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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2017 March 10 at $16^{h}\ 00^{m}$
in the Geological Society Lecture Theatre, Burlington House

J. C. ZARNECKI, *President*
in the Chair

The President. Good afternoon, ladies and gentlemen and welcome to the March Open Meeting of the Royal Astronomical Society. I start with some sad news: some of you might be aware of the recent and premature death of Neil Gehrels who was active at NASA Goddard Space Flight Center and worked with many Fellows on the *SWIFT* mission amongst others. I mention it in particular because he was made an Honorary Fellow of this Society just over a year ago. It is indeed bad news.

I move on now to the announcement of the AGM, the 197th Annual General Meeting of the Society: it will take place here at 4 pm on Friday 12th of May 2017. In association with that, there is the matter of the honorary auditors. Fellows will be aware that we produce the annual report and the accounts and prepare these for the AGM. In addition, we appoint two Fellows who are not members of Council to be the honorary auditors. They are directed by the by-laws of the Society to deliver a personal report on the resources, goals, structures, activities, conduct, and general health of the Society, but not on matters relating to finance, law, or personnel, and this report is delivered to the AGM. So this year, the two honorary auditors are Katherine Joy and Euan Monaghan and they will undertake this role. They would welcome input from any members of the Society, any comments that you might have, so please do feel free to communicate with them.

We now move to today's programme and we start with John Armitage of the Institut de Physique du Globe de Paris; John was of interest to us as he was appointed to an RAS Research Fellowship in 2013, which he held for two years until he left it early. They normally run for three years, but he has moved to Paris. He's going to talk on the subject of 'Can variations in the Earth's orbit create stratigraphic sequences?'

Dr. J. Armitage. I begin by introducing a word, apophenia. Apophenia is the experience of seeing patterns or connections in random or meaningless data. Meaningless is a bit hard to swallow as a scientist, yet when looking at sedimentary deposits, where different sedimentary units rest on top of one another, we have to question if the patterns we observe are real and meaningful.

Sequence stratigraphy is a branch of geology that attempts to subdivide and link sedimentary deposits into unconformity-bound units on a variety of scales, and explain these stratigraphic units in terms of variations in sediment supply and variations in the rate of change in accommodation space. It is often assumed that the change in accommodation space is associated with changes in relative sea level. As sea level falls the sedimentary system progrades away from the land, pushing the coastline sea-ward, and conversely as sea-level rises, land is flooded and the sedimentary system back-steps. Therefore the movement up-stream and down-stream of depositional boundaries, such as the coastline, might be evidence for historic movement on sea level. What could cause change in sea level? Climatic change is a potential driver. The Earth's orbit around the Sun is not a perfect circle, it is very slightly eccentric. The eccentricity of the Earth's orbit may have changed throughout the last few billion years, with a cyclical increase and reduction over periods of 100 000 to 400 000 years. Furthermore, the Earth precesses as it spins on a 26 000 year cycle, and its tilt oscillates between roughly 22° and 25° in a period of 41 000 years. If these various orbital frequencies are put together, it is plausible that climate will vary with a certain frequency depending on which orbital 'wobble' is dominant.

The deposition of sediment at the coastline is, however, not only affected by the relative height of the sea. It is also a function of the quantity and calibre of sediment delivered from the eroding catchments, which is also affected by change in climate through weathering, precipitation-rate change, and many other factors. This leaves a question: are the patterns we see in the sedimentary rock a record of a cyclical movement of gravel, sand, and mud deposition caused by changing sea level or change in the sediment delivery?

To explore this question I developed a simple numerical model of sediment transport in the subaerial and submarine domains. The reason I developed a simple model stems from the following rationale: if the geological complexity can be captured in the parameter G, and the mathematical complexity of the model as M, then $G \times M = 1$. That is, the simpler the system we wish to understand, the more complex the mathematical model becomes, as we have a greater control of the experiment, and therefore we can get into greater detail on the mechanisms behind, say, the movement of a single grain of sand. However, if we have no experiment, but a series of observations from the addition of all the processes that move sediment on the Earth's surface, then the mathematical model has to be simple. We cannot hope to capture all the complexity, so we must begin by trying to understand how the first-order controls work.

In the spirit of a simple model for sediment transport across the Earth's surface, I developed a 1-D model that solves for transport down a line. I define two domains: a subaerial region that is above sea level, and a subaqueous region that is below. The transport of sediment is a function of the local slope, the input flux of sediment and water, the precipitation rate within the subaerial domain, and the sea level within the subaqueous domain. Sediment transport and deposition is then controlled by three factors: (*i*) precipitation rate, (*ii*) sea level, and (*iii*) the input of sediment from the eroding catchment.

Periodic oscillations in precipitation rate and sea level lead a lateral movement in three major depositional boundaries: the gravel front, the coastline, and the sand front. The gravel front is the point at which all gravels have been deposited.

The sand front is the point at which all sand has been deposited. For an increase in precipitation rate, the gravel front and sand front migrate seawards. The coastline also migrates laterally due to the increased deposition driven by the increased flux of water. For an increase in sea level the coastline migrates landwards, as does the sand front. However, the gravel front does not respond to change in sea level, as the signal of change cannot propagate up into the landscape. Therefore periodic change in sea level creates a different response to periodic change in precipitation rate, as only the latter causes migration of the gravel front.

This simple conclusion is then complicated by the addition of oscillations in the input sediment flux at the upstream end of the model. As more sediment enters the model domain, the gravel-front movement is damped, because the increased transport rate is balanced by an increased amount of sediment. Therefore change in both precipitation rate and input sediment flux creates a pattern of deposition that is very similar to change in sea level alone. Stratigraphic architecture is most certainly non-unique.

To explore the implications of this model, we have to move away from an idealized model with arbitrary input sediment flux and subsidence. The Book Cliffs in Utah, USA, are a beautiful set of cliffs, with huge scree slopes, and exposed rocks that display transitions from gravels, sands, and mud stones. I am not a sedimentologist, but by working with sedimentologists at Royal Holloway, University of London and Imperial College — Peter Burgess, Philip Allen and Gary Hampsen — the exposures are mapped and a stratigraphic section can be constructed. The section would suggest a periodic movement of the coastline, with a period of around 100 000 years, when these rocks were deposited more than one hundred million years ago. Yet, was it sea level or sediment supply and transport that caused this pattern of movement in the coastline?

The simple 1-D numerical model is therefore forced with a pattern of subsidence that matches the observed stratigraphic section. Sediment is delivered to the ancient Book Cliffs formation in line with the observed deposits, and either precipitation rate or sea level is oscillated. Periodic change in the precipitation rate leads to a lateral movement of the coastline of the order of tens of kilometres in agreement with the observations, but it is accompanied by a very large, 100 km or more, cyclical lateral movement in the gravel front. Periodic change in sea level by 20 metres likewise leads to lateral movement of the coastline by tens of kilometres. Gravel deposition is, however, steady.

Within Utah and Colorado there is no compelling evidence of large migration of gravel deposition during the formation of the Book Cliffs deposits. Could it therefore be that sea level rose and fell by 20 metres throughout the Cretaceous? If all the ice were to melt on the Earth today, sea level would likely rise by more than 60 metres. Would the tiny change in climate driven by the subtle change in the Earth's eccentricity cause one quarter of all the ice present on the Earth to melt? Perhaps, but it is worth considering if we are guilty of apophenia, and are we instead simply seeing patterns in random or meaningless data?

The President. Thank you very much indeed. We have time for a few questions or comments.

Dr. R. Catchpole. There was a report, I think in *Nature* a year or two ago, suggesting that you could see Milankovitch periodicities in the mid-oceanic ridges off Australia. What do you think about that?

Dr. Armitage. I have opinions about that. There's a nice debate going on in the geophysical community. Milankovitch oscillations would affect the water level, and the water level would affect the pressure on the zone of partial melting below the mid-ocean ridge. If you release pressure then decompression melting increases and you'll get more melting, so you erupt more and make a slightly thicker mid-ocean ridge and make sea mounts. Therefore you could map your sea mounts away from the mid-ocean ridge, and if you have nice oscillations in sea-floor elevation at around about 20000 years or maybe 40000-year periodicity then bingo: that's the climate influencing the deep. It's a nice idea, but the trouble is that the sea floor tends to fault, and faults tend to happen at regular spaces. If you do a Fourier transform of any good fault model then you also find a periodicity of around 20000 to 40000 years, you can get oscillations, so the jury is out. There's a strong group from Harvard that thinks the oscillations in sea-floor bathymetry are climate-driven, and there's a strong group from Woods Hole Oceanographic Institute that thinks it is due to faulting.

The President. Thank you very much indeed, John. Our next contribution is from Jane MacArthur of the University of Leicester. The title of her talk is 'The first regolith breccia meteorite from Mars'.

Ms. Jane MacArthur. I'm grateful for the opportunity to speak here today about my PhD research at the University of Leicester, where my supervisors, Professor John Bridges, Professor Mike Branney, Dr. Steve Baker, and postdoc Dr. Leon Hicks have given me a lot of support; I am also indebted to Dr. Katie Joy and Professor Ray Burgess at the University of Manchester, who have done some argon-dating work with us, and to Dr. Natasha Stephen at Plymouth who did a microprobe map of one of our sections.

The first regolith breccia meteorite from Mars was discovered in 2011 and first characterized by Carl Agee *et al.* in 2013. It's different from the other Martian meteorites because it's more representative of the surface material. It's thought to have been formed in an impact event, fusing together lots of different pieces of the surface of Mars of different ages and different mineralogies. We have a 1·8-g sample, and there are at least seven known 'paired' meteorites, *i.e.*, separate meteorites with the same characteristics. When I started my PhD we knew of about 80 unique Martian meteorites, but we have recently passed 100 as new ones are always being found and classified.

Traditionally the Martian meteorites were known as 'SNCs' after three witnessed falls: Shergotty in India, Nakhla in Egypt, and Chassigny in France, and so we have the shergottites, nakhlites, and chassignites, classes of meteorites that share similar characteristics with the original falls. These are deeper-formed rocks from igneous processes. The shergottites are all relatively young, under 0·5 Gy, and the nakhlites are all of a very similar age, around 1·3 Gy, thought to be one ejection event from Mars; then ALH84001 is the oldest at 4·5 Gy.

The regolith breccia of the meteorite that I am studying has a whole-rock age of 2·1 Gy (Rb–Sr), sampling a new reference point in the Martian time frame. However, the breccia also contains zircons that are 4·4-Gy old (U–Pb) and there's a whole range of younger argon-dating ages published, between about 1·3 and 1·8 Gy, which may indicate resetting events, such as the impact event that formed this meteorite.

Why is this meteorite important? It's our first opportunity to sample materials at an impact crater other than on Earth or the Moon, and impact cratering is a very important process throughout the Solar System. It's thought to have formed in some sort of ejecta blanket of a large impact event and possibly it was subject to a hydrothermal system, so we set out to look at the thermal history and alteration history within that meteorite.

When you think of impact craters you may traditionally think of the meteor crater in Arizona, 1·2 km in diameter, but for this breccia the formation impact would have been much larger, like the Ries crater in Germany, where the original impact is thought to have been 24 km in diameter. It isn't a traditional bowl-shaped crater, but it is recognised as a crater by diagnostic characteristics like shatter cones, breccia, and shocked quartz. My colleague Stuart Turner from our group at Leicester characterized craters on Mars using *HiRISE* imagery. One example shows rings outside the crater of what appears to be fluidized ejecta material. With an impact of sufficient force, you can get flow-like features, and hydrothermal minerals are also found here, so it is thought that a hydrothermal system operated after the impact event, mobilizing water in the crust.

Looking at textural features of interest, our CT scan shows a sphere of accreted material with different layers and dusty rims. These have been seen in several other samples of the meteorite, and we're wondering how they formed. They're similar to accretionary lapilli that are seen in pyroclastic events, explosive volcanism, which are also found as impact features at the Ries crater and Sudbury impact basin. It has been proposed that pellets, airborne in an impact-ejecta column or pyroclastic plume, gradually accrete dust particles, with water vapour in Earth's atmosphere, but could this process operate on Mars? Professor Michael Branney has compared Stac Fada impact deposits with Tenerife pyroclastic deposits, showing the similarities between accretionary lapilli and pellets from the two events. In pyroclastic and explosive volcanism, you can have a big plume form and lots of fallout material but you can also have devastating pyroclastic density currents and flows, and it is likely all of these can occur as the result of an impact event instead.

The bulk of my work has focussed on examining the meteorite with electron-microscopy and X-ray-spectroscopy techniques, looking at very tiny materials with very big machines. We've made three sections which are just a few mm across and can get down to nm resolution and beyond with the transmission electron microscope. Our meteorite has a very-fine-grained matrix, so in order to analyse a specific point without hitting several different phases at once, you need a highly focussed beam size.

That is why we do a lot of work at the *Diamond Synchrotron*. We've had four beam-time sessions in my PhD time, three on beam line I–18, where we can do X-ray absorption spectroscopy (XAS); this helps us compare the iron 2+ and iron 3+ to show us how oxidized it is, and X-ray diffraction (XRD) allows us to determine precisely what mineral we have; there is a whole range of hydrated iron oxides that we can't tell apart with electron microscopy techniques. We used beamline B–22 for Fourier-transform infrared spectroscopy (FTIR), which allows us to look for the hydration and the water content. We know this meteorite shows high hydration but is it in pyroxenes or the feldspars or the iron oxide? Whereabouts is the water hosted and why and how did it get there?

We have found a good number of pyroxene clasts showing exsolution lamellae, the separation of calcium-rich and calcium-poor areas during slow cooling. These compositions can be plotted on a pyroxene quadrilateral, and using thermometry you can get the temperature at which they crystallized, and the exsolution clasts crystallized at around 900 degrees Celsius. Some pyroxene clasts contain small white grains of iron oxide, and some have feldspar borders, showing evidence of re-melting or recrystallization at some stage.

We selected some pyroxene areas with the iron-oxide grains to look at with transmission electron microscopy (TEM). TEM images show we've got pyroxene but it is breaking down into iron oxide and an amorphous aluminium-silicate material that we see in between. This is unusual because pyroxene is quite stable, so it suggests it's a high-temperature breakdown which we don't see very often; in fact I've been struggling to find analogues in the literature of why this would occur.

To look into this further and look at the oxidation of this region, we did X-ray absorption spectroscopy on the pigeonite (pyroxene) clast, and another clast of bulk basaltic composition. Both of them contained the small iron-oxide grains and, using a calibration scale from Hicks *et al.*, we found there to be about 25% iron 3+. Monomineralic pyroxene clasts, without iron-oxide grains, did not show oxidation. To see if water was involved in this breakdown and alteration, we used FTIR, but did not see any hydration at all in those two clasts, so this reaction must be at high temperature and anhydrous.

