superficial; there is much more to George Berkeley's empiricism, for example, than naïve solipsism.

Thereafter the book turns into a collection of short descriptions of trendy cosmological concepts (dark energy, the anthropic principle, black holes, *etc.*) and ends with the suggestion that the resolution of the deepest mysteries of the cosmos may lie with a better understanding of human consciousness rather than a better understanding of physics. I didn't find any real justification for this assertion, as with a similar suggestion made by Roger Penrose in his book *The Emperor's New Mind*. It has always seemed to me that the only thing that really separates our brains from monkeys' is our sense of self-importance. — PETER COLES.

Celestial Delights: The Best Astronomical Events Through 2020, by F. Reddy (Springer, Heidelberg), 2012. Pp. 438, 23·5 × 15·5 cm. Price £40·99/$44·95/€44·95 (paperback; ISBN 978 1 4614 0609 9).

This publication is yet another of Patrick Moore's wide-ranging *Practical Astronomy Series*, which has now reached nearly one hundred volumes. This book is clearly aimed at the novice astronomer and is presented in a very readable style with a generous number of good-quality illustrations. Covering the period 2011–2020, topics include sky lore, the Moon, Mercury and Venus, eclipses of the Sun and Moon, Mars, Jupiter and Saturn, stars, meteor showers, and unpredictable events. Each chapter contains explanatory material on these topics and a generous number of references for those wishing to pursue the subject further.

The chapter on Mercury and Venus provides useful diagrams to help locate those planets as well as the crescent Moon. It also shows the motion of Mercury and Venus as both morning and evening objects. The transit of Venus in June this year is described. Useful maps for total and annular solar eclipses are given in the eclipse section, which runs to nearly sixty pages, as well as configurations for the lunar eclipses in the same period. The chapter on unpredictable events includes the aurora, comets, and supernovae.

The four appendices provide the calendar of celestial events, Moon phases, greatest elongations of Mercury and Venus, and the oppositions of Mars, Jupiter, and Saturn, respectively. The calendar provides data in Eastern Standard and Daylight Time as well as Universal Time, which will no doubt be welcome to observers on the east coast of America. The events are the usual fare including solstices, equinoxes, Moon phases, planetary information, and meteor showers. However, one omission I did find was that occultations of bright stars and planets by the Moon were missing from the celestial-events list.

This book is nicely produced and will no doubt find a home on many amateur-astronomers' bookshelves. In many respects, it is an introductory guide to what is observable in the night (and daylight) sky. It also provides the first port of call for an explanation of some of the interesting happenings in the night sky. I just wonder if the price of nearly £41 is a little on the expensive side. This is a minor criticism and should not discourage anyone from buying a copy of this handy guide. — STEVE BELL.

The Casual Sky Observer's Guide, by R. De Laet (Springer, Heidelberg), 2011. Pp. 292, 20·5 × 12·5 cm. Price £35·99/$39·95/€39·95 (paperback; ISBN 978 1 4614 0594 8).

At first sight this publication resembles those other pocket-sized paperbacks which introduce new-comers to the night sky. However, there are some considerable differences. The author is Belgian but the English is excellent, there being but few minor solecisms (one I noticed was 'grand circle' for 'great circle'). The purpose of the book is to introduce the beginner to objects in the Northern night sky by using binoculars and small telescopes up to 4-inches aperture.

The first 57 pages deal with the basics of observational astronomy, ending with an excellent summary of the function of the eye and the performance of optical equipment. The second chapter, of 18 pages, describes the structure of the Galaxy as it is revealed by the Milky Way. Throughout the book the author is at pains to relate those individual objects under discussion with our Galaxy, thus putting them within a much wider context.

There follows a month-by-month description of the more prominent objects available for observation through modest equipment. Each chapter opens with a drawing of the Sun's neighbourhood with the objects under discussion positioned therein and with simple maps of the night sky showing locations. This is followed by a discussion of the nature of the individual object and its appearance as illustrated by a drawing of the field of view. This means that one is presented with a circle containing a scattering of stars and the object, which is often very faint, positioned therein. There are no eye-catching photographs in this publication but rather a representation of what one would actually see. Hence, the new-comer is introduced to the realities of observing. The book ends with a good glossary, a complete index, and references to useful internet sites, together with a selection of books and software.

This is a fine book which, if followed through the astronomical year, would well teach the budding astronomer the arts and crafts of proper observing and a secure knowledge of the night sky. However, the price does worry me. I fear that at the point of sale it is likely to be rejected in favour of other publications with a similar objective which would cost often less than a third of this one. — RICHARD H. CHAMBERS.

How We See the Sky: a Naked-Eye Tour of Day and Night, by Thomas Hockey (University of Chicago Press) 2011. Pp. 237, 23 × 15 cm. Price $20 (about £13) (paperback; ISBN 978 0 226 345 71 2).

Author Hockey is an historian of astronomy who frequently looks up at the sky and so has been able to reconstruct more or less what our ancestors would have seen as well as what we might reasonably see if telescope-less at a decent site. He also requires you to live at high latitude or high altitude, since his perfect January night includes snow crunching under your boots. Non-western thoughts get a decent look-in: a Japanese sky myth that explains why a lunar probe was named *Hagomoro*; how to use an archaic Indian kamal (not for riding or smoking); and the 34 temple platforms on the island of Necker, last and smallest of the Hawaiian chain, which happens to lie on the Tropic of Cancer.

Hockey's "quote of the month" pertains to a film he did not see, *2001: A Space Odyssey*, "No, but I read the book". This probably works better if you happen to remember Marilyn Monroe saying it about *Village of the Damned*, or my father's response to "Have you seen *The Ten Commandments*?" And, although the author confines himself to a mere handful of footnotes per chapter, most to readily accessible sources like *Sky & Telescope*, it is clear that he has read a great many books.