Looking through the literature, we've found a paper by Warren and Rubin about pyroxene smelting in ureilites, another type of meteorite entirely, but they've got very similar textures to ours with the pyroxene breaking down to iron. Ureilites have had a completely different history to Martian meteorites — their reactions would have happened in a reducing environment, so they have metallic iron rather than iron oxide, but it's a very similar sort of texture that they've got: pitted porous pyroxene with iron oxide. They suggest that this oxidation of pyroxene was caused by impact smelting.

The clast of bulk basaltic composition has a large (~50 micron) grain of iron oxide, shown to be highly oxidized by X-ray absorption spectroscopy. An X-ray fluorescence map shows a trace of Ni throughout this grain, which implies it's been changed by impactor material. We did an X-ray diffraction map across that grain and the d-spacing peaks show a close match with a goethite standard. Goethite is an orthorhombic mineral, so to see how well it matched we calculated the unit cell size and plotted those dimensions against a database of goethite reference materials, which confirmed the identification.

Two veins cross-cut this goethite grain, and contain calcium carbonate from terrestrial weathering. The fact that they cross-cut the goethite means that the goethite formed earlier, which is likely on Mars, though we cannot rule out terrestrial weathering. Therefore we've sent the section to collaborators in Australia to obtain the deuterium/hydrogen (D/H) ratios, the heavy hydrogen to the lighter hydrogen, which has a different ratio in the Martian atmosphere and would distinguish it from terrestrial weathering.

How did goethite form on Mars? There are a couple of main reactions that could have taken place from magnetite or maghemite, which we know exist in the meteorite, and it could also form from pyrite. These are low-temperature water–rock reactions, and if they occurred on Mars, it suggests a post-impact hydrothermal system may have affected the breccia.

So to put this all together, there's the ancient precursor material, there's an impact formation event, probably 1·3 Gy ago, it has veins that have experienced melting temperatures, and pyroxene clasts with plagioclase feldspar borders; we've also got a breakdown of the pyroxene to iron oxide and amorphous aluminium silicate, which is an unusual anhydrous high-temperature reaction, but then we also have a lower-temperature late reaction creating goethite, which could have happened late in its history on Mars or possibly in the desert on Earth.

There's quite a lot of different literature coming out on this meteorite all the time: there are seven different pairs of this meteorite, so many different labs around the world have got samples to work on, and everybody's seeing different things to some extent, but the story is gradually becoming clearer as we go on.

Before I finish, I've got quite a round of thanks to express, not only to my collaborators at Leicester but to Ian Crawford for inviting me to speak here today and to RAS Council and to everyone who elected me to RAS Council; I'm very grateful for that experience and for learning so much about the Society.

The President. Thank you very much, Jane. Any comments or questions? Let me ask probably a very naïve question. You said there are now about a hundred identified Martian meteorites — are they still confirmed by the analysis of the trapped gas?

Ms. MacArthur. It is mainly oxygen isotope ratios that they look at now because, for heavy oxygen to light oxygen, all rocks on Earth plot on one line whereas all rocks on Mars tend to plot on another line. There's a slight anomaly in the oxygen isotopes on this meteorite as it happens that they're even higher than the normal Martian values, but they're closer to that than anything else, so the community is satisfied that this one is from Mars.

The President. So that's now a standard test?

Ms. MacArthur. Yes, the oxygen isotopes are the easiest.

Professor Sara Russell. You mentioned the possibility that the iron oxides could have been terrestrial, so I wonder if you could comment in general on how weathered this sample is?

Ms. MacArthur. We think we've got some relatively fresh fusion crusts, though someone else informed us that none of these have fusion crusts and it's more desert varnish, so it's difficult to know. But we don't see abundant calcium carbonates or anything. Another group thinks there's alteration to pyrites that happened on Mars and alterations to goethite that happened on Earth. In their sample, they've done the D to H ratio and proved that the goethite was from Earth. Ours was in a different context to theirs and the water has got to be somewhere. We know there's a high level of water and the original isotope analysis shows there is a strong Martian ratio in the water as well, so even if half the water might be terrestrial at least half of it must be Martian — it must be somewhere in this rock.

Professor I. Crawford. Jane, do I understand correctly that your ejection age from Mars was about 1·3 billion years ago?

Ms. MacArthur. No, sorry, I said that for the age of the nakhlites. For this meteorite, it's got a cosmic-ray-exposure age of about five million years. They think it travelled in space for about five million years before it landed on Earth, but they don't really know how long it sat on Earth for.

Mr. C. Taylor. Just a quick follow-up on the President's question. How secure is the identification that these objects are coming from Mars if it's based just on the O^{16}/O^{18} isotope ratio? Do we have surface analyses for the isotope ratios for any other solid surfaces? Could they have come from somewhere else — how secure is it?

Ms. MacArthur. I think it's secure because the other compositional data and everything else fits with what we know from Mars and what we know from the rovers and other things. As far as other surfaces go, I'm not entirely sure; there might be people that can answer that better than me, perhaps Sara [Russell].

Mr. Taylor. But is there other evidence on the issue, not just that isotope ratio?

Ms. MacArthur. Well, the other evidence being all the rest of the compositions found in the meteorite matching compositions we're seeing from the rover data and so forth.

The President. Sara, do you have a comment?

Professor Russell. We have loads of pieces of different bodies of the Solar System in the form of meteorites on Earth and each family has a distinctive isotope composition. You can't jump from one of the them to the other by mass fractionation — it's a mass-independent effect which suggests therefore different bodies. So I think it's our best guess that these objects are from Mars.

The President. What do you mean when you say many different bodies?

Professor Russell. Well, mostly asteroids; the only other planet we have are these bits of Martian meteorite, but we have bits of the Moon as well.

Ms. MacArthur. I should have brought my meteorite classification chart because you've got some of the L meteorites, the H, the double L, the enstatite chondrites. Basically each of those groupings has quite a big data sheet now, with 20 or 30 different groups, and they're all known to have similar characteristics to each other and different ones to the other sets.

The President. And is it still correct that you could just about get a piece of Venus to the Earth?

Ms. MacArthur. The escape velocities would be pretty hard, I think, but maybe it's not beyond all question. I'm hoping for one from Mercury to turn up. I think Mercury has a slightly better chance but of course things are more likely to chip into the Sun. Jupiter spikes things from the asteroid belt into the inner Solar System so they're more likely to go the wrong way from Mercury.

Dr. S. Jheeta. I refer to structures called ovoids and they are small marble-like beads reported in 2014 and they made big news worldwide. I wonder if you discovered or saw any nakhlites, nakhlite meteorites? I think they were discovered in nakhlites.

Ms. MacArthur. I haven't done any work on the nakhlites myself but my group has. Leon and John do a lot of work on the nakhlites but I think if they found any they'd be publishing about it.

Dr. Jheeta. They have been discovered — they were reported in 2014 and really did make big news.

Ms. MacArthur. I don't think they've found any more.

The President. Thank you very much indeed, Jane. [Applause.] I would just mention in passing, because Jane referred to it, the fact that she has been on RAS Council for the last two or three years. I just thought I would mention that some people maybe feel that to be on the Council you've got to be particularly old, a grey-haired professor like one or two [laughter]. She's a good example that that isn't the case and I think it's important to mention that. If there are any potential younger PhD students here, or that you are aware of, who have some experience and would be welcome additions to Council, please do encourage them to apply because we ought to have a very broad range of people and experiences on Council.

It's now my pleasure to introduce the 2017 Eddington Lecture. It will be given by Kathryn Johnston from Columbia University and the title is, as you can see, 'Physical manifestations of evolution, regularity, and chaos in and around our galaxy'. [Applause.]

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

The President. Thank you very much indeed, Kathryn. We now have some minutes for comments and questions.

Professor R. Ellis. According to the standard cold-dark-matter model, the halo is full of lumpy dark things, so-called missing satellites. These thin streams are surely a very valuable probe in testing whether the halo in the Milky Way is lumpy, so what are the prospects for using these streams as very-fine-grained tests of the distribution of matter in the halo?

Professor Johnston. I planted that question so now I'm going to start another hour-long talk. [Laughter.] As I'm sure you know, the prospects for that are very good but, of course, in my talk I pointed out yet more things that can affect this. The work that really needs to be done now is to try to understand how to disentangle those effects. Indeed, there have been a couple of analyses recently with hopeful signs that you may be able to use Palomar 5 to detect dark-matter structures. Unfortunately, part of the talk I didn't get to was another effect that can cause similar signatures. I think we want to find similar streams but at larger distances from the Galactic centre, and I'm still very hopeful that this is the way we're going to find those dark-matter structures.

Dr. F. Favata. I was struck by the lack of any mention of the existence of the *Gaia* mission which I would have thought would be at least of marginal interest to your studies.

Professor Johnston. Yes — *Gaia* is fantastic!

Dr. Favata. For example, you mentioned distances. You didn't specify but I assume you're talking about photometric distances, so not 'real' distances, and you didn't give us magnitudes, but picking up −2 absolute magnitude roughly brings us to between apparent magnitude 10 and 15 in the regions we're talking about. *Gaia* would give you geometric distances accurate to 10% from parallax. I would have thought that this would be of some interest to this field of studies and I am a bit surprised by your lack of mention of *Gaia*.

Professor Johnston. Yes, I apologise. [Laughter.] I probably offended the entire of this side of the Atlantic, so I'm glad *you* mentioned *Gaia*. On that note, I would say with regard to distances to the M giants that they are currently photometric only. I have been pointing out to my colleagues how useful proper motions will be in anticipation of the next data release, although it might be the one after because the current data release doesn't go that faint. Sergey Koposov and company are trying to use the current data release with SDSS to map the nearby features, but it doesn't actually go faint enough. Indeed, a lot of these questions will become really interesting.

Dr. Favata. That's what *Gaia* was built for.

Professor Johnston. Yes. [Laughter.] It's a very good point, I completely agree.

A Fellow. I have a question along the lines of the first one which is, if my memory is right and correct me if I'm wrong, dark haloes in cosmological simulations have a ring shape but on average they are prolate. I guess the question is, when you show the discrepancy between your oblate potential model and the tri-axial one, have you checked what should be the most common shape which is a prolate one? If yes or no, is the Milky Way special in any way or are the simulations wrong?

Professor Johnston. I think that's a great question and that's another talk to give which would be on measuring potentials. Indeed, the hope is, with *Gaia* and other measurements, that we will measure the shape of the halo as a function of galactocentric distance; in fact the *Gaia* proper motions for that problem are absolutely essential. I'm going to mention *Gaia* every second sentence. [Laughter.] I'm actually hopeful that if we get accurate distances from distance indicators, fantastic proper motions from *Gaia*, that we will measure the potential out to the virial radius. Then we can study tri-axiality, orientation, and changes in orientation as a function of radius. But we also have then to separate out the effects of sub-lumps and chaos.

Mr. M. Hepburn. You talk about chaos as if it was a thing in itself, whereas to most of us the general shape of the Galaxy looks statistically very highly determined with, as you move out from the centre, a smooth distribution of stars. Surely there is a determining factor which, in terms of the orbital speeds of stars about one another, should match the orbital speed of them around the centre of the Galaxy. As soon as you look at the Sun and α Centauri you see that the match is pretty close and broadly there is a standard distance between the stars. Surely, this is not something accidental, not the action of some mysterious thing called chaos. It's a second way of organizing the way in which a lot of stars come together.

Professor Johnston. When I was talking about chaos, I was really talking about the presence of chaotic orbits within a more-global well-determined structure. So you can have global structures that still contain some chaotic orbits. The interplay between the two is that the global structure itself determines the nature of the orbits, so even if there are only a few chaotic orbits, if you're able to locate those chaotic orbits, it actually says something about the global structure. It's not that the structure itself is chaotic, it's that it can contain chaotic orbits within it. I think we're talking at cross-purposes.

The President. Well you seem to have satisfied everybody, especially with the second mention of *Gaia*. [Laughter.] Can we all thank Kathryn very much indeed? [Applause.]

I told you before that I have a crib sheet here and it tells me to give notice that the next Open Meeting of the Society will be on Friday April 14th, which is wrong because this is one of the only occasions when we don't meet on the second Friday of the month because next month it falls on Good Friday. So for April please make sure to be here on the 7th, the first Friday. Finally, can I remind you that we have our normal drinks reception over in the RAS library and I look forward to meeting most of you over there.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2017 April 7 at $16^{h}\,00^{m}$
in the Geological Society Lecture Theatre, Burlington House

J. C. Zarnecki, *President*
in the Chair

The President. Good afternoon, ladies and gentlemen. Welcome to the April meeting of the Royal Astronomical Society. It is good to see people here on this first Friday of the month, which is a very unusual event for us of course. And now to proceed to the advertised programme, we start with a talk by Melanie Vandenbrouck of the Royal Museums Greenwich. Her title is, 'Whatever shines should be observed: astronomical prize medals'.

Dr. Melanie Vandenbrouck. None of the research for this talk would have been possible without the generous guidance and inexhaustible patience of Sian Prosser, the Royal Astronomical Society's librarian. Thanks are also owed to the Royal Observatory's Public Astronomer, Marek Kukula, for initiating me to the wonders of astronomy. As hand-held sculptural objects, prize medals do not only excite emulation and foster excellence, they are beautiful objects in their own right. This talk considers the creation of the first two medals of the Royal

Astronomical Society, the Gold Medal and the Jackson-Gwilt Medal. The first helped define the Society while the lengthy debates around the latter reveal the complexity of commissioning a medal that appropriately commemorates and encourages achievements in the specialized field of astronomy.

Inspired by the Royal Society's Copley Medal (from 1737), the creation of a medal that would reward contributions to and advances in astronomy was considered by the Astronomical Society of London almost immediately after its foundation in 1820 — although it took four years before its first recipients were awarded the highest honours of the new Society. It was designed by 'first medallist in England' George Mills (1792/3–1824), then modified by Chief Engraver to the Royal Mint William Wyon (1795–1851) after the society was attributed Royal Charter in 1831 and renamed the Royal Astronomical Society. It bears, on the obverse, the profile bust of Sir Isaac Newton, an apt embodiment of the ethos of the Society, for his discoveries had far reaching consequences for the field of astronomy. The import of his *œuvre* is summarized in the medal's inscription in exergue below the bust, *NUBEM PELLENTE MATHESI*, mathematics (science) dispelling clouds, that is, reason defeating ignorance. This aphorism formed part of the Astronomer Royal Edmond Halley's 'Ode to Newton', published as an epigraph to Newton's *Principia* (1687).