Do we agree about everything? Of course not! I think it is a 'quarter moon', because you are a quarter of the way through the 'moonth', rather than because of the 90° angle between the Earth–Moon and Moon–Sun lines. And my mother always said "the old Moon in the new Moon's arms", rather than the other way round. Do I entirely believe that *Blue Moon* is the second-most recorded song after *Happy Birthday to You*? Not entirely. Does anybody out there know for sure, or how to find out?

There are some tricky things explained very clearly, for instance, how the Egyptian use of decans (root dec = 10, as in December) led to 24-hour days, and why Stonehenge no longer quite points to sunrise on the summer solstice. Hockey also provides the 5th or 6th explanation of the Harvest moon I've read this year, surely correct, but somehow not much more persuasive than the others. Perhaps it's because mother never let me go on hayrides. And if it is my imagination that trying to work under fluorescent lights, which flash 120 times per second, impedes dark adaption, it is an imagining shared by the author and his source (the great William C. Miller).

All in all, a charming book, and if it drives you out of your office to look at the sky, well that is what astronomers are supposed to do. Conflict of interest? I'm thanked, but, honest Injun (the one who knows how to use a kamal) I can't remember for what. — VIRGINIA TRIMBLE.

The Amateur Astronomer's Guide to the Deep-Sky Catalogs, by Jerry D. Cavin (Springer, Heidelberg), 2012. Pp. 390, 23·5 × 15·5 cm. Price £35·99/$39·95/€39·95 (paperback; ISBN 978 1 4614 0655 6).

The collection of astronomical catalogues has a long tradition. Some may remember Dixon's monumental 'Master List' of non-stellar objects. However, in amateur astronomy, especially where deep-sky objects are concerned, there is little literature. Now the American amateur Jerry Cavin has tried to fill the gap, presenting a "catalogue of catalogues", which is announced as a "complete guide to the heavenly bodie [*sic*]". The author has selected 12 "historical deep space sky catalogues" (note the strange term). Moreover, he wants to provide the reader with "historical information" about their origin and makers. Does the book achieve these ambitious aims?

A first browsing shows an odd proportion between text and data: 90% of the 368 pages contain tables (the NGC alone covers 175). This could indicate a great amount of work by the author, but a detailed inspection unmasks a rigorous 'copy & paste' action. Is this really a problem? Yes, because of two facts: (*i*) the external data were adopted with no criticism, and (*ii*) the necessary formatting for a homogeneous presentation in a book has been strongly neglected. The result is a hotchpotch of unusable information.

The first three catalogues — from Ptolemy (*Almagest*), Brahe, and Hevelius — list single stars and cover 100 pages. What should an observation of these 'deep-sky objects' yield? Many stars appear in all three tables, which is not obvious as only the *Almagest* offers modern designations. They may look cryptic ("7Nu 2CMa", "41Ups4Eri"), but the really hard stuff is the unexplained column "HR number". It took me some time to clear up all its mysteries. The *Bright Star Catalogue* with 9110 entries is meant: for instance, "49Mu Tau" is listed as "HR 1,320" (note the comma!). However, "38Nu Tau" bears no number, though it is actually HR 1251. There are four numbers well beyond 9110: "HR 10,869", "HR 12,632", "HR 15,139", and "HR 16,475", three of which have a visual magnitude of 8! Fortunately, ecliptic coordinates for 137 AD are given (not quite user-friendly for amateur observers of today). A little calculation yielded an interesting result: the mysterious objects are the Double Cluster h + χ Persei (NGC 869/884), Praesepe (NGC 2632), the globular cluster ω Centauri (NGC 5139), and the open cluster M 7 (NGC 6475). What about the high HR numbers? Compare the NGC- and HR-number and omit the first one or two digits of the latter — so much for that! The next table, presenting Christian Mayer's double stars, struggles with the Latin genitive, *e.g.*, "24 Canceri", "λ Arietus", "τ 94 Taurus". Units for distance and position angle are missing and the important designation "WDS" is not explained (it is actually the *Washington Double Star Catalogue*).

Really problematic are the very deep-sky catalogues, associated with the names of Al-Sufi, Messier, Bode, Herschel (page heading: "Alexander Herschel"), Dreyer, Arp, and Moore. The Arp catalogue (which does not contain "338 galaxies"!) and Moore's *Caldwell Catalogue* are barely 'historic'. Surprisingly, the Al-Sufi table is modern too: Cavin presents 122 known objects (M 38 appears twice and the sorting order remains a secret). From these, the Persian astronomer has mentioned only four in the 10th Century (curiously the Large Magellanic Cloud is missing). Dreyer's *New General Catalogue* — called "New General Catalogue of nebula [*sic*] and clusters" — is oddly arranged by constellation, making it difficult to find a particular object. Completely ignored are the many identities (*e.g.*, NGC 6 = NGC 20). In these cases the positions match, whereas magnitudes and sizes often do not. There are even discrepancies between catalogues, like in the cases of M 82 (NGC 3034) and M 110 (NGC 205), which is called "Part of M 31" in the Messier table. The obscure galaxy list "Hershel 3" [*sic*] offers no constellation for nearly half its entries and for some they are wrong (NGC 16 in Pegasus, for instance, is placed in Andromeda). Generally, the author struggles with constellations. One finds, *e.g.*, "Boö", "Arg", or "Atn". The last two appear in the Hevelius table and might be Argo and Antinous, respectively; there is no mention in the constellation list (Appendix A).

In all, the catalogue selection is unsatisfactory and the tables brim over with typos ("Triffid Nebula", "Leo Mor"), content-related errors, and format problems. The information presented is often incorrect, inconsistent, irrelevant, or just strange (*e.g.*, unexplained types "NbDF", "GxyCld", "144Glx"). Relevant data are missing. The internet offers much better data.