The reverse represents Sir William Herschel's Great Forty-Foot Telescope, then the largest in the world. The first president of the new Society, Herschel had died on 1822 August 25. While it remains to be found whether the reverse design was chosen before or after his death, the latter would signal the Society's desire to honour Herschel's legacy in a durable way. On the medal, it stands for the astronomer himself, his inventiveness in telescopic instrumentation, and his all-consuming scrutiny of the heavens. Herschel's motto above, *QUICQUID NITET NOTANDUM*, whatever shines should be observed, suggests the relentless 'sweeping' of the night-sky that led him to catalogue an unprecedented number of nebulae, star clusters, and double stars, and make some of the most provocative contributions to theoretical astronomy. In the Report of the Society's Third AGM on 1823 January 14, it was declared that "His name was so intimately connected with the progress of modern Astronomy, and the science had been so eminently enriched by his discoveries & researches, that it could not fail to shed a lustre upon the Society over which he presided". Significantly, the medal's reverse would become the emblem and motto of the Society.

The conception of the next RAS medal took some 36 years of gestation. It was made possible thanks to a gift in 1861 from a 'somewhat eccentric lady' of comfortable means and an astronomy enthusiast, Mrs Hannah Jackson-Gwilt, who had offered in 1861 the capital sum of £300 stock, its dividends to be used after her death for the award of a cash prize and medal. This, she specified, was to be awarded "for the encouragement from time to time of any person writing the best astronomical work, or in any other way advancing astronomy, either by the invention of a new astronomical instrument or by the discovery of a new heavenly body". Hannah Jackson-Gwilt died on 1893 December 1. By January 1895, a sub-committee was formed to consider the medal, and its commission. Its members were spectroscopy pioneer Sir William Huggins (1824–1910); Edward Walter Maunder (1851–1928) from the solar department at the Royal Observatory; the Astronomer Royal Sir William Christie (1845–1922); and Professor Herbert Hall Turner (1861–1930), Director of the Radcliffe Observatory in Oxford, the latter coordinating their views. The correspondence kept in the RAS archives also reveals that Huggins's wife, Margaret Lindsay Huggins, herself an astronomer, was instrumental in these debates.

Lengthy epistolary debates and meetings ensued, as to the design and designers of the medal, its material (eventually chosen to be bronze), size, and technique (cast rather than struck). The Hugginses proposed the sisters Nelia (1859–1950) and Ella Casella (1858–1946) as designers, an enlightened choice that demonstrates their familiarity with contemporary medallic circles. The Casellas had trained at the progressive Slade School of Art, where Professor Alphonse Legros had introduced medal-making in the syllabus, and initiated a resurgence of cast medals in England, in the tradition of the masters of the Italian Renaissance. Well-connected in London's artistic and intellectual circles, the Casellas enjoyed success as modellers in wax, and medallists, exhibiting regularly at the Royal Academy and other London galleries. These were important matters, as the Hugginses recommended "modelling of the highest order" for "a work of art worthy of the society".

Reaching consensus on the design proved the trickiest. The Hugginses declared the obverse should bear the portrait of an eminent astronomer, suggesting Nicolaus Copernicus, Johannes Kepler, or William Herschel. Turner himself proposed a portrait of Caroline Herschel, while Maunder expressed a preference for re-using Wyon's die for the Gold Medal (making the Jackson-Gwilt medal silver) or, failing this, was in favour of Caroline Herschel, rejecting Copernicus and Kepler as foreign astronomers. Caroline Herschel was a daring, if pertinent suggestion. 'Minding the heavens' as her brother's indefatigable astronomy assistant, she compiled a catalogue of the 2500 nebulae she observed with him, and achieved the colossal task of reorganizing their *General Catalogue* by position rather than class, which earned her the Gold Medal in 1828 (no woman would be awarded it again until Vera Rubin in 1996). But more than simply being her brother's amanuensis, she discovered no fewer than eight comets, became the first woman to be paid for her contribution to science, and to be named an Honorary Member of the Royal Astronomical Society in 1835 (with Mary Somerville). Interestingly, it was Margaret Huggins herself who rejected the suggestion of a female sitter, claiming that "surely on a medal of this kind there should be a portrait of a *master spirit*" [original emphasis]. The Committee eventually agreed on William Herschel, whose medallic features were to be inspired by a marble bust by John Charles Lochée then with the Herschel family.

Initially suggested to display an arrangement of laurels, title of the prize, and name of the recipient, it was eventually decided that the reverse would show an allegorical subject reflecting Herschel's significant contribution to astronomy. He had shot to astronomical fame for discovering Uranus in 1781, the first planet to be discovered since antiquity. His sweeping of the visible sky and study of deep-space objects already evoked on the Gold Medal, it seemed fitting that the new medal would concentrate on our Solar System. The medal, finished in 1897, shows the figure of Urania camped atop the globe of the Earth and holding an armillary sphere, against a background of stars with the Moon and Saturn. With its powerful lettering, compositional monumentality, and clarity of design, the medal reveals the artists' ability to translate the nature of a discipline and ethos of a society into something equally beautiful and inspiring, an appropriate reward to talented recipients. Herschel's profile displays the gravitas and the intense gaze of a man whose life was dedicated to the scrutiny of then unbelievably far and faint objects. The elegant simplicity of the stars and celestial bodies reveals that clarity may be found even in the most unfathomable darkness of the Universe. With its Pisanello undertones, but nods towards the future, this medal firmly places Herschel as a true successor of the great thinkers of the Renaissance and the Enlightenment, and its recipients as the heralds of the future.

If prize medals are about celebrating achievements, the medallists themselves often remain the unsung heroes behind their creations. It is remarkable that, although the Gold Medal and Jackson-Gwilt Medal are still awarded to this day as the RAS's most prestigious prizes, their design and designers are now largely eluding attention. While it is to be lauded that new awards are still being created, for these more recent medals, the 'artistry' that was so crucial to the Hugginses is now given little consideration. The RAS Eddington (1953), Chapman (1973), Herschel (1974), Patrick Moore (2012), and Annie Maunder (2017) medals show the competent skills and quality striking of the Royal Mint and Thomas Fattorini Ltd., but these are literal, mechanized transcriptions of existing portraits, and did not involve designers *per se*. They have become merely illustrative of achievements, rather than objects to be admired in their own right, that beg to be touched and proudly displayed, and used as prompts for elevated conversation. It is remarkable, too, that except for the recently created Annie Maunder Medal, which commemorates one of the Royal Observatory at Greenwich's 'female computers' and a pioneer of solar astrophotography, women are conspicuously absent from the faces of these medals. It is to be hoped that future astronomical prize medals will both call for the artistic skills of outstanding medal-makers, and for the long overdue representation, in medallic form, of Caroline Herschel, Mary Somerville, or indeed Margaret Huggins, and their celebrated successors.

The President. Thank you very much indeed. We have time for one question perhaps.

Dr. Vandenbrouck. Everybody is stunned by the medals!

Professor D. Lynden-Bell. Is the name of the recipient on the Jackson-Gwilt medal or not?

Dr. Vandenbrouck. That is a very good question. Margaret Huggins, who was full of good ideas, suggested that it ought to be on the rim of the medal, which in itself is quite a difficult achievement technically speaking. Now, I have only seen the primary model of the medal in the archive, and I haven't seen one of the awarded medals. So, if one of you has got his/her medal here with them tonight, I would love to see if the name of the recipient is on it [laughter].

The President. So we move to the second talk by Don Kurtz (University of Central Lancashire), the title, 'Asteroseismology: a new Keplerian revolution'.

Professor D. W. Kurtz. [Much of this talk was reported in *A&G* for August 2016, page 4.37.]

The President. Time for one question.

Dr. P. M. Allan. Thank you for a really interesting talk. You made the point that in a star/sun we don't see the g-modes because they don't get to the surface, but is there any coupling between the p-modes and the g-modes?

Professor Kurtz. We can't see that in the Sun, but we see it in abundance in main-sequence stars a little further up. For the one in which I showed you the internal rotations, I can see the p-modes and g-modes coupled quite easily, and in the red giants it's dominant. We get what we call mixed modes, which have the characteristics of both. So, the answer is yes, they definitely do couple.

The President. We must move on, I'm afraid. Thank you very much, Don. [Applause.]

Our next contribution is from Roger Walker of the European Space Agency. He gave a talk this morning at the very well-attended specialist meeting. His title is, 'The rise of CubeSats: from educational tools to operational systems'.

Dr. R. Walker. CubeSats are very small satellites, so-called nanosatellites because they typically have a mass of 10 kg and sizes of order 10 × 10 × 10 cm. They have only been launched in the last few years and were originated by Professor Bob Twiggs of Stanford University in 2001 as a means to educate students in space-systems engineering and space technology. To date more than 500 have been launched not only for educational but also other purposes. A one-unit CubeSat occupies a volume of about 1 litre and weighs about 1 kg. Two-, three-, and six-unit CubeSats have also been launched, and China launched a 12-unit set last year.

Why are people interested? The components used in CubeSats are from terrestrial electronics and sensors of the kind that appear in smart phones, for instance, so the miniaturization revolution has been exploited for space. Their advantages include very low cost and speed of manufacture and the relatively low cost of launches, since this is based on the weight of the payload. In addition, short development time-scales can be achieved with small teams over typical periods of one to two years. As CubeSats can piggyback on larger launch vehicles, there is a high availability of launch opportunities.

With the second generation of CubeSats, small industries are getting involved whilst a revolution in technical development in space systems is leading to the possibility of a third generation, not only for technical demonstrations, but small systems will be used in constellations in low Earth orbit for various purposes. It will soon be possible to do completely autonomous docking and fly small swarms, to act as an interferometer array, for example.

Recent investigations by NASA and ESA are looking at applications involving missions beyond low Earth orbit, such as small-body asteroid science. We can now control and stabilize CubeSats in three axes and also transmit in the X-band (8 GHz). Eventually they will be able to receive any sort of radio signal using software-defined radio-signal-processing techniques.

In 2017 August we will be launching CubeSats with propulsion on board which can manoeuvre and transmit data between themselves. There will be a hyperspectral imager which can take images in up to 40 spectral bands through the visual to the near IR with a spectral resolution of 10 nm. Another mission is *PICASSO* which will point an imager at the limb of the atmosphere when illuminated by the Sun at sunset and sunrise in order to check the absorption band to determine the vertical profile of stratospheric ozone to 5% accuracy. There will be a mission to measure total solar irradiance and the Earth radiation budget and compare the two to try and characterize the Sun–Earth radiation imbalance.

We are also planning missions to test future operational technology by *in-situ* measurement of the radiation environment and magnetic field in space. What makes all this possible is a revolution in attitude control. We have previously managed to point CubeSats with an accuracy of 10 or 20 degrees, whereas now with miniature star trackers and reaction wheels together we can get ±0·2 degrees. In communication, too, we are using higher frequencies so it's possible to get more scientific data back. Developments in cold-gas-propulsion technology include miniaturizing thrusters to give six degrees of freedom in control of satellites relative to other satellites. Electric-propulsion technology such as gridded ion thrusters can give small thrusts (of the order 1·7 milli-Newton) but with very high fuel efficiency. Such propulsion could change the velocity of a 12-unit CubeSat by 3 km sec^{-1}. If we wish to visit asteroids then we need substantially better navigation technology, and a miniature LIDAR system will allow us to measure distances when in close proximity to small bodies.

I have already mentioned multi-spectral imaging. The University of Leicester has an instrument for measuring NO_2 on the ground with a resolution of 500 metres that will allow accurate evaluation of the level of pollution in cities. By receiving signals from GPS and *Galileo* constellation satellites we can measure temperature, pressure, and humidity in the Earth's troposphere and thus feed-back data into numerical prediction models which can help to improve weather forecasts. By combining 30 or 40 spacecraft as a swarm orbiting the Earth we will get high spatial and temporal coverage, and such a swarm further away from Earth or in lunar orbit could do low-frequency (10–20 MHz) radio astronomy. We are also looking at the possibility of docking CubeSats in order to build up larger structures.

Beyond low Earth orbit we can piggyback CubeSats on missions to highly elliptical geostationary Earth orbit for astrobiology and measuring the effects of radiation in the space environment on biological samples. Beyond that there are the possibilities of missions to the Lagrangian points, the Moon, and the asteroids.

The President. Thank you very much for getting us back on schedule. There's one word you didn't use, which astronomers tend to use quite a lot, which is "aperture". Isn't that the fundamental limitation, at least for astronomical and some geophysical applications?

Dr. Walker. You are absolutely right. For astronomical purposes, aperture is a severe constraint for CubeSats, and I believe that is why we haven't seen so many applications in that domain. Simply put, the large telescopes would be everything you would want in one system. What may be possible in the future, I think particularly in radio astronomy, would be interferometric arrays, where we might see big swarms of these doing something that large, monolithic spacecraft could never achieve.

Mr. H. Regnart. Very impressive so far and tremendous potential I am sure, but there is inevitably an anxiety over the issue of orbital debris pollution.

Dr. Walker. Yes, I'm glad you mentioned that. Actually I was doing research in the orbital-debris domain for nearly a decade in the past, so I do know the subject very well. There is concern, of course, with the proliferation of the number of satellites; you are absolutely right. However, there have been actual studies performed with the debris-environment models looking at the long-term evolution of the space-debris environment. I actually developed a model myself in the past and published research on this subject in *Acta Astronautica* on the impact of nanosatellites on the space-debris environment. There have been some more recent studies than that, and the conclusion is, provided that these small satellites comply (as with the rest of the space missions) on post-mission lifetime of less than 25 years in low Earth orbit, we could actually launch up to about 300 to 400 per year without having a significant impact. So that is about the limit coming from the models today. We have had launches of multiple CubeSats: there has been one this year actually, where over 100 CubeSats have been deployed in one go, so we might start hitting that limit quite soon. That would be the time to become concerned and we will have to look at what extra measures would need to be taken to limit the impact. But for the time being, we are in not a bad situation.

Mr. I. Ridpath. You mentioned rendezvous with near-Earth objects, but would you actually have enough thrust to move any of them?