The sparse text cannot relieve this harsh verdict. It too is full of errors, omissions, and inconsistencies. For instance, biographical data (year of birth/death) are missing or names are wrong ("M. Schjellerup", "Knoble"); the author confuses the 3rd and 4th Earl of Rosse and the index, where a system is not apparent, gives "Rosse, L.". Wrong names appearing in the text (*e.g.*, "Hershel", "Voroncoc-Velyaminov") are copied here. Also the appendix, the references (where important books like Stoyan's *Atlas of Messier Objects* are missing), and the few figures engender criticism. William Herschel is shown looking through a large refractor and Johann Elert Bode appears with his damaged left eye, while the text speaks of his right eye.

It is sad, but I'm unable to write anything positive about Cavin's book. Ironically, in a single volume the book does come up with his claim to be an "amateur astronomer's guide": it is amateurishly made! Not only can the author be blamed, but also Springer. There is no sign that a suitable proof reading had taken place — though it is possible to alter the content in order to get a good result. But it seems that neither the author nor the publisher have the necessary knowledge and concept to produce such a book. This is also true for other Springer publications, regardless of whether they belong to *Patrick Moore's Practical Astronomy Series* or other series. Conclusion: because the main advertised feature of the book — the "catalogue of catalogues" — has proved to be useless, it loses its right to exist. It's a sloppy work and not worth the money. I cannot see who will profit from the book. A good chance to fill a gap was missed. — WOLFGANG STEINICKE.

Observing the Messier Objects with a Small Telescope, by P. Pugh (Springer, Heidelberg), 2012. Pp. 401, 23·5 × 15·5 cm. Price £40·99/$44·95/€44·95 (paperback; ISBN 978 0 387 85356 7).

I can hear the groans already — not another Messier book! However, despite the subject being very familiar, Philip Pugh has come up with a new twist and some novel features in an attempt to make his contribution unique. Given the hefty price tag, has he succeeded? You will have to read on to find out!

The meat of the book is an object-by-object description supplemented with sections covering 'What it looks like', 'Charles Messier's original notes', 'How to view', and 'Photographic details'. The author's visual descriptions are all based on observations with nothing larger than a 5-inch telescope and binoculars — hence the book's title. A 5-inch telescope is, of course, very similar to what Messier himself used. The observations are brief and very matter-of-fact with few exaggerated claims of what really can be seen under typical observing conditions. They are eminently believable and probably typical of what the beginner can really expect to see with modest equipment.

The book has a large number of images — several for each object. Some are in colour but this is not a 'wow' book for imagers. They vary in quality from good to, being kind, less so. That new twist is the inclusion of what is described as "modified images". I could find no explanation of this but they appear to be the main images with a Gaussian blur applied; I presume this is an attempt to simulate a visual appearance. As might be suspected, sometimes this works but often perhaps not. This brings me to the book's biggest weakness, the finder charts. I guess these are actually images with text overlaid but whatever their origin they are simply not fit for purpose. Far too few stars are visible and their orientation is inconsistent. For a book of this cost they are just not acceptable.

Finishing on a positive note, Chapter 1 — a resumé of Messier's life and work — is excellent and the translation of Messier's notes (and occasionally Méchain's) is a useful inclusion. Somewhat unusually from Springer, a comprehensive index is provided — another plus. Overall, this was an interesting book with several good points but I feel its shortcomings sadly make its price unjustifiable. — DAVID RATLEDGE.

The Spiral Galaxy M 33, by P. Hodge (Springer, Heidelberg), 2012. Pp. 170, 24 × 16 cm. Price £90/$139/€99·95 (hardbound; ISBN 978 94 007 2024 4).

It is probably arguable that the greatest steps in the progress of astronomy come from the development of models based on large amounts of data, such as surveys or extensive programmes of observations. But for me, at least, the greatest satisfaction comes from case studies of individual objects, such as the stars carefully examined by Roger Griffin in the remarkable series running in this *Magazine*, exemplified by the *tour de force* on ζ Cancri C which opened Volume 120.

Thus, while the progress of cosmology doubtless advances with *Sloan* surveys and results from *WMAP*, *etc.*, I would hope that Paul Hodge derived satisfaction in producing this fine study of what would appear to be a 'bog-standard' spiral galaxy. M 33, to be found, perhaps by the eagle-eyed and certainly by the users of binoculars, on the edge of the small constellation of Triangulum, is a member of the Local Group but is somewhat dwarfed by M 31 and our own Milky Way. However, its proximity to us and relatively face-on attitude means that it has been amenable to studies which are more difficult to carry out for its larger neighbours. This has enabled Hodge nicely to bring together historical endeavours, particularly those of Hubble, with a host of more recent work on the galaxy's structure, stellar populations, interstellar medium, abundances, and dynamics, in a compact volume which should be in every astronomical library.

Complete with useful tables, numerous figures gathered from the literature, and copious references as recent as 2010, the book is also an ideal text for undergraduates trying to get a feel for a typical extragalactic component of the Universe. — DAVID STICKLAND.

The Exoplanet Handbook, by M. Perryman (Cambridge University Press), 2011. Pp. 410, 25 × 19 cm. Price £45/$80 (hardbound; ISBN 978 0 521 76559 6).

About a decade ago I began teaching a short course to our third-year undergraduates on extra-solar planets. I briefly covered the two main detection methods at the time, radial-velocity variations and transits, although the latter was then in its infancy. I also looked at the implications for our understanding of planet formation that were beginning to be inferred from the population statistics. However, this is a rapidly advancing field. Each year I have to modify my lecture notes, and not just to include the up-to-the-minute exoplanet 'count' in case any pedant in the audience decides to raise their hand!