Dr. Walker. That is also an interesting question, because I also published a paper on that topic [laughter], again in *Acta Astronautica*. In order to deviate the trajectory of one of the large asteroids, say 100 metres in diameter, you would need a spacecraft of around 20 tonnes and you would have to generate a power of 200 kW; provided you could land that on the surface, you could generate enough thrust about 20 years in advance of any potential impact. That's about what we might expect from the so-called 'rendezvous, land, and push' scenario. There have been other studies also done — so-called 'kinetic-energy impactor' studies. That turns out to be more effective, but you need actually to do some quite exotic trajectories and again you need quite a lot of propulsion capability to deviate your trajectory such that you maximize the impact velocity and transfer as much kinetic energy as possible. But again we are talking about spacecraft of 10 to 20 tonnes, so it is really a different scale of things.

Mr. Ridpath. So you are not going to save the world after all?

Dr. Walker. I'm afraid not [laughter].

The President. On that happy note, thank you very much, Roger [applause].

Our final contribution today is from Charles Barclay, of Marlborough College and the University of Oxford, and his title is, 'The International Olympiad on Astronomy and Astrophysics'.

Mr. C. E. Barclay. I am very pleased to have been given this opportunity to raise the awareness of a new venture in astronomy education and one which I hope can be an inspirational aim for our brightest and best. This is not a typical talk for an Open Meeting nor, indeed, for me to deliver. I have no whizzy deep-space pictures to hide behind, nor animations of seismic resonances. This venture would not have got off the ground without the ideas and hard work of the committee and the admin team in the Oxford Physics Department. I would particularly like to thank Sandor Kruk, my co-team-leader, and Robin Hughes, our team observer, for editing this talk. My aim in this talk is to raise the profile of our competition and give you an idea of what has been achieved, and look forward to how we can reach out across the UK to unearth the best talent in schools, which we may not at present be accessing.

The International Olympiad on Astronomy and Astrophysics (IOAA) is now well established. Initiated in the Far-East in Thailand in 2007, it now has a growing membership with the first team from the African continent (Mali) competing this year in the 10th Olympiad in Bhubaneswar, India. Forty-two Countries took part this year; until 2015 the UK was not one of them. The Olympiad lasts for ten days and a great deal is made of the publicity locally with local or national government involvement in the opening and closing/medal ceremonies. The host country makes all the arrangements regarding local travel and accommodation and organizes setting and marking of the papers. Each team has a guide for both students and team leaders and, in Indonesia, each coach had a police escort. Every country enters five high-school/secondary-level students and two team leaders and one observer. The competition is highly valued and many countries choose from a vast number of initial entrants (as many as 100 000). Team members may be offered places at top universities based on their involvement alone (Iran). Some countries have training programmes throughout their school system and even specialist Olympiad schools. The competition itself is highly demanding and requires significant training. There are three exams: an observational round (two hours) consisting of naked-eye, aided, and planetarium papers; a theoretical round (five hours) with multiple-choice, short, and extended answers; and data analysis (four hours). The theory paper has a 50% mark weighting. The time allowance is very minimal and students have to work fast.

So, where does astronomy sit in our current education system? We are well catered for in comparison to some. Young children's interest is harboured at primary school with Solar System at KS2, and some elements are continued in KS3 Physics, but uniquely we do have a qualification at secondary level. The GCSE evolved out of the 'O' level and was historically taken by very small numbers from largely independent, 6th-form, and adult evening-class students. The specification evolved and a push was made to increase both knowledge of the GCSE and to help with the pragmatic implementation in centres. From 2003 to 2010 there was an exponential growth in entrants, primarily from the state sector, to a current 276 centres with 2500 candidates, over 71% from the state sector (23% Academies) with a number of religious centres from Jewish, Islamic, C of E, and Catholic persuasions. The majority of centres are from England with a small number from Wales, Ireland, and Scotland and a few from overseas. There are also a growing number of special-needs schools, and we have two home-educated and also two autistic centres. Many centres value the GCSE as a means of stretch and challenge and a way to harness enthusiasm. The lack of physics teachers, let alone astronomy teachers, as well as timetable restrictions, make this tough to initiate. The growth in popularity and the greater involvement of tertiary-level students and in outreach has, however, enabled support on a local basis with amateur-astronomy groups playing an important role too. Unlike physics, the profile of pupils is much more even, with many of the top students each year being girls. Indeed the GCSE is 40% female, 60% male. Only one exam board is involved and the exam is run by a small group who are closely linked to the research community, and the RAS has played a significant role as the specification has developed. Indeed, as well as Space Ambassadors, we have two FRAS chairs in astronomy education. This Society is pouring money *via* its RAS200 initiative into outreach projects aiming to widen the diversity of access to the world of astronomy. For many the excitement begins at a young age and to have an inspirational recognition for the best in the country seems an important next step.

I have been honoured to have served two terms as Chair of the Education and Outreach committee here in which time I have helped to establish not only Presidential Certificates for the best performing GCSE students but also awards for teachers in the form of the Patrick Moore Medal, now in its 6th year, and the Annie Maunder Medal for Outreach, being awarded this year for the first time, with its first recipient Marek Kukula. Though the idea of competing before in IOAA had been considered, it was not until 2014 that there was the idea of sending an observer to see what was involved. A committee was brought together and, under the umbrella of the established Physics Olympiad, it became clear that we might be able to take a trial team of three pupils to the 9th Olympiad in Central Java. Having no astronomy papers, we decided to select from the finalists of the British Physics Olympiad (BPhO) and once at the March training camp (the final 12) offered the possibility of IOAA or IPhO competitions. Three independent-school pupils with a keen interest in astronomy accepted.

We had a good idea of the competition from Sandor Kruk who had himself competed for the Romanian team from 2008 to 2010 and indeed won one of the top gold medals in his final year. With little time, we put together a training programme and, using papers and marking schemes *via* email, managed to prepare the three students enough to feel we could have a go in Summer 2015. The experience was incredible for all concerned and the organization of the event superbly managed. We were delighted with the result and two unexpected silver medals and accolades from the established countries for our IOAA debut, with UK ranked 10th overall. We now were able to plan ahead and quickly established a series of papers from which to select a team in 2016. We still decided to use the BPhO as the initial selection tool, of course a limiting step, as there is a restricted diversity among BPhO centres. Unfortunately, just as we had arranged a training programme, it was announced that the 10th Olympiad would take place in December (due to the weather conditions) and thus our team would have left school and would require absence from university. Luckily term had ended and, though training-camp time had to be found in their busy first-term schedule, all bar one (who was in the lower 6th during the competition phase in UK) were able to prepare and all happened to be together at Cambridge.

The team attended the annual award ceremony, now widened to include the BAAO, at the Royal Society, where each team member and the reserve were presented with binoculars (funded in 2015 by the RAS Education and Outreach Committee) and book prizes. Four boys and one girl made the team, with one from the state sector. Training continued until the last moment, with me working on projections of the celestial sphere (I handled the observational side) and Sandor (the theoretical side) briefing them on exam technique as we waited for internal flights in Delhi airport. The team leaders set up in a hotel some 50 km from the students. It is an incredible experience to share a room with astronomy educators from 42 countries, many of whom I had never spoken to before, and to see political barriers being broken down as we were all speaking the same language of astronomy. The days for the students were long but for the team leaders longer, with meetings starting at 9 a.m. and on several occasions continuing till 3 a.m., as papers were set and marking schemes argued over, before papers were translated and then printed and packed for the students to sit the next day. The marking is done in parallel by the local committee and the team leaders from each country, who enter their marks on-line before finding out the official mark. A hectic day is then spent in moderation, arguing every single mark for the team. Of course, there were some brief times to relax and visit local sites, for example the World Heritage Sun Temple at Konark.

I thought I ought to try and give a flavour of questions asked. Not only is the level high but the time pressure extreme. Very fast processing is needed. Here are some initial multiple-choice: true/false. (*a*) In a photograph of the clear sky on a Full Moon night, with sufficiently long exposure, the colour of the sky would appear blue as in daytime. (*b*) An astronomer at Bhubaneswar marks the position of the Sun on the sky at 05:00 UT every day of the year. If the Earth's axis was perpendicular to its orbital plane, these positions would trace out the arc of a great circle. (*c*) The centre of mass of the Solar System is inside the Sun at all times. (*d*) A photon is moving in free space. As the Universe expands its momentum decreases.

The theory paper has a very wide coverage of contemporary fields in astronomy and astrophysics: gases on Titan (r.m.s. speeds), early Universe (density parameter), shadow lengths (latitude variation), Cepheid pulsations (modelling), telescope optics (Barlow lenses), Mars-orbiter mission (orbit boosts), gravitational lenses (mass distribution), gravitational waves (merging candidates), exoplanet detection (transit curves with star spots and limb darkening).

The result for the whole team (1 gold (10th overall), 1 silver, 1 bronze, and 2 honourable mentions) was really exciting and again we felt all the efforts had been worthwhile. To come so high up the table of competing countries (6th behind only India, Iran, Russia, China, and USA), given our hasty preparation programme, was satisfying. Only 14 gold medals were awarded and 50% of those competing got no award at all.

We are currently entirely dependent on the financial support of the Physics Olympiad and need to look to self-sufficiency if we are to maintain an entry in IOAA. The generous sponsorship of the BPhO can, we hope, be continued but we need now to look wider. I would also like to acknowledge that this year we had private sponsorship from an RAS Fellow, which not only enabled support for the 2016 team but will also support future students. All expenses are covered, so entrants can be from any financial background, but extra costs are sometimes incurred, for example, medical inoculations. The sums involved are not huge and cover accommodation and travel to the training camps and the admin costs — also the entry to IOAA costs $1k per head; currently the total is £20k per annum. I am hopeful that the Society will be able to help support this venture which flies the flag for UK students in our subject on an international stage. I left the Oxford training camp yesterday where we have just selected the 2017 team. This time, due to the IOAA timing and difficulty with disturbing the university terms, we selected from current year-12 pupils. We are pleased to have one girl and two state-school pupils in the 2017 team who will be received at the Royal Society in May and compete in Phuket, Thailand, in November after months of further training.

Professor I. Roxburgh. Do you cover all the expenses for the students who might want to take to take part in the early stages?

Mr. Barclay. Absolutely, all of them. All expenses.

Professor Roxburgh. Good.

Dr. P. Daniels. Obviously for this to work and be really successful, the schools need to know about it. How do you let all the schools know, and how do you know that all the schools know about it?

Mr. Barclay. You've hit the nail on the head. I've been involved with this with the GCSE astronomy, and in fact in any area in education you send out leaflets to all the schools — it depends on whose desk they land on, and most of them go in the bin and they don't reach the right person. It is almost impossible to get messages out to schools. What one can do is to raise the PR. Now these are very early days, and it is partly a good idea to start here with the community, make sure people know, write in *A&G*. The idea now would be to write articles in the typical type of school magazines that are read: *Physics Education*, *School Science Review*, or whatever they happen to be. That is the key, to get the message out, because we want schools to know that this exists, and then we want to support them in getting pupils to do it.

Dr. Daniels. Amateur astronomical societies have a strong influence with schools and outreach with Cubs, Scouts, Brownies, and so on. Is there anything amateur astronomical societies can do to raise the profile?

Mr. Barclay. That is a very good point. I should say that there is a lot of involvement of universities and students and astronomical societies in helping with outreach in general. Certainly, with the GCSE astronomy and certainly when we have an individual who has made the UK team who needs to have observational experience, it would be fantastic if local astronomical societies got involved. I would be surprised if a local centre would not try and involve them, but you are absolutely right in terms of getting the whole thing known about, it is a good idea to get that message out to all of them as well. As you see it is very early days, and we wanted to see if we could do it as an overall thing, and we weren't going to make a fool of ourselves, and we were at the right level. And I think the answer is that we can.

Professor Kurtz. Who are the judges?

Mr. Barclay. That is quite complicated. The local organizing committee set the papers over a long period of time. They mark the papers, we mark the papers as well in parallel, then the two are compared. And actually one of the most critical days is the penultimate day where we have a moderation session where we have ten minutes with every single exam setter/marker to argue the marks through. Once the marks are agreed and you sign off that you are happy with the marks, hopefully there are no errors made but sometimes people haven't seen the working (actually some of our pupils get answers that are better than the ones they have produced). This illustrates how important it is for pupils to structure their answers. Everything is done as a committee of 42 countries. Why these meetings go on so long is because we argue over every word of every question and every mark scheme, and it is done as a committee. And there is an organizing committee that has a final say.

The President. Thank you very much indeed, Charles. [Applause.]

That concludes our meeting for today. May I remind you of our drinks reception in the RAS library immediately following this meeting. And finally I give notice that the next monthly meeting of the Society will be on Friday the 12th of May 2017, following the AGM.

ON THE INTRODUCTION OF THE TERM 'GIANT' INTO STELLAR ASTRONOMY

By Graeme H. Smith
University of California Observatories/Lick Observatory

"These two classes of stars were first noticed by Hertzsprung[1], who has applied to them the excellent names of 'giant' and 'dwarf' stars." (Henry Norris Russell[2].)

Introduction

The introduction of the terms 'giant' and 'dwarf' into stellar astronomy have often been attributed by both astronomers and historians to Ejnar Hertzsprung. For example, in the 1950s, Abetti[3], Eggen[4], and Parenago[5] all dated the terms to early papers by Hertzsprung[1,6] from 1905 and 1907. Perhaps most influentially, Henry Norris Russell in writings as early as 1912 attributed the terms to Hertzsprung[7]. One notable instance can be found in Russell's work[8] of 1913 published in *The Observatory*, wherein is the first use of the paired terms 'giant' and 'dwarf' stars within the title of an astronomical paper. In addition, the above quote was obtained from a 1914 article by Russell. Textbooks and other monographs from the period 1917–1930 typically attributed the terminology to Hertzsprung and sometimes Russell[9–12].

However, authors such as Lundmark[13], Strand[14], Gingerich[15], and Dick[16] have pointed out that neither the terms 'giant' nor 'dwarf' appear in the earliest writings of Hertzsprung[1,6,17] on stellar luminosities. Strand goes further, stating that Hertzsprung in his 1905 and 1907 papers demonstrated that "among the stars of spectral types later than G there were two series, which for the same spectral class differed in absolute magnitude. [He] concluded that the real reason could be large radii of the luminous red stars. He therefore did not use and did not approve of the terminology of giants and dwarfs." Additionally, Gingerich[15] quotes correspondence in which Hertzsprung writes: "I hasten to say that I have avoided the expressions 'giant' and 'dwarf', because the stars are not very different in mass, but in density." In a footnote within a 1915 paper[18] Hertzsprung does in fact refer to 'giant' yellow stars and 'dwarf' stars, but no reference to this terminology is given. He again uses the terms in a 1917 paper[19], by which time it had also been adopted by other authors such as Eddington[20,21], Fowler[22], Plummer[23], and Adams[24].