The thousandth confirmed planet is likely to be found in the next couple of years. This figure does not include another thousand candidate transiting planets in *Kepler* data, many of which are likely to be real, although their masses cannot be measured with current instrumentation. Planets are now found routinely by methods such as direct imaging, astrometry, pulsation timing, and microlensing, although radial-velocity variations and transit detection are still the most successful techniques. We are now able to investigate their atmospheres with transmission and emission spectroscopy, particularly from space. We have a better, though far from complete, picture of planetary evolution. And contrary to popular opinion a decade ago, which was based on a poor understanding of biasses in observational data, it seems solar systems like our own are unlikely to be particularly rare.

In 2000 Michael Perryman wrote an excellent review of our knowledge of extrasolar planets in *Reports on Progress in Physics* (**63**, 1209), including a much-used graphic, sometimes dubbed the "Perryman Tree", which depicts the many potential exoplanet detection methods and their sensitivities. He has now produced an excellent textbook, *The Exoplanet Handbook*, which begins with this figure and leads the reader in detail through each detection method, the mathematics and physics underpinning them, the instruments and missions that employ them, and the successful detections. These are then discussed in the context of planet formation and evolution, and our understanding of the physics of planetary atmospheres. There are also sections devoted to exoplanet host stars, brown dwarfs, theoretical models of formation, structure and interiors, and finally a section on the Solar System and its evolutionary history.

There are quite a few books on exoplanets now available. Many are aimed as introductions for the layman or undergraduate. Perryman also describes his book as "formulated as an overall introduction for those new to the field", but it is more technically detailed and comprehensive than many of the rival texts. It is not a coffee-table guide for the amateur. Rather, it is an ideal companion for a PhD student in the field, as well as an excellent reference for the experienced researcher. In reality, very few of us research all aspects of exoplanets, and this book is sufficiently comprehensive to provide a useful guide to the areas with which we may be less familiar. But this is also an excellent, detailed textbook suitable for a specialist undergraduate- or postgraduate-lecture course, and this is where I will find it most useful. I thank Perryman for producing exactly the textbook I was looking for to expand my undergraduate course to cover more sensibly the rapid progression of the subject in recent years. Indeed, if proof were needed of the fine detail covered, my own rather more 'left-field' work on planets around white-dwarf stars is discussed. Twice.

The book ends with a comprehensive bibliography, and Appendices detailing the planets detected though radial-velocity and transit observations at the time of publication. Which, of course, is already significantly out of date. The results from the *Kepler* mission over the last year or so have impacted enormously on our understanding of the frequency and constitution of planetary systems. In particular, *Kepler* has identified a large population of bodies dubbed "Super-Earths" with radii somewhere between Earth and Uranus. Are these gaseous, or rocky? And despite some 'hyping up' of recent discoveries that are not likely to be habitable Earth-size bodies, *Kepler* may well identify such objects in the near future. I hope Perryman will be able to incorporate such results in a future edition. *Kepler* quite possibly deserves a chapter of its own. — MATTHEW BURLEIGH.

Handbook of X-ray Astronomy, edited by K. Arnaud, R. Smith & A. Siemiginowska (Cambridge University Press), 2011. Pp. 197, 23·5 × 15·5 cm. Price £35/$60 (hardbound; ISBN 978 0 521 88373 3).

This book is one of the CUP series providing observing handbooks for research astronomy. I have a couple of the others on my shelf, including the one on *Infrared Astronomy* by Ian Glass, and they have proved very useful in both teaching and research. The style of the series is to provide a modest-sized, modest-priced book which contains just enough information to introduce the subject while explaining the basics. This book is destined to be a fine addition to the series.

The editors are world experts in their field and provided most of the text, supplemented by a few chapters from others. The amount of detail varies somewhat from chapter to chapter but it is highly readable throughout. The book starts with an overview on X-ray optics and detectors, followed by chapters on how to analyse data, where to obtain data, and statistics, before concluding with the tricky subject of dealing with extended emission. If you have ever wondered about what a response matrix is, how to grade events, what the Suzaku MJD offset is to 19 decimal places (Table 4·3), and the like, this book will make it clear as well as explaining how to fit data and what not to believe. I usually hesitate to read anything with the word Bayesian in it but this is a gentle start.

I was a little disappointed with the somewhat USA-centric examples (or rather *Chandra*-centric) but perhaps that's understandable given where the editors work. The optics chapter, for example, ends with a brief look to the future which mentions slumped-glass technology but not the European Space Agency-sponsored work on micro-pore optics (MPO) — a technology that provides a route to lightweight, large-effective-area missions such as *Athena*. Likewise micro-channel plates get mentioned as detectors in section 2.5 but not as an optics option. Such an optic will soon fly on the ESA *BepiColombo* mission to Mercury. There are a few other omissions, including the survey section of Chapter 6 not mentioning the *XMM* Slew Survey, which provides an ingenious way to get a free sky survey by using data obtained while slewing between targets. The list of operational missions in table A4·3 also should, I feel, include the ESA *Integral* and *ASI Agile* missions as they have some X-ray capability (the *Jem-X* and *Super-AGILE* detectors, respectively).

These are, however, minor quibbles. Overall, this is an excellent, good-value book and should be required reading for all those interested in X-ray astronomy. — PAUL O'BRIEN.

Why Galaxies Care about AGB Stars II: Shining Examples and Common Habitats (ASP Conference Series, Vol. 445), edited by F. Kerschbaum, T. Lebzelter & R. F. Wing (Astronomical Society of the Pacific, San Francisco), 2011. Pp. 637, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 770 4).