It is the introduction of the term 'giant' into astronomical nomenclature that is the subject of this note. The earliest reference to a 'dwarf' star is harder to discern. Both Gingerich[15] and Dick[16] have noted that use of the term 'giant' can be dated at least as early as a 1908 lecture by Karl Schwarzschild entitled *Uber das System der Fixsterne*[25]. The published version of that lecture makes reference to '*Giganten*' stars, but does not contain the term 'dwarf'. As a result, Gingerich[15] surmised that "it is quite possible that Russell himself invented the paired usage of both giant and dwarf." Alternatively, Dick[16] suggested that "Hertzsprung may have used the terms in correspondence with Schwarzschild, rather than in his early papers."

DeVorkin[26,27] in his reviews of the origins of the Hertzsprung–Russell diagram has pointed out that the terminology of 'giant' stars goes back to at least the writings of John Ellard Gore[28]. Even Gore's writings, however, do not constitute the first published appearance of terms such as 'giant suns' or 'giant stars' in the astronomical literature. It is a purpose of the present paper to point out that another popularizer of astronomy in the Victorian era, Richard Anthony Proctor, may also have contributed to introducing the terms 'giants' and 'dwarfs' into astronomical discussions. In addition, attention is drawn to several other writings that make mention of 'giant' stars prior to 1912–1914 when Russell[2,8,29] started to popularize that term. The term 'giant' as denoting a type of star with a radius much larger than that of the Sun seems to have made an appearance in the astronomical literature nearly 20–30 years prior to the publication of the first 'Russell' diagram.

John Ellard Gore's giant suns

DeVorkin[26,27] has traced the roles of W. H. S. Monck and John Ellard Gore[30,31] in recognizing the existence of stars much larger than the Sun. Of these two, it was the Irishman Gore who actually used the term 'giant' in his writings in reference to stars of size much larger than the Sun. Holberg[32] and Shears[33] have pointed out that Gore referred to both 'giant' and 'miniature' suns. For example, in Gore's 1893 book[28] *The Visible Universe*, Chapter 12 is entitled 'Giant and miniature suns' and describes some stars as being 'giant suns', or as being in a 'giant' category. Could the chapter title be a possible forerunner of the dichotomous 'giant–dwarf' terminology that Russell came to use so frequently? In some respects, Gore's work on binary-star observations and the compilation of parallax information from which to derive stellar luminosities can be seen as a forerunner to the extensive programmes of Russell. In fact, in a footnote to Fitzgerald's[31] article about Gore, the editor of the *Irish AJ* noted that one of Gore's papers[34] had compiled sufficient data to permit an early plot of an H–R diagram, although Gore did not do so.

Gore's earliest use of the term 'giant' extends at least as far back as 1890. A volume of the *Journal of the British Astronomical Association* published in that year[35] contains a report on a paper by Gore published earlier in *The Journal of the Liverpool Astronomical Society* that is entitled 'Giant Suns'. Gore writes within: "So when the parallax of Arcturus is compared with that of Sirius, the latter should be 511 times brighter than the former, but it is really only three times brighter. Arcturus must therefore be enormously larger, for its spectrum being of the 2nd or Solar type, it is highly improbable it can be intrinsically brighter. Among these Giant Suns, besides Arcturus ...". Here we see the recognition of stellar 'giants' in the modern sense as being stars of large radius, not just high luminosity.

Gore would publish these ideas in subsequent books[28,36]. In *The Visible Universe*, referred to above, for example, we find the following quotes: "Measures of stellar distance do not, however, lend much support to this apparently reasonable hypothesis of equable distribution and size among the starry hosts", ... "there can be little doubt, I think, that Arcturus is a far larger body than our Sun", ... "Another star which may also be classed as a giant sun is α Cygni (Deneb)."

The *Simbad* database[37] lists Deneb as having a spectral type of A2 Ia. Among other members of what Gore called "the giant class" he included Capella (G1 III + K0 III), Vega (A0 V), Rigel (B8 Iae), α Cephei (A8 V), Pollux (K0 III), β Ursae Minoris (K4 III), Betelgeuse (M1–M2 Ia–ab), and Canopus (A9 II). The spectral types noted here from *Simbad*, confirm most, but not all, of those stars as substantially more luminous than the main sequence. Ten years later Gore[36] would write in another monograph: "For Arcturus, ... at Yale Observatory a parallax of 0″·024 was found. If this minute parallax is anything near the truth, Arcturus must be a sun of gigantic size."

In his books and talks, Gore's use of the term 'giant' often (but not always) conveyed a similar sense to that adopted by Russell some 10–20 years later, *i.e.*, some of Gore's 'giant' stars were of comparable colour and spectral class to solar-like stars, but were of much larger radius. Gore's use of the term 'giant' precedes that of Russell, Hertzsprung, and Schwarzschild. However, at least one astronomer was referring to 'giant' stars or suns prior to the writings of Gore. That astronomer was Richard A. Proctor.

Richard Proctor's giant suns

When it comes to use of the adjective 'giant' to describe a star, examples can be found in the writings of Richard Proctor that precede the quotations noted above from the works of J. E. Gore. The contexts in which Proctor employed this adjective, however, are not quite as consistent with modern usage as in Gore's writings.

One instance of Proctor's popular writings from 1874 reveals his recognition of stars larger than the Sun. In *The Universe and Coming Transits*[38] he writes in regard to stars for which Secchi had found spectra similar to Sirius, Vega, and Altair: "stars belonging to this particular type are certainly in many cases, and probably in all very large orbs." Two years later, the term 'giant sun' makes an appearance in one of Proctor's books *Our Place Among Infinities*[39].

That monograph, which is a series of essays, has a chapter entitled 'A Giant Sun'. The chapter is dedicated to describing the properties of Sirius, including evidence that it has changed colour, that it is a "giant sun" in size, and has a faint companion. Following the discovery of the Hertzsprung–Russell diagram, it was recognized that Sirius, Vega, and Altair are not what would today be called giant stars. They are rather main-sequence stars hotter and larger than the Sun, with spectral types (from *Simbad*) of A1 V + DA, A0 V, and A7 V, respectively. Those stars do not attain the large radii of the coolest types of 'giant' stars that J. E. Gore would subsequently write about. However, Gore did include Vega in his 'giant' category, so even his concept of that term overlapped to some extent with that of Proctor.

Perhaps the most notable quotes from Proctor's writings come from an 1883 book entitled *Mysteries of Time and Space*[40]. In the chapter entitled 'Star unto star' we have a detailed exposition on the possible relative sizes of stars as a function of three major colour groupings and spectral information from Huggins, Secchi, and Draper. Proctor contemplated the "bluish-white stars of which Sirius, Vega, Altair, and others are typical". The only one of those for which a measured parallax was available at the time was Sirius "a yellowish-white star ... [that] is demonstrably a much larger orb than our own sun, if the quantity of light which a sun emits is an indication of size". Regarding the "brighter stars of the bluish-white kind ... we may confidently say that these stars are very much larger than our own sun". Proctor estimated the diameter of Sirius to be 14 times that of the Sun, based on a comparison of luminosities between the Sun and Sirius. He noted the "great breadth and strength of the hydrogen lines in these monstrous suns (suns exceeding our own sun much as our sun exceeds Jupiter and Saturn ... ", and went on further to write that "the darkness of the hydrogen lines is a characteristic of stars at a much higher temperature than our sun and suns of the same class". Proctor argued that "all stars having these very strong hydrogen lines are very much larger than our own sun". Clearly what Proctor had in mind in the context of a 'giant' sun is a star that is both much hotter and larger than the Sun. Then he goes on to make a very relevant statement[40]: "So this is the case before us. Some bluish-white suns with spectra indicative of stellar youth are no doubt enormously large orbs, compared with which our sun is little more than as a dwarf compared with a giant."

Might this be the first appearance of the paired 'dwarf'–'giant' terminology in the astronomical literature? In Proctor's view the Sun is a dwarf by comparison with what are now recognized as larger main-sequence stars.

Two years later Proctor[41] suggested that comets "were originally ejected, not from suns like our own, but from the giant suns like Sirius, Vega, Altair, and others". His idea was reported to a meeting of the Royal Astronomical Society held in 1885 May, during which "Mr. Knobel read a paper from Mr. R. A. Proctor". The records[42] of the meeting reveal several uses of the term "giant suns", with Sirius being in mind as an example of such a star. Proctor's giants were hotter, larger, younger versions of the Sun. It is not clear that he ever thought of giant stars as being comparable or cooler to the Sun in temperature and colour. It is possible that it was Proctor who introduced the term 'giant', and maybe 'dwarf', into astronomical nomenclature, although what he had in mind for the former is not the type of yellow or red 'giant' stars that were discussed by Gore, and fully recognized from the work of Hertzsprung and Russell.

Richard A. Proctor

A definitive biography of Proctor (1837–1888) does not appear to be available, but a number of sources[43–50] give some details of his career and contributions to astronomy. Born in 1837, Proctor graduated in 1860 in mathematics from St. John's College at Cambridge. His first writings upon astronomy started to appear about five years later, and in 1866 he gained election as a Fellow of the Royal Astronomical Society, soon becoming a Secretary of that Society. At that point Proctor took to the occupation of an independent writer, and began a prodigious output that would eventually amount to over fifty books[50]. He engaged in writing monographs on astronomy at the popular level for the public, as well as original research works on subjects such as the rotation period of Mars[51], the distribution of stars within the Milky Way[52], and transits of Venus[53]. He argued for the impact origin of lunar craters[54,55]. Proctor spent considerable time from the mid-1870s to mid-1880s engaged in international lecture tours that included not only England, but the United States (where he settled in 1884), Australia, and New Zealand. The US tours seem to have been particularly successful and many of the talks were delivered to full venues[49,56].

Within an account of Proctor's early career up to 1873, J. Fraser[43] states "Ten years ago, the name of Richard Anthony Proctor was absolutely unknown; ... to-day it is familiar as household words to every educated man in England, and to many thousands in this country. ... Barely eight summers have flown since his maiden work appeared, and in the interval he has contributed a score of volumes to the library of science, some of them profound, many of them entirely original, and all of them thoroughly elevating ... As an Astronomer and Mathematician he stands in the front rank of scientists."

A variety of other quotes can be given that attest to a substantial output both in books and magazines, and on public lecture tours. One example is: "His industry was prodigious. He may almost be said to have laid down his pen to enter the lecture room, and to have quitted the platform to resume his pen again."[44] Willard[45] quotes "Professor Young" (presumably Charles Augustus Young) as stating that as "an expounder and popularizer of science, he [Proctor] stands, I think, unrivaled in English literature".

Such statements might to some extent reflect a genteel hyperbole not uncharacteristic of the Victorian era. However, it is not inconsistent with the assessment of historian Lewis Saum[47], who offers a detailed study of Proctor's public visibility in the United States and the manner in which North American newspapers reported upon his circumstances. The introduction to Saum's essay seems relevant here: "By the mid-1870s Richard Anthony Proctor, having risen to prominence in English scientific circles, began visiting America on lecture tours devoted largely to his astronomical studies. Though almost totally forgotten now, except in specialized writing, Proctor loomed large in his time, and in the scientific efflorescence for which it is remembered."

The appearance of a somewhat caricatured portrait of Proctor in an 1883 issue of *Vanity Fair*[57] as part of a feature article entitled 'Man of the Day', would seem to confirm a notable level of recognition.

Proctor's work, therefore, was of a very public nature, and as such he may have played no small role in introducing the concept of 'giant' stars, and possibly 'dwarf' stars, to the astronomical literature and the general public of the Victorian age.

Stellar giants elsewhere in pre-1912 literature

Whether they were influenced by Gore or Proctor, a number of authors took up usage of the term 'giant' in the 1880s and 1890s, although there is little evidence for comparable use of the term 'dwarf'. One of the earliest examples is the reference by Wilson[58] in 1886 to the "giant star Sirius". Wilson's terminology here seems to be in the context of Proctor's work rather than the wider and more modern usage of Gore. Another such example is Maunder's[59,60] discussion of stars of the first and second Secchi type: "So it may well turn out to be the case that there is only a superficial resemblance between the spectra of giants, like Sirius and Vega, and of the little stars which swarm in the galaxy, ..."*. Maunder states that Proctor "often spoke" of such stars as "giant suns", and comments that this "was a practice in which many other writers have imitated him". Here is additional evidence that the term 'giant' was being adopted by astronomical writers of the Victorian era who were being influenced by Proctor.

It is in the mid-1890s that the term takes on more of the connotation intended by Gore. In discussing the great intrinsic luminosity of Rigel based on the then-measured parallax of that star, Agnes M. Clerke wrote in 1890[61]: "we are only beginning our acquaintance with the giant suns of space. Rigel is not unique in its magnificence; it has equals, if not superiors, in Betelegeux, Canopus, and Arcturus." Clerke was an influential popularizer of astronomy in the 1890s and 1900s. She also referred to 'giant' stars elsewhere[62], including a book entitled *Problems in Astrophysics*[63]. Her writings are evidence that by the mid-1890s the idea of referring to large stars as 'giants' was becoming established.

Also helping to cement this usage may have been the writing of Mary Proctor, daughter of Richard Proctor, and a noted popularizer of astronomy in her own right. For example, in an 1897 article that appeared as part of a regular series entitled 'Evening with the Stars', Miss Proctor[64] muses in regard to the star Capella that its "constitution as revealed by the spectroscope, resembles that of our Sun, but the Sun would make but a sorry figure if removed to the side of this giant star".

In context with his 1886 quote noted above, Wilson's[65] reference in 1904 to Aldebaran as "a giant among suns" and elsewhere as a "giant Sun" is now in keeping with Gore's non-main-sequence usage. By 1907 we have Lanhau[66] writing that "Canopus and Arcturus — are vaster in volume than our immense Sun", and referring to them as "those remote giant stars". Bryant[67] in a 1907 text on the history of astronomy also comments upon "giant stars". By this time we are getting close to Schwarzschild's[25] '*giganten*', which begs the question of whether Schwarzschild himself got the notion from Hertzsprung, or from the earlier usages that had been appearing before 1908 in both astronomical journals and popular reference books.