Asymptotic-giant-branch (AGB) stars include the largest stars in the Universe. They are bright. Their surface layers are very, very cool, although the notion of a surface is perhaps difficult to define for stars as tenuous as these. As a consequence, many pulsate with long periods and huge amplitudes in a somewhat irregular fashion. They produce dust. They shed mass. They make new elements from hydrogen and helium, which they pump back into interstellar space, ready to seed the next generation of stars and planets. And *that* is why galaxies care about them.

This volume represents the proceedings of a conference held in Vienna in 2010, attended by some 180 delegates and including most of the experts in the field. It contains a truly impressive number of papers summarizing some 60 talks and even more posters on AGB stars and their relationship to the galaxies they occupy. The material is divided into five sessions, covering AGB-star atmospheres, AGB-star circumstellar environments, AGB stars in different populations, AGB stars in other galaxies, and recent AGB-star observations from space. The talks are supplemented by notes of the ensuing discussion, which are frequently illuminating, and by photographs of the participants. Both of these add extra value to proceedings which might otherwise become a reorganization of material available elsewhere; at least, one hopes that most of the results have been or will be published more formally. Two pages limit the scope of the poster presentations, but at least they are included.

Of greater importance, proceedings such as these gather together material on a common theme which can be used as a direction finder for researchers feeling their way around a new subject. I certainly found a number of papers which I will be following up. At a modest price, this well-edited book represents a valuable resource. — C. Simon Jeffery.

Cosmic Paradoxes, by J. A. Gonzalo (World Scientific, London), 2011. Pp. 134, 22·5 × 15·5 cm. Price £22/$34 (paperback; ISBN 978 981 4355 11 7).

This book attempts to examine cosmic paradoxes from an honest, conservative point of view. Honest it may be, but unfortunately the narrative is too uninformed and challengeable to be of much value, and I would not recommend reading this book except to use it as an exercise to show where the arguments are incomplete or simply wrong. It is a collection of chapters where themes are developed but not carried through. The author is keen to respect energy conservation, despite the fact that the associated time symmetry does not apply in the evolving Universe. In dropping the requirement for energy conservation, the author claims that cosmologists are able to use the freedom to design countless imaginary worlds, which is too strong in the opposite direction. Olbers' paradox is briefly discussed, but unfortunately too briefly to include its perfectly adequate resolution in Big Bang cosmology. A short chapter on the strengths of the four forces of nature makes one point, that if the 'constants' of nature e, h, and c vary, then they had better do so in a contrived way which keeps the fine-structure constant virtually constant. This is fair enough, but it is a stretch to describe it as a cosmic paradox, as evidence for significant changes in the constants of nature is hardly compelling. Vacuum energy and curvature are described as being equivalent to each other, when they clearly are not, as their effects are very different as the Universe expands. Counter to claims in the book, open universes do not imply a counter-gravitational force, as a simple matter-only expanding universe will attest to. And so it goes on. A detailed calculation is performed deriving the matter-density parameter of the Universe on the basis of the present-day Hubble constant and the age of the Universe of 13·7 Gyr. It is briefly interesting to note that the answer comes out to be very close to the baryon-density parameter, but it neglects to acknowledge that the age is derived from fitting observations to a model which requires dark matter and dark energy as well as baryons, so the calculation is not self-consistent. With some incorrect special relativistic interpretations of redshift, an assertion that neutron stars might be the dark matter, despite violating Big Bang nucleosynthesis constraints, and a rather bizarre and idiosyncratic list of singular moments in the Universe, the book fails in its aim. — Alan Heavens.

Galaxy Evolution: Infrared to Millimeter Wavelength Perspective (ASP Conference Series, Vol. 446), edited by W. Wang, Z. Yang, J. Lu, H. Hua, Z. Luo & Z. Chen (Astronomical Society of the Pacific, San Francisco), 2011. Pp. 423, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 772 8).

This conference proceedings summarizes the presentations at a conference held in Guilin, China, in 2010 October. The emphasis of the meeting was on results obtained at infrared and longer wavelengths, and was one of the first big astrophysics conferences aimed at this area since the first flood of results from the *Herschel Space Observatory*. It also includes an impressive suite of results from the legacy of the cold mission of the *Spitzer Space Telescope*, and the on-going warm operations of this mission, and images from the final suite of science instruments installed aboard the *Hubble Space Telescope*.

The impression from the volume is of great activity, with links between established areas of study and the new infrared-based facilities. The excitement of new tools becoming available to open up whole new vistas is clear: for a variety of uses — the discovery of amazing new targets in *Herschel* surveys (shown by Lutz and Magdis); new ways to investigate them and probe the conditions within (shown by Spinoglio and Riechers); and to dissect their properties in the greatest of detail (shown by a wide range of contributors throughout the chapters on redshifts 2–3 and beyond).

Reviewing the book at the very beginning of 2012, a little more than a year after the meeting, I can assure you that the excitement in this area has only grown, and that the future remains very bright. Despite the era of *Spitzer* and *Herschel* gradually drawing to a close, new observational results and capabilities are still becoming available. In fact, while the meeting was being held, the cold-phase of the NASA *WISE* all-sky infrared survey, which took place from 2010 January to September, was just ending.

From the ground, the *SCUBA2* camera is now operating at the *JCMT* submillimetre-wave telescope on Mauna Kea, truly dramatic advances in millimetre-wave spectrographs are in service at the IRAM telescopes, and the first operations of the titanic *ALMA* interferometer have begun for the whole science community. From air and space, the *SOFIA* 747-based infrared observatory is being used for the first time, the *WISE* all-sky infrared survey mission is completed, and while there are many issues of funding and spacecraft engineering to work through, instrument development for the future *JWST* and

SPICA infrared telescopes are extremely encouraging. While *ALMA* will offer astonishing improvements in capabilities, it will be far from the only exciting tool to use over the coming years to extend further the boundaries of the science described in the Guilin conference-proceedings volume.