One particularly interesting reference from 1911 occurs in a large article edited by Schlesinger[68] entitled 'Correspondence concerning the classification of stellar spectra'. This collection of correspondence describes the responses of many astronomers to various questions posed to them about stellar spectral classification. In his contribution Russell refers to "Hertzsprung's giant and dwarf stars". Here is clear evidence that Russell himself was using the paired terms at least two years before his 1913 and 1914 papers in which he first presented his magnitude *versus* spectral-class diagram[2]. Thus it may well be,

* Might here also be an early allusion to the Sun as a dwarf star?

as suggested by Dick[16], that before those terms appeared in the published works of Russell and Hertzsprung, both scientists were using them in written correspondence or talks*. As a consequence, in writings from 1910 and 1911 that refer to 'giant' stars[71,72], it is hard to discern whether the terminology has been influenced by Russell and Hertzsprung, or the earlier work of Gore, Proctor, and others†.

Conclusion

Although we cannot prove that Richard Proctor was the first astronomer to write and talk about so-called 'giant' stars, evidence has been presented above that is consistent with such a surmise. In Proctor's terminology a 'giant' sun was a star that was significantly larger than the Sun, and as such, he felt justified in applying the term to main-sequence stars like Sirius and Vega that are more massive than the Sun. However, following the recognition of giant and dwarf sequences in the Hertzsprung–Russell diagram, a giant star came to mean one that is much larger than a main-sequence (dwarf) star. In this context it was arguably Gore, not Proctor, who pioneered the use of the term 'giant' in a modern sense. Both astronomers evidently had an influence on astronomical writings of the later Victorian era, and in the period from 1887–1907 their use of the term 'giant' seems to have been parallelled, or adopted, by other authors.

One of the most influential text-books on astronomy during the early years of the 1900s was the *Manual of Astronomy* by Charles A. Young[74]. In that volume it is said of the stars that "Small as they appear to us, they are many of them immensely larger and hotter than the sun; others, however, are smaller and cooler, and some hardly shine at all. They differ enormously among themselves in mass, bulk, and brightness, ... differing as widely as whales from minnows."

That statement calls to mind Hertzsprung's later metaphor for Antonia Maury's so-called c-stars[75] (now classified as supergiants) which he likened to whales amongst the fishes[76]. Whether the actual term 'giant' was being used or not, astronomers were evidently quite used to the notion of such stars prior to the researches of Hertzsprung and Russell. It would, however, be hard to deny that it was the work of those two giants of astronomy that firmly entrenched 'giants' and 'dwarfs' in the astronomical landscape, both as concepts, and as accepted terminology in stellar astrophysics.

Acknowledgements

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References

(1) E. Hertzsprung, *Zeitschrift für Wissenschaftliche Photographie*, **3**, 442, 1905.
(2) H. N. Russell, *Popular Astronomy*, **22**, 275, 1914.

*The same could also be surmised of Proctor and Gore's earlier use of the term 'giant', making it hard to determine when those two authors first originated their terminology. See DeVorkin[69,70] for numerous illustrations of the type of correspondence that was being exchanged between Russell, Hertzsprung, Pickering, and other stellar astronomers between 1900 and 1913.

†In Russell's extensive 1910 paper[73] on stellar parallaxes in which he discussed the existence of two distinct size classes among the coolest stars in his programme, there are no anthropomorphisms used in regard to those classes.

(3) G. Abetti, *The History of Astronomy* (Sidgwick & Johnson, London), 1954.
(4) O. J. Eggen, *ASPL*, **7**, 233, 1956.
(5) P. P. Parenago, in J. L. Greenstein (ed.), *The Hertzsprung-Russell Diagram*, Proceedings of IAU Symposium No. 10, *Annales d'Astrophysique*, Supplement No. 8 (CNRS, Paris), p. 11, 1959.
(6) E. Hertzsprung, *Zeitschrift für Wissenschaftliche Photographie*, **5**, 86, 1907.
(7) H. N. Russell, *Proceedings of the American Philosophical Society*, **51**, 569, 1912.
(8) H. N. Russell, *The Observatory*, **36**, 324, 1913.
(9) J. Jeans, *Problems of Cosmogony and Stellar Dynamics* (Cambridge University Press), 1919.
(10) H. Dingle, *Modern Astrophysics* (Collins & Sons, London), 1924.
(11) H. N. Russell, R. S. Dugan & J. Q. Stewart, *A Revision of Young's Manual of Astronomy. II. Astrophysics and Stellar Astronomy* (Ginn & Co., Boston), 1927.
(12) J. C. Duncan, *Astronomy* (Harper Brothers, London), 1927.
(13) K. Lundmark, *Handbuch der Astrophysik*, **5**, part 1, p. 210, 1932.
(14) K. Aa. Strand, in A. G. Davis-Philip & D. H. DeVorkin (eds.), *In Memory of Henry Norris Russell, Dudley Observatory Report*, **13** (Dudley Observatory, Schenectady), p. 55, 1977.
(15) O. Gingerich, in M. J. Way & D. Hunter (eds.), *Origins of the Expanding Universe: 1912–1932*, ASP Conference Series Vol. 471 (Astronomical Society of the Pacific, San Francisco), p. 205, 2013.
(16) S. J. Dick, *Discovery and Classification in Astronomy: Controversy and Consensus* (Cambridge University Press), 2013.
(17) E. Hertzsprung, *Pub. Ap. Obs. Potsdam*, **22**, Nr. 63, 1911.
(18) E. Hertzsprung, *ApJ*, **42**, 92, 1915.
(19) E. Hertzsprung, *ApJ*, **45**, 285, 1917.
(20) A. S. Eddington, *The Observatory*, **36**, 467, 1913.
(21) A. S. Eddington, *MNRAS*, **77**, 596, 1917.
(22) A. Fowler, *The Observatory*, **38**, 379, 1915.
(23) H. C. Plummer, *MNRAS*, **76**, 121, 1915.
(24) W. S. Adams, *PNAS*, **2**, 152, 1916.
(25) K. Schwarzschild, *Himmel und Erde*, **21**, 433, 1909.
(26) D. H. DeVorkin, *Physics Today*, **31**, 32, 1978.
(27) D. H. DeVorkin, in O. Gingerich (ed.), *The General History of Astronomy. Volume 4. Astrophysics and Twentieth-Century Astronomy to 1950: Part A* (Cambridge University Press), 1984.
(28) J. E. Gore, *The Visible Universe* (Crosby Lockwood & Son, London), 1893.
(29) H. N. Russell, *Nature*, **93**, 252, 1914.
(30) H. MacPherson, *Popular Astronomy*, **18**, 519, 1910.
(31) A. P. Fitzgerald, *IrAJ*, **7**, 213, 1966.
(32) J. A. Holberg, *BAAS*, **39**, 873, 2007.
(33) J. Shears, *JBAA*, **123**, 85, 2013.
(34) J. E. Gore, *MNRAS*, **65**, 264, 1905.
(35) J. E. Gore, *JBAA*, **1**, 94, 1890.
(36) J. E. Gore, *Studies in Astronomy* (Chatto & Windus, London), 1904.
(37) M. Wenger, *et al.*, *A&AS*, **143**, 9, 2000.
(38) R. A. Proctor, *The Universe and the Coming Transits* (Longmans, Green & Co., London), 1874.
(39) R. A. Proctor, *Our Place Among Infinities* (D. Appleton & Co., New York), 1876.
(40) R. A. Proctor, *Mysteries of Time and Space* (R. Worthington, New York), 1883.
(41) R. A. Proctor, *MNRAS*, **45**, 405, 1885.
(42) Anonymous, *Astronomical Register*, **23**, 125, 1885.
(43) J. Fraser, *Scribners Monthly, an Illustrated Magazine for the People*, **7**, 172, 1873. https://todayinsci.com/P/Proctor_RichardA/ProctorRichardA-BioScribners.htm
(44) W. Noble, *The Observatory*, **11**, 366, 1888.
(45) C. R. Willard, *Popular Astronomy*, **1**, 319, 1894.
(46) C. W. Moulton, (ed.), *The Library of Literary Criticism of English and American Authors. Vol. VII. 1875–1890* (Moulton Publishing Co., Buffalo New York), p. 659, 1904.
(47) L. O. Saum, *American Studies International*, **37**, 34, 1999. http://www.ianridpath.com/atlases/Proctor_in_America.PDF
(48) Anonymous, *Observer*, **1**, Issue 13, 1880. https://paperspast.natlib.govt.nz/newspapers/TO18801211.2.36
(49) Anonymous, *The New York Times*, September 3, 1888.
(50) A. C. R. [A. C. Ranyard], *MNRAS*, **49**, 164, 1889.
(51) R. A. Proctor, *MNRAS*, **27**, 309, 1867.
(52) R. A. Proctor, *MNRAS*, **33**, 539, 1873.
(53) R. A. Proctor, *MNRAS*, **38**, 406, 1878.
(54) R. A. Proctor, *The Moon: Her Motions, Aspect, Scenery, and Physical Conditions* (Alfred Brothers, Manchester), 1873.
(55) C. Koeberl, *Earth, Moon and Planets*, **85–86**, 209, 2001.
(56) Anonymous, *The Argus*, May 24, 1880. http://trove.nla.gov.au/newspaper/article/5972393

(57) L. Ward, (aka Spy), *Vanity Fair*, March 3, 1883.
(58) H. C. Wilson, *Sidereal Messenger*, **5**, 257, 1886.
(59) E. W. Maunder, *JBAA*, **2**, 35, 1891.
(60) E. W. Maunder, *Astronomy & Astro-Physics*, **11**, 145, 1892.
(61) A. M. Clerke, *The Observatory*, **13**, 313, 1890.
(62) A. M. Clerke, *The Observatory*, **25**, 357, 1902.
(63) A. M. Clerke, *Problems in Astrophysics* (Adam & Charles Black, London), 1903.
(64) M. Proctor, *Popular Astronomy*, **4**, 515, 1897.
(65) H. C. Wilson, *Popular Astronomy*, **12**, 611, 1904.
(66) J. P. Lanhau, *Popular Astronomy*, **15**, 390, 1907.
(67) W. W. Bryant, *A History of Astronomy* (Methuen & Co., London), 1907.
(68) F. Schlesinger, *ApJ*, **33**, 260, 1911.
(69) D. H. DeVorkin, in A. G. Davis-Philip & D. H. DeVorkin (eds.), *In Memory of Henry Norris Russell*, Dudley Observatory Report, **13** (Dudley Observatory, Schenectady), p. 61, 1977.
(70) D. H. DeVorkin, *Henry Norris Russell: Dean of American Astronomers* (Princeton University Press, Princeton), 2000.
(71) Anonymous, *Popular Astronomy*, **18**, 447, 1910.
(72) Anonymous, *The Observatory*, **34**, 125, 1911.
(73) H. N. Russell, *AJ*, **26**, 147, 1910.
(74) C. A. Young, *Manual of Astronomy, A Text-Book* (Ginn & Company, Boston), 1902.
(75) A. C. Maury, *Harvard Annals*, **28**, 1897.
(76) E. Hertzsprung, *AN*, **179**, 373, 1909.

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 256: HD 147250, HD 151446, HD 151448, AND HD 157540

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The stars discussed in this paper — as in several recent papers in this series — are all nominally ninth-magnitude late-type stars that were initially observed in the course of the 'Clube Selected Areas' programme of radial-velocity measurements. HD 151448, only, features in the original paper[1] on that programme; the others were included later, when the programme was expanded by deeming certain of the Areas to be considerably increased in size. The first three of the stars discussed in this paper, in order of RA and of *HD* number, are in Area 1 of the Selected Areas, near RA 16½ hours, declination +30°. They have orbital periods of about 6·21, 3·84, and 4·91 years, respectively, determined in each case to within a few days. HD 157540, which is near 50° declination, is in Area 2, and has a much shorter period of about 106 days, known to within a few minutes. The mass functions are all quite small, and the secondary component has not been observed in any of the systems. The orbital eccentricities are also moderate, except in the case of HD 147250, whose eccentricity is very nearly 0·9.

Introduction

The 'Clube Selected Areas'[1] are a set of 16 small areas of sky systematically situated in terms of Galactic coördinates. The Clube programme has been briefly described in the introductory texts of a number of recent papers[2] in this series, as well as being set out in the original, more specific, description of it elsewhere[1]. Some of the Areas are too far south to observe from Cambridge, and were observed[3] with the Geneva Observatory's *Coravel* on the Danish 1·5-m telescope at ESO. The richness of the *HD* catalogue in the southern hemisphere, and the good observing conditions at ESO, resulted in an imbalance in the numbers of stars measured in the two hemispheres; redress was sought by increasing the sizes of the northern Areas, thereby bringing a lot of previously un-observed stars into the programme.

Orbits have already been presented for well over 100 of the Clube stars, either in the series of papers of which *this* one is a member or else in the RAS *Monthly Notices*[4], and four more are given below. Only one of the four stars discussed in this paper (HD 151448) has had any radial velocity published for it previously. That star featured in the original paper[1] on the Clube Areas; there were six measurements of it, not in good mutual accord but not quite scattered enough to warrant a conclusion of real variation. The other three stars treated here were not part of the Clube programme until the northern Areas were enlarged, and their radial velocities have until now been unknown. All four orbits have quite small mass functions and the stars are observationally single-lined.

HD 147250

HD 147250 is a star of almost exactly the ninth magnitude, to be found in the constellation Corona Borealis, some degrees to the east of the conspicuous 'coronet' and close to the boundary with Hercules; it is nearly 1° south-preceding the 5^{m} K-type giant ξ CrB. We are indebted to *Tycho 2*[5] for determining its magnitude and colour index to be $V = 9^{m}{\cdot}03$, $(B - V) = 0^{m}{\cdot}85$. The only other information that is reliably known about it seems to be its *Hipparcos* parallax of $0''{\cdot}00478 \pm 0''{\cdot}00108$, which shows it to have a distance of about 210 pc (though with an uncertainty of more than 20%) and thus a distance modulus of about $6^{m}{\cdot}6$. Its absolute magnitude is therefore about $+2^{m}{\cdot}4$ with an uncertainty of about $0^{m}{\cdot}4$ — distinctly on the faint side of normal-giant luminosity. In accordance with one of the selection criteria for Clube stars, its spectral type as given in the *Henry Draper Catalogue* is K0, but a 'spectral type' estimated on the basis of the *Hipparcos* colour index and parallax would be about G6 III–IV.

The first radial-velocity observation of HD 147250 was made at Cambridge in the summer of 2003. The next, a year later, was in modest (2 km s^{-1}) disagreement, and was supported by a third measurement made soon afterwards. At the start of the following season, however, an altogether unambiguous change had taken place, and the object was transferred to the binary-star watch list. It was observed at intervals of two or three weeks whenever it was reasonably accessible in the sky, and showed a progressive change. By late 2009 it had recovered the velocity found at the initial observation, and the six-year period became apparent. The star has been retained on the writer's observing programme and there are now 72 radial velocities* available; they are set out in Table I. After the first orbital cycle had been witnessed and the very high eccentricity of the orbit (almost 0·9) was recognized, most of the sudden

*One, which gives an exceptional residual, in excess of 1 km s^{-1}, is rejected.