The location of the meeting in China is also very interesting. The volume highlights the large number of Chinese students and postdocs working extensively around the world on the science presented here, Chinese-led research becoming a significant contributor to astronomical facilities for the years ahead, including suggestions for a large submillimetre-wave-telescope facility at China's spectacular ice-cap-dome research station in Antarctica. — ANDREW BLAIN.

Astrophysical Dynamics: From Stars to Galaxies (IAU Symposium No. 271), edited by N. H. Brummell, A. S. Brun, M. S. Miesch & Y. Ponty (Cambridge University Press), 2011. Pp. 413, 25 × 18 cm. Price £73/$125 (hardbound; ISBN 978 0 521 19739 7).

Forty-five years ago I and a student, Douglas Gough, raised the question of how a strongly turbulent fluid mass accommodates its angular momentum. If the fluid is nearly inviscid, the lines of vorticity can be taken to move with the fluid. Does the turbulent muddling of the vortex lines diminish any net mean vorticity and rotation as in Weiss's expulsion of magnetic field from a persistent eddy in a conducting medium? With this question in mind I read these proceedings.

Thanks to helioseismology we now know how the Sun rotates inside, and asteroseismology will soon tell us the internal rotations of other stars. The conference was dedicated to Juri Toomre who has master-minded the development of computer codes to tackle both such fluid problems as well as the more complicated MHD-dynamo problems posed by the magnetism of stars.

The contributor who came nearest to answering my question was Mathew Browning of CITA, who used such a code to model a fully convective star of $0{\cdot}3\,M_{\odot}$. His results agreed with those of Gilman in finding a surface with equator rotating faster than the poles, as in the Sun, when the ratio of buoyancy driving to Coriolis forces was less than unity. However, when that ratio was greater than unity the equator was the slower. Excitedly I looked for an explanation. Ferriere asked the crucial question. Why does the Sun's equator rotate faster than the poles? But Browning's reply was, "We still have a 'description' of how this happens rather than a first-principles theory." Computations can tell *how* things happen but it needs good brains to see *why*!

There is an interesting paper on wreath-building dynamos in the convection zones of rapidly rotating stars which demonstrate magnetic reversals when the parameters are set appropriately. However, I should not imply that the conference was limited to stellar interiors. In keeping with the breadth of Juri Toomre's interests there are papers here on galaxy interactions, supermassive black holes, the Galactic halo, and galaxy magnetic fields. Those developing the ASH code for convection clearly feel that agreement with observation is no longer a distant dream, while those concerned with asteroseismology see a new era opening with the observations from the *Kepler* and *Corot* satellites.

The 'Golden Age' of astrophysical fluid dynamics still lies in the future but good new graduate students will find a rapidly developing subject on which to make their mark. This beautifully produced book will provide them with an up-to-date survey of suitable research problems. — D. LYNDEN-BELL.

Proceedings of the 3rd Stueckelberg Workshop on Relativistic Field Theories, edited by N. Carlevaro, R. Ruffini & G. V. Vereshchagin (Cambridge Scientific Publishers), 2011. Pp. 314, 23·5 × 15·5 cm. Price £50 (paperback; ISBN 978 1 904868 73 6).

The workshop of which this volume represents the proceedings took place in Pescara, Italy, in 2008 July. According to the preface, there were three sessions, on quantum effects around black holes, observations of gamma-ray bursts and lightning with the *AGILE* satellite, and quantum physics and the gravitational field.

Unfortunately, the 41 contributions are arranged alphabetically by first author rather than by subject matter, apart from a longer presentation by G. 't Hooft on Hilbert space in deterministic theories, and it is not always 100% clear which papers belong to which session, apart from the gamma-ray-burst discussions, most of which feature a minority opinion of the underlying physics. Lightning does not appear in any of the titles or abstracts.

Stueckelberg was clearly an unusual sort of physicist, known (though not well) for the 1942 suggestion that positrons could be described both as having negative energy and as travelling backward in time. His personal stationery, shown on the book cover, described him as Baron Ernst Carl Gerlach Stueckelberg v. Breidenhach zu Breidenstein u. Melsbach, place names one suspects might be about as common in German-speaking lands as Springfields are in the USA. (He was born and died in Basel, Switzerland.)

The most memorable chapter title is surely "Planck scale theories of not everything", which, says author G. Amelino-Camelia, bear the same relationship to theories of everything that the Fermi theory of the weak interaction does to quantum electrodynamics. There is no index or conference photo, and no evidence of poster papers or a conference summary. There may be nuggets of gold in this volume, but the editors have made them impossible to find. — VIRGINIA TRIMBLE.

Numerical Modeling of Space Plasma Flows: ASTRONUM–2010 (ASP Conference Series, Vol. 444), edited by N. V. Pogorelov, A. Audit & G. P. Zank (Astronomical Society of the Pacific, San Francisco), 2011. Pp. 300, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 768 1).

In recent years numerical modelling of astrophysical and space-plasma systems has become an indispensible research tool that is joined together with ground- and space-based observations to elucidate the physics of remote cosmic environments. Nearer to home, detailed modelling of the solar corona and heliosphere is employed to provide predictive capability of space-weather events. This volume contains over forty brief (6–8 page) summaries of papers presented at the fifth annual International Conference on Numerical Modeling of Space Plasma Flows held in 2010 June in San Diego. Accordingly, the dominant flavour is American, but with a decent sprinkling of Europeans, French, German, Russian, *etc.*, but curiously not one 'Brit'. As may be expected from such a gathering, the largest slice of output, divided somewhat uncertainly in the volume into six parts, is devoted to numerical methods and algorithms spanning a broad range of techniques. One notes in particular the emergence of codes that not only incorporate adaptive mesh size, but also adaptive solution techniques within the computational domain, *e.g.*, fluid or kinetic according to requirements as the system evolves. Other notable themes in the volume include studies of plasma turbulence, of central importance in cosmic-ray acceleration and propagation, the physics of accretion discs, and in star formation, and the structure of the heliosphere and its interaction with the local interstellar medium, one striking output from which adorns the cover of the book. The latter topic has been stimulated not only by continuing measurements from the *Voyager* spacecraft, but also by unexpected results on energetic neutral-atom emissions from the outer heliosheath obtained recently by the NASA *IBEX* space mission, modelling of which is addressed in two papers in the volume. Overall, this book presents a condensed snapshot of a wide range of commendably recent numerical modelling work on space-plasma systems being undertaken internationally — other, of course, than in the UK. — STAN COWLEY.