TABLE I

Radial-velocity observations of HD 147250

All the observations were made with the Cambridge Coravel

Date (UT)			*MJD*	*Velocity* $km\ s^{-1}$	*Phase*	*(O–C)* $km\ s^{-1}$
2003	Aug.	3·96	52854·96	+12·2	0·743	+0·1
2004	Sept.	15·88	53263·88	+14·4	0·923	+0·3
	Oct.	7·80	285·80	+13·9	·933	–0·4
2005	May	12·09	53502·09	–0·1	1·029	–0·1
		29·04	519·04	+1·4	·036	+0·3
	June	23·01	544·01	+2·3	·047	–0·1
	July	16·95	567·95	+3·4	·058	+0·2
		28·95	579·95	+3·3	·063	–0·3
	Aug.	15·89	597·89	+4·2	·071	+0·2
	Sept.	7·83	620·83	+4·4	·081	–0·1
		23·87	636·87	+5·1	·088	+0·3
	Oct.	20·77	663·77	+4·9	·100	–0·4
	Nov.	16·72	690·72	+5·4	·112	–0·3
2006	Mar.	2·21	53796·21	+6·8	1·158	0·0
	Apr.	5·11	830·11	+7·3	·173	+0·3
	May	11·05	866·05	+6·7	·189	–0·6
	June	12·02	898·02	+7·8	·203	+0·3
	July	15·03	931·03	+7·8	·218	+0·1
	Sept.	10·88	988·88	+8·2	·243	+0·1
	Nov.	18·73	54057·73	+8·5	·274	+0·1
2007	Mar.	27·13	54186·13	+8·8	1·330	–0·2
	May	15·07	235·07	+9·2	·352	0·0
	July	8·02	289·02	+9·4	·376	0·0
	Sept.	6·84	349·84	+9·6	·403	0·0
	Nov.	15·73	419·73	+9·5	·434	–0·3
2008	Apr.	8·14	54564·14	+9·7	1·497	–0·6
	June	26·01	643·01	+10·6	·532	+0·1
	Aug.	30·87	708·87	+10·5	·561	–0·2
	Oct.	21·78	760·78	+11·0	·584	+0·1
2009	Feb.	7·27	54869·27	+11·5	1·632	+0·3
	Apr.	22·09	943·09	+11·9	·664	+0·4
	June	17·04	999·04	+12·3	·689	+0·7
	Aug.	15·89	55058·89	+11·9	·716	+0·1
	Oct.	8·77	112·77	+12·5	·739	+0·5
	Nov.	17·73	152·73	+11·8	·757	–0·4
2010	Mar.	23·17	55278·17	+12·8	1·812	+0·1
	May	12·06	328·06	+13·1	·834	+0·2
	July	6·02	383·02	+13·2	·859	0·0
	Sept.	14·86	453·86	+13·6	·890	0·0
	Oct.	27·76*	496·76	+12·7	·909	–1·2
	Nov.	6·73	506·73	+13·3	·913	–0·7
		10·73	510·73	+14·2	·915	+0·2
2011	Jan.	19·27	55580·27	+14·0	1·946	–0·5
	Mar.	14·23	634·23	+14·9	·970	0·0
	Apr.	7·16	658·16	+14·5	·980	–0·2
	May	10·10	691·10	+8·9	·995	+0·2
		11·10	692·10	+8·1	·995	0·0
		12·09	693·09	+7·7	·995	+0·4

TABLE I (*concluded*)

Date (UT)			*MJD*	*Velocity* $km\ s^{-1}$	*Phase*	*(O–C)* $km\ s^{-1}$
2011	May	13·05	55694·05	+6·7	1·996	+0·1
		15·08	696·08	+4·2	·997	−0·5
		19·05	700·05	+0·1	·999	−0·1
		21·03	702·03	−2·1	·999	+0·1
		24·03	705·03	−5·1	2·001	+0·2
		25·02	706·02	−6·5	·001	−0·4
		31·98	712·98	−7·9	·004	+0·3
	June	2·97	714·97	−7·9	·005	+0·2
		4·03	716·03	−7·9	·006	0·0
		8·04	720·04	−7·6	·007	−0·5
		10·03	722·03	−6·4	·008	+0·2
		17·01	729·01	−5·0	·011	+0·1
		20·03	732·03	−4·5	·013	0·0
		26·98	738·98	−3·5	·016	−0·2
	July	24·91	766·91	−0·3	·028	−0·2
	Sept.	12·84	816·84	+2·8	·050	+0·2
2013	Apr.	6·15	56388·15	+8·5	2·302	−0·2
	May	9·09	421·09	+9·4	·317	+0·6
2014	Feb.	26·23	56714·23	+9·7	2·446	−0·2
	Apr.	8·17	755·17	+10·1	·464	+0·1
	May	15·10	792·10	+9·8	·481	−0·4
		31·06	808·06	+10·3	·488	+0·1
	July	21·93	859·93	+10·5	·510	+0·1
2016	Apr.	17·15	57495·15	+12·6	2·791	+0·1

*Omitted in the solution of the orbit.

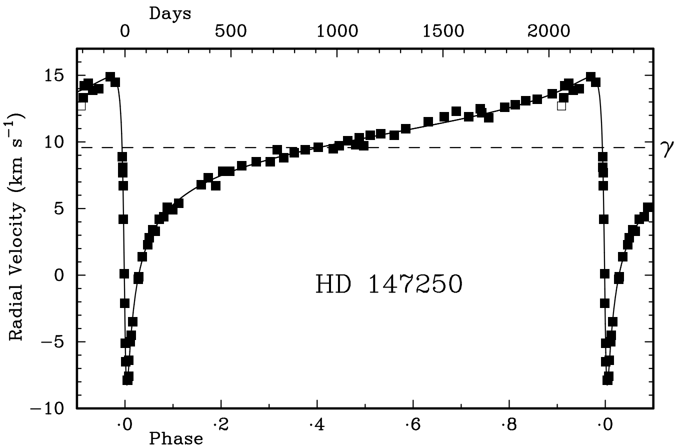

FIG. 1

The observed radial velocities of HD 147250 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. All of the 72 radial-velocity observations were made by the writer with the Cambridge *Coravel*; one, which has the largest residual by a considerable margin and is plotted with an open symbol, has been rejected.

decline and minimum in velocity that marked the second periastron passage, in 2011, was carefully watched. The character of the orbit is demonstrated by Fig. 1, and the elements are set out in the first numerical column of Table V towards the end of this paper. The high eccentricity has allowed the period to be determined to little more than one part in a thousand.

HD 151446

This star is just over 2° south-following the $3\frac{1}{2}^{\rm m}$ G-giant star η Herculis; M13, the great Hercules globular cluster, lies only 1° south-preceding. The *Tycho 2*[5] magnitudes of HD 151446 are $V = 8^{\rm m}{\cdot}63$, $(B - V) = 1^{\rm m}{\cdot}44$. The star was not on the *Hipparcos* programme, so we have not got an objective measure of its distance or absolute magnitude. The colour index, however, is too large to belong to a K dwarf; it is that of a late-K giant, so we may tentatively conclude that HD 151446 is a giant star with a spectral type that is probably near K5 rather than the K0 given in the *Henry Draper Catalogue*. The rather deep dips seen in radial-velocity traces are characteristic of a star of such a type. No other astrophysical information is available; there are only two papers listed for HD 151446 by *Simbad*, both referring to its proper motion, which is unremarkable.

Radial-velocity observations of HD 151446 were begun at Cambridge in 2003, soon after it became apparent that more stars should be added to the 'Clube' programme in the northern hemisphere to balance the richness in the south. It is the writer's deliberate practice normally to wait a year before making a second observation, with a view to allowing even fairly slow spectroscopic binaries to 'show their hand'; in the case of HD 151446 that tactic paid off, and resulted in the discovery of the binary nature of the star in 2004 at only the second observation. Systematic observations then brought to light the orbital period of about four years. Although the declination of the star is such that it is not quite circumpolar at Cambridge, observations have been made in every calendar month except January, and now number 57. They are listed in Table II, and readily yield the orbit whose elements are given in Table V and which is plotted in Fig. 2. The velocity amplitude is seen to be quite modest, and the corresponding smallness of the mass function accordingly does not encourage any expectation that the secondary star should be detectable in the radial-velocity traces.

The 'dips' in the traces show very little broadening arising from rotation of the star. Rotational velocities are routinely derived from every radial-velocity trace; they are not determined to arbitrarily high precision but are quantized at half-km s^{-1} eigenvalues, and in the case of HD 151446 the resultant $v \sin i$ estimate of an absolute majority of them has been 0·5 km s^{-1}. That is compatible with a rotation synchronized with the orbital period, but the probably small mass of the secondary star does not encourage the supposition of a captured rotation.

The telescopic field of HD 151446 includes another star, HD 151501, of rather similar brightness but less red, about 6′ south-following; its photometry and *Hipparcos* parallax indicate it to be of near-solar type. A radial-velocity measurement was inadvertently made of it on 2006 Nov. 1·79, when it was mistaken for HD 151446; the velocity was +17·8 km s^{-1}. By way of certifying its identity, it was deliberately re-observed shortly afterwards, on 2006 Nov. 6·77, the result then being +18·2 km s^{-1}.

TABLE II

Radial-velocity observations of HD 151446

All the observations were made with the Cambridge Coravel

Date (UT)	*MJD*	*Velocity* km s^{-1}	*Phase*	*(O–C)* km s^{-1}
2003 Aug. 29·92	52880·92	−0·5	0·738	+0·5
2004 Nov. 4·78	53313·78	−6·9	1·047	+0·2
14·72	323·72	−7·1	·054	+0·1
2005 May 5·07	53495·07	−5·6	1·177	+0·3
29·07	519·07	−5·5	·194	+0·1
June 23·01	544·01	−4·6	·212	+0·6
July 17·96	568·96	−4·6	·230	+0·3
Aug. 15·92	597·92	−4·9	·250	−0·4
Sept. 12·88	625·88	−4·1	·270	0·0
Oct. 25·79	668·79	−3·3	·301	+0·2
Dec. 8·71	712·71	−3·2	·332	−0·3
2006 Mar. 23·17	53817·17	−1·8	1·407	0·0
Apr. 26·07	851·07	−1·0	·431	+0·5
May 30·03	885·03	−1·0	·455	+0·2
June 22·02	908·02	−0·9	·472	+0·1
July 17·95	933·95	−0·8	·490	0·0
Aug. 7·93	954·93	−0·6	·505	+0·1
Sept. 10·86	988·86	−0·3	·529	+0·2
Oct. 4·88	54012·88	−0·5	·547	−0·1
Nov. 6·77	045·77	−0·5	·570	−0·2
Dec. 2·71	071·71	−0·3	·589	0·0
2007 Feb. 4·27	54135·27	−0·4	1·634	−0·1
Mar. 27·15	186·15	−0·4	·670	0·0
Apr. 30·10	220·10	−0·9	·695	−0·4
May 23·08	243·08	−0·8	·711	−0·1
June 21·00	272·00	−0·5	·732	+0·4
July 18·96	299·96	−0·8	·752	+0·3
Aug. 30·93	342·93	−1·7	·782	−0·1
Sept. 22·85	365·85	−2·0	·799	−0·1
Oct. 17·79	390·79	−2·0	·816	+0·2
Nov. 15·74	419·74	−3·1	·837	−0·4
2008 Mar. 5·22	54530·22	−4·4	1·916	+0·4
Apr. 8·15	564·15	−5·1	·940	+0·3
May 19·05	605·05	−6·1	·969	0·0
June 26·03	643·03	−6·6	·997	0·0
July 21·92	668·92	−6·5	2·015	+0·4
Sept. 26·87	735·87	−7·5	·063	−0·3
Oct. 24·78	763·78	−7·5	·083	−0·4
Nov. 25·73	795·73	−7·2	·106	−0·3
2009 Feb. 4·27	54866·27	−6·0	2·156	+0·3
Oct. 22·78	55126·78	−2·8	·342	0·0
Nov. 17·73	152·73	−2·9	·361	−0·4
2010 Nov. 15·72	55515·72	−0·4	2·620	−0·1
2011 June 17·05	55729·05	−1·3	2·772	+0·1
Sept. 28·80	832·80	−3·0	·846	−0·1
Nov. 9·73	874·73	−3·8	·876	−0·1
Dec. 3·71	898·71	−4·4	·893	−0·3

TABLE II (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2012 Feb. 19·25	55976·25	−5·7	2·949	−0·1
Aug. 15·93	56154·93	−7·3	3·076	−0·2
Nov. 5·76	236·76	−6·8	·135	−0·2
2013 June 4·08	56447·08	−3·9	3·285	−0·1
Sept. 14·85	549·85	−2·9	·358	−0·4
Oct. 16·76	581·76	−2·1	·381	0·0
2014 Sept. 9·88	56909·88	−0·3	3·615	0·0
Nov. 5·75	966·75	−0·5	·656	−0·2
2015 July 7·98	57210·98	−2·7	3·831	−0·1
Aug. 26·91	260·91	−3·6	·866	−0·2

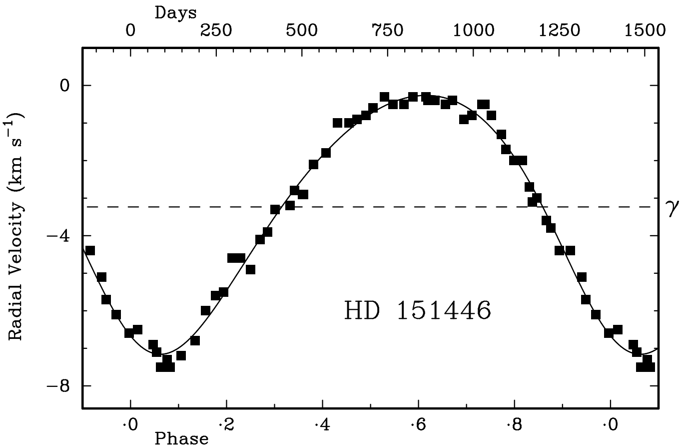

FIG. 2

The observed radial velocities of HD 151446 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. All of the 57 radial-velocity observations were made by the writer with the Cambridge *Coravel*.