Relativistic Quantum Physics: From Advanced Quantum Mechanics to Introductory Quantum Field Theory, by T. Ohlsson (Cambridge University Press), 2011. Pp. 297, 25 × 18 cm. Price £38/$65 (hardbound; ISBN 978 0 521 76726 2).

Having given a course to particle-physics and cosmology postgraduates in relativistic quantum mechanics through to introductory quantum field theory for over ten years, it was good to see a new book published covering this specific area. It is beautifully produced and covers all the key topics. The development of the Klein–Gordon and Dirac equations and the investigation of their properties are very clear. An example of the care taken is the explicit inclusion of the identity matrix where necessary to make the formalism easily accessible to those new to the field. Many previous texts just imply these, which can be confusing.

The introduction to quantum field theory through the quantization of a non-relativistic string gives clear insight into the process and the rôle of the commutation relations. What is learnt from this section is then applied to the Klein–Gordon and Dirac equations after a brief introduction to *s*-matrix theory and propagators. I would especially commend the way the electromagnetic Lagrangian density is clearly developed and then extended to describe the more general Yang–Mills Lagrangian, which lays an excellent foundation for an understanding of the Standard Model. The development of perturbation theory is also clearly presented and the route to Feynman diagrams *via* the interaction picture and time ordering is shown.

I have previously found no texts which give a clear and full exposition of the calculation of the electrodynamic cross-sections using the tools generated in this field, so I had to develop my own. However, this text describes in reasonable detail these calculations for many of the two-body leptonic interactions. Since such calculations are the ultimate goal of this subject, their inclusion is, in my opinion, highly commendable.

Clearly in writing such a review one does not have time to read the book cover to cover and attempt all the exercises. I did however ask a number of questions of this book that are often not trivial to students on any such course. Where it tackled those problems (and mostly it did) I found the answer well explained. It does, however, reflect the mathematical background of the author and sometimes I feel that the rigorous mathematical approach sometimes lacks a few of the more intuitive insights that might help someone new to this field. Although compact, the book is relatively short and I never find that more explanation or insight rather than less goes unappreciated by most students, whose insight into this subject might be accelerated with a little more explanation in places. The questions and exercises in this book are relevant and instructive and a valuable asset to a lecturer or student in this field.

One area I did find more difficult was the initial chapter and the relevant appendices on the Lorentz group. There was a high level of mathematical formalism which most physics students would find unfamiliar and which perhaps does not add significantly to an understanding. I feel that some of the arguments in that chapter were not fully developed and would be hard to follow.

I would recommend this book highly as a course text, but would use it alongside other texts, as in many areas it presents a good, clear explanation of a difficult subject. It would certainly be useful to anyone familiar with the field who would value a reference and compendium of the arguments and formalism. — ROB HENDERSON.

Principles of Stellar Interferometry, by Andreas Glindemann (Springer, Heidelberg), 2011. Pp. 342, 23·5 × 15 cm. Price £81/$124/€89·95 (hardbound; ISBN 978 3 642 15027 2).

Andreas Glindemann has worked since 1997 with the *Very Large Telescope Interferometer*, and the material in the book is based on his experience in planning and developing the facility. He believes that optical and near-infrared interferometry have become an essential part of astronomy and that there is, therefore, the need for a book presenting the underlying physical principles, deriving the relevant properties for interferometry and relating them to interferometric observations (paraphrased from the author's preface). The book is designed for use, in the sense that most of the 19 sub-sections end with one-page summaries containing the information a reader needs to go on to the next chapter.

The focus is on amplitude interferometry, with frequent sidelights on intensity interferometry (Hanbury-Brown and Twiss), speckle interferometry (Labeyrie), and adaptive optics. This last is essential to get the best results out of any interferometer whose mirror sizes are larger than about 20 cm (a basic blob of turbulent air). And one must understand interferometry thoroughly in order to achieve aperture synthesis — the mocking-up of a single large mirror with multiple small ones, which either move around on the Earth's surface or are carried around by Earth rotation. Aperture synthesis is the primary topic of another, recent Springer book by S. K. Saha, and I have just spent an hour or so flipping back and forth between the books, trying to decide whether either can replace the other. Both have tables of symbols (Glindemann is more complete) and long lists of references (Saha is longer — 18 pages *versus* 11) and cite many of the same experts (J. M. Beckers, H. M. Colavita, J. D. Monnier, F. Roddier, and so forth), though neither has a 'back index' or an author index to enable the reader to figure out who is being credited for what. Saha says more about how radio astronomers pioneered serious interferometry and aperture synthesis, and Glindemann a bit more about devices operating or operated in the recent past. Both have sections called "Maximum Entropy Method" (MEM) but containing superficially, at least, none of the same equations (yes, there are lots, and lots, and lots of equations in both volumes).

Thus if I were setting out to build an optical amplitude interferometer with adaptive optics to use for aperture synthesis, I would want both books and also the help of both authors and some other experts as well. The topics are difficult but important ones, to which both authors have made helpful contributions. — VIRGINIA TRIMBLE.

Jets at All Scales (IAU Symposium No. 275), edited by G. E. Romero, R. A. Sunyaev & T. Belloni (Cambridge University Press), 2011. Pp. 420, 25 × 18 cm. Price £73/$125 (hardbound; ISBN 978 0 521 76607 4).

IAU Symposium 275 took place in Argentina in 2010 September, and the proceedings reached *The Observatory* office at the beginning of 2011 April, about as close to the IAU nominal six-month deadline as anyone ever comes. The volume has a number of other virtues — discussion after most talks; author, object, and subject indices (though the latter does not mention some key concepts and questions, like confinement, launching, and baryon content); a list of participants with e-dresses (about 125, 27% women, and a large fraction from Latin America); papers that start on either odd or even numbered pages, sparing us blank pages or photos of the social events; and a conference photo in glorious black & white, printed well enough that at least half the participants can be recognized.

The symposium was quite rightly focussed on five key areas — basic physics, AGNs, micro-quasars, gamma-ray bursts, and young stellar objects (though Sgr A* hides in the first, and one planetary nebula among the YSOs — but it does have a jet!). One might also have expected to read something about pulsar-wind nebulae, the Crab Nebula, and supernovae/SNRs in general. But in fact most of the traditional questions of baryon content, magnetic fields, binary black holes and mergers, quark stars, magnetars, jet confinement and launching, and the rôle of discs are mentioned, though I don't think a careful reader will come away more certain of the answers to those issues than before the reading. This seems nearly always to be the case for conference proceedings, even for conferences where participants will say afterwards that they think some particular dispute has finally been resolved or some significant falsification has taken place.

The quote of the week (September 13–17) appears in a post-talk discussion: "It would be nice, I think, if the XXX Team check the relevant literature. Just a comment." And the response, "Thank you for your pointing out. In fact we have been aware of your work on ... However our work … ." Feel free to fill in whatever mission and topic you are currently feeling annoyed about! The response ends, "We did not have room for listing all previous studies." The published papers range from two pages (for each poster) up to 12 for invited reviews (including the introduction by Felix Mirabel, whose 65th birthday was being celebrated). In general, I think, the two-page and long papers do their job well. It is the (typically) four-page contributed talks that don't have the opportunity either to focus on a specific result or to put a whole territory in context. Just a comment. — VIRGINIA TRIMBLE.

ENGLISH LANGUAGE RELEASE

The Astronomer Jules Janssen: A Globetrotter of Celestial Physics, by F. Launay (Springer, Heidelberg), 2012. Pp. 220, 24·5 × 18 cm. Price £90/$129/€99·95 (hardbound; ISBN 978 1 4624 0696 9). Favourably reviewed in the French original by A. Heck in Vol. **128**, p. 413, 2008.

THESIS ABSTRACT

Of Spirals and Satellites: A Study of Andromeda

By Michelle Collins

Understanding the origin and evolution of $L_\star$ galaxies like the Milky Way and Andromeda (M31) is one of the key goals of modern astrophysics. Galaxies like these are representative of the general field population of the Universe, and have been evolving into their present form for the majority of cosmic time. The study of resolved stellar populations of nearby galaxies in recent years has revolutionized our understanding of the processes underlying the formation and evolution of these spiral galaxies, with large-scale surveys such as the Sloan Digital Sky Survey and the Pan-Andromeda Archaeological Survey (PAndAS) pioneering the detailed study of structure and substructure within the Local Group. While these studies have illuminated the rich and diverse evolutionary histories of nearby galaxies, we are still a long way from a complete understanding of the intricate processes that govern galactic genesis.

This thesis aims to bring us closer to these ambitious aims by shedding light on the stellar populations of the Andromeda system. I present an analysis of the resolved stellar populations pertaining to the satellite and stellar-disc components of M31 by using data from two large-scale surveys of the Andromeda–Triangulum region: the PAndAS *CFHT MegaCam* photometric survey and the Z-PAndAS spectroscopic follow-up survey, conducted with the *DEep Imaging Multi-Object Spectrograph* on the *Keck II* telescope.

Chapters 1 and 2 serve as an introduction to the topic of near-field cosmology and a description of the PAndAS and Z-PAndAS surveys.

In Chapter 3, I present a kinematic analysis of an extended cluster, EC4, in the Andromeda halo, showing that it possesses little or no dark matter, making it consistent with the globular-cluster population of M31.

In Chapter 4, I present a kinematic analysis of four of the faintest dwarf spheroidal galaxies (dSphs) orbiting M31, demonstrating that their velocity dispersions (and hence, central masses and densities) are lower than their Milky Way counterparts. This suggests that they reside in different dark-matter halos than their Galactic cousins.

In Chapter 5, I continue this analysis with a kinematic study of two of the brighter 'classical' M31 dSphs and show that it is generally the more extended M31 dSphs (*i.e.*, those with larger-scale radii than Milky Way dSphs of comparable luminosity) that possess lower-than-expected velocity dispersions. I argue that this phenomenon could be caused by tidal forces exerted on the dSphs by the stellar disc.

Chapter 6 presents a kinematic analysis for five further M31 dSphs, before summarizing the kinematic and chemical properties for all M31 dSphs that have been spectroscopically surveyed to date. I also investigate how robust my analysis techniques are, given the régime of low-number statistics that I am working in.

In Chapter 7, I present a kinematic detection and analysis of the M31 thick stellar disc, showing it to be hotter and more massive than that of our own Galaxy but comparable to other galaxies where such a component has been observed.

Finally, Chapter 8 summarizes the outcomes of this work and outlines pertinent projects which will serve as the natural extension of this work for the next several years. — *University of Cambridge; accepted 2011 August*.

A full copy of this thesis can be requested from: collins@mpia.de

Here and There

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