HD 151448

This is the only one of the stars treated in this paper that was on the 'Clube' programme right from the outset: it was one of the 406 objects that featured in the paper[1] published some 30 years ago giving the radial velocities of all those stars. It is to be found just over 1°, in p.a. about 80°, from the 6^m star 39 Her, a well-known double-lined binary system[6–8] consisting of a pair of F stars in an orbit with a period of only 2·3 days. The *Tycho 2* magnitudes of HD 151448 are $V = 8^m{\cdot}67$, $(B - V) = 1^m{\cdot}16$. The star gives good deep dips in radial-velocity traces, persuading the writer that it is a giant — a conclusion reinforced (albeit not conclusively) by the smallness of its proper motion. If it is indeed a giant, then its colour index indicates a type close to K1 III. The *only* paper retrieved for it by *Simbad* is the present writer's own[1], to which reference has already been made. That paper presented six measurements, obtained in 1970–76 with the original radial-velocity spectrometer[10] at Cambridge, of the star's radial velocity; they had a range of nearly 4 km s^{-1}, which was not regarded at the time as quite enough to warrant designation of the star as a spectroscopic binary.

After the southern Clube Areas had been diligently observed from ESO, the northern Areas were re-observed with the then-new *Coravel*-type radial-velocity spectrometer at Cambridge, beginning in 2003. With the new instrument, velocity changes that could pass for observational error with the original spectrometer became significant, and although it was not until the fourth season that the measured velocities fell distinctly outside the range of the original six, it became clear then that they referred to a binary system. Continued systematic observations have revealed the orbit to have a period of about 5 years, and to be of very modest amplitude and eccentricity. The observations are listed in Table III, and the orbital elements are in Table V; the orbit is plotted in Fig. 3. As in the case of HD 151446, a majority of the radial-velocity traces indicates a rotational velocity of 0·5 km s^{-1}, which is compatible with the rotation being synchronized to the orbital motion but does not by any means demand it.

TABLE III

Radial-velocity observations of HD 151448

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

Date (UT)	*MJD*	*Velocity* *km s^{-1}*	*Phase*	*(O – C)* *km s^{-1}*
1970 Aug. 2·95*	40800·95	−47·9	0·525	−1·1
1973 Sept. 14·85*	41939·85	−50·6	1·162	−0·5
1975 Aug. 3·94*	42627·94	−46·8	1·546	0·0
26·88*	650·88	−47·0	·559	−0·2
1976 July 31·93*	42990·93	−49·7	1·749	−1·7
Aug. 19·88*	43009·88	−47·6	·759	+0·5
2003 Aug. 29·92	52880·92	−48·1	7·274	+0·2
2004 Nov. 4·78	53313·78	−46·8	7·516	0·0
2005 May 12·09	53502·09	−47·3	7·621	−0·4
June 1·00	522·00	−46·7	·632	+0·3
Aug. 21·96	603·96	−47·2	·678	+0·1
2006 July 15·97	53931·97	−49·6	7·861	+0·3
Aug. 7·93	954·93	−50·1	·874	+0·1
Sept. 10·85	988·85	−50·9	·893	−0·3
Oct. 26·76	54034·76	−51·2	·919	−0·1
Nov. 18·73	057·73	−51·2	·932	+0·2
2007 Mar. 27·15	54186·15	−52·0	8·003	+0·3
Apr. 30·11	220·11	−52·0	·022	+0·3
May 23·09	243·09	−52·4	·035	−0·2
June 21·00	272·00	−52·1	·051	0·0
July 18·96	299·96	−51·5	·067	+0·4
Aug. 26·95	338·95	−51·6	·089	−0·1
Sept. 22·85	365·85	−51·5	·104	−0·3
Oct. 17·77	390·77	−51·1	·118	−0·1
Nov. 15·74	419·74	−51·2	·134	−0·5

TABLE III (*continued*)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O–C) km s⁻¹*
2008 Mar. 5·23	54530·23	−49·0	8·196	+0·5
Apr. 24·11	580·11	−49·0	·223	0·0
June 26·03	643·03	−48·5	·259	0·0
Aug. 30·89	708·89	−47·9	·295	+0·1
Sept. 26·87	735·87	−47·9	·310	0·0
Oct. 24·78	763·78	−47·4	·326	+0·3
2009 Feb· 4·27	54866·27	−47·2	8·383	0·0
Mar. 27·19	917·19	−47·2	·412	−0·1
Apr. 29·12	950·12	−46·8	·430	+0·2
May 23·12	974·12	−46·8	·444	+0·1
June 17·04	999·04	−47·0	·458	−0·1
July 22·97	55034·97	−46·6	·478	+0·2
Aug. 28·85	071·85	−46·7	·498	+0·1
Oct. 8·77	112·77	−47·1	·521	−0·3
2010 Mar. 23·19	55278·19	−47·0	8·613	−0·1
May 12·10	328·10	−46·8	·641	+0·2
22·07	338·07	−46·8	·647	+0·3
June 23·00	370·00	−47·2	·665	0·0
Aug. 15·92	423·92	−47·2	·695	+0·2
Sept. 14·87	453·87	−47·4	·712	+0·2
Oct. 16·74	485·74	−47·5	·729	+0·2
Nov. 15·73	515·73	−48·0	·746	−0·1

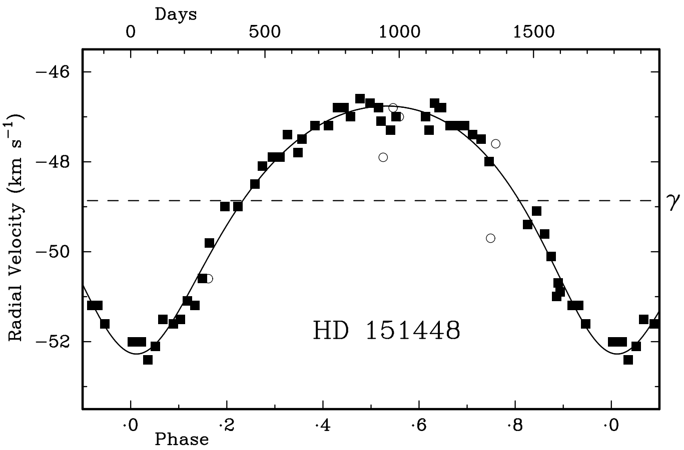

FIG. 3

The observed radial velocities of HD 151448 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. Six measurements made with the original radial-velocity spectrometer[10] in 1970–76 are plotted as open circles; forty years ago their scatter did not seem quite large enough to warrant the conclusion that the velocity was really variable. After renewed interest was taken in the northern Clube Areas in 2003, HD 151448 was re-observed with the new and more accurate *Coravel* spectrometer; it was more than three years before the measured velocity fell outside the range of the 1970s observations, but by then it had become apparent that the velocity *did* change, and 52 measurements have been made with the *Coravel*. They have been given unit weight in the solution of the orbit, whereas those made with the original spectrometer have been weighted 1/10.

TABLE III (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2011 Apr. 7·18	55658·18	−49·4	8·826	−0·2
May 10·12	691·12	−49·1	·844	+0·4
July 25·02	767·02	−51·0	·887	−0·6
28·97	770·97	−50·7	·889	−0·2
Nov. 9·73	874·73	−51·6	·947	+0·1
2012 Nov. 5·76	56236·76	−50·6	9·149	−0·2
Dec. 1·70	262·70	−49·8	·164	+0·3
2013 Oct. 28·74	56593·74	−47·8	9·348	−0·3
Nov. 12·72	608·72	−47·5	·357	−0·1
2014 Oct. 7·81	56937·81	−47·3	9·541	−0·5
27·74	957·74	−47·0	·552	−0·2

*Observed with original Cambridge spectrometer; weight ¹⁄₁₀

HD 157540

This star is at a higher declination (about +51°) than the other three, and features in Area 2 of the Clube Areas whereas the other three are all in Area 1. Like so many other stars, it was added to the observing programme when the northern Areas were enlarged in order to embrace more stars eligible for the Clube programme, for comparability with the southern Areas where the number of eligible stars per square degree is much higher. So whereas HD 151448, which was on the programme from the beginning, was first observed in 1970, HD 157540 (like the other two stars discussed in this paper) was not observed until after work started on the newly enlarged northern Areas in 2002.

In contrast to the stars treated above, the last two of which have colour indices that are unexpectedly large for stars that according to the *HD* are of type K0, HD 157540 is extraordinarily blue for a star of such a type: Oja[9] has given its V magnitude as 8·90 and its $(B - V)$ colour index as only $0^{m}{\cdot}45$. The star was observed by *Hipparcos*, which found a parallax of $0''{\cdot}00945 \pm 0''{\cdot}00072$, equating to a distance modulus of $5^{m}{\cdot}1$ and thus implying an absolute magnitude of +3·8. Both the absolute magnitude and the colour index are those of a main-sequence star of type F6 or F7. The proper motion of HD 157540 is misleadingly small (less than $0''{\cdot}005$ per annum in each coördinate), and in the absence of parallax information would make it seem likely that the star would be of higher luminosity. The radial-velocity traces exhibit a 'dip' whose equivalent width (expressed exactly like that of a line in a spectrum) is just under 2 km s^{-1}, whereas the dips of the rest of the stars discussed in this paper are all much larger — about 4 km s^{-1} in the case of HD 147250 and 5½ in the cases of the other two stars. The weakness of the dip in traces of HD 157540 is in keeping with the unusually early type implied by both the absolute magnitude and the colour index.

The first radial-velocity observation of HD 157540 was made at Cambridge in the summer of 2002; the next, almost a year later, disagreed with it by 7 km s^{-1}, so the star was transferred to the binary programme. When a further small discordance was found after only ten days the object was regarded with some priority, and 22 further observations were obtained before the end of the calendar year, by which time the star was out of reach in the evening sky but its 106-day period had been discovered and was already quite well determined.

TABLE IV

Radial-velocity observations of HD 157540

All the observations were made with the Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2002 Sept. 10·95	52527·95	−20·7	0·334	−0·5
2003 Aug. 19·97	52870·97	−27·9	3·566	+0·3
29·87	880·87	−30·0	·660	−0·3
Sept. 14·87	896·87	−29·3	·810	−1·1
23·87	905·87	−20·7	·895	+0·1
24·83	906·83	−18·6	·904	+0·7
Oct. 3·85	915·85	+4·2	·989	0·0
7·93	919·93	+9·8	4·028	−0·4
9·88	921·88	+9·2	·046	−0·3
11·88	923·88	+8·1	·065	+0·8
14·83	926·83	+3·5	·093	+0·4
16·79	928·79	−0·8	·111	−1·1
17·81	929·81	−1·4	·121	−0·2
18·77	930·77	−1·6	·130	+0·9
27·75	939·75	−12·6	·215	−0·6
29·82	941·82	−13·9	·234	−0·2
Nov. 1·75	944·75	−14·7	·262	+1·1
2·76	945·76	−16·4	·271	0·0
3·74	946·74	−16·8	·280	+0·2
4·75	947·75	−17·2	·290	+0·5
5·77	948·77	−17·9	·300	+0·3
7·80	950·80	−19·5	·319	−0·2
26·73	969·73	−26·0	·497	+0·5
Dec. 7·73	980·73	−29·6	·601	−0·7
18·73	991·73	−29·9	·704	0·0
2004 Apr. 7·16	53102·16	−28·2	5·745	+1·5
20·11	115·11	−24·1	·867	+0·3
22·15	117·15	−21·2	·886	+0·9
May 19·08	144·08	−4·2	6·140	−0·3
22·05	147·05	−6·9	·168	+0·4
23·03	148·03	−8·8	·177	−0·5
June 13·03	169·03	−22·6	·375	−0·5
15·03	171·03	−22·8	·394	+0·2
17·05	173·05	−24·0	·413	−0·3
19·01	175·01	−23·9	·432	+0·5
22·03	178·03	−25·5	·460	−0·1
27·97	183·97	−26·3	·516	+0·7
July 9·94	195·94	−29·1	·629	+0·2
Aug. 1·93	218·93	−25·9	·846	+0·4
10·99	227·99	−13·5	·931	+0·2
12·95	229·95	−8·7	·949	−0·2
18·99	235·99	+8·5	7·006	+0·4
29·91	246·91	+0·1	·109	−0·5
Sept. 7·85	255·85	−9·3	·193	+0·7
8·85	256·85	−10·9	·203	0·0
Nov. 4·81	313·81	−29·5	·740	+0·3
13·78	322·78	−28·6	·824	−1·0
19·78	328·78	−22·5	·881	+0·4
2005 May 15·08	53505·08	−27·6	9·542	+0·1
June 9·02	530·02	−30·2	·777	−1·0
27·00	548·00	−8·7	·947	+0·6
28·01	549·01	−7·0	·956	−0·6
July 17·00	568·00	−3·1	10·135	+0·1
Sept. 12·95	625·95	−30·1	·681	−0·3
23·86	636·86	−29·6	·784	−0·5
Oct. 27·78	670·78	+1·6	11·104	+0·2

TABLE IV (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)* *km s⁻¹*
2006 May 11·09	53866·09	−11·0	12·944	−1·0
17·08	872·08	+7·0	13·001	0·0
Aug. 28·91	975·91	+1·3	·979	+0·2
2007 May 8·10	54228·10	−21·4	16·356	−0·1
June 28·01	279·01	−26·6	·836	+0·3
July 7·04	288·04	−16·7	·921	−0·6
8·04	289·04	−14·0	·930	−0·2
12·92	293·92	+1·1	·976	+1·0
31·91	312·91	−5·9	17·155	−0·1
2009 June 2·07	54984·07	−26·7	23·480	−0·7
July 22·98	55034·98	−5·9	·960	−0·7
24·06	036·06	−2·0	·970	−0·1
2010 May 12·12	55328·12	−30·3	26·722	−0·4
2011 Aug. 17·03	55790·03	+5·5	31·075	−0·3
2013 May 3·11	56415·11	−3·3	36·966	−0·1
5·13	417·13	+2·7	·985	−0·2
July 8·02	481·02	−28·7	37·587	−0·1
Sept. 16·88	551·88	−15·0	38·255	+0·3
Oct. 6·79	571·79	−25·9	·443	−1·1
Dec. 4·72	630·72	+7·0	·998	+0·6
2014 June 13·10	56821·10	−27·8	40·792	+1·1
Sept. 22·84	922·84	−29·7	41·751	0·0
2015 May 19·12	57161·12	+6·3	43·996	+0·3
2016 July 19·00	57588·00	+9·3	48·019	−0·5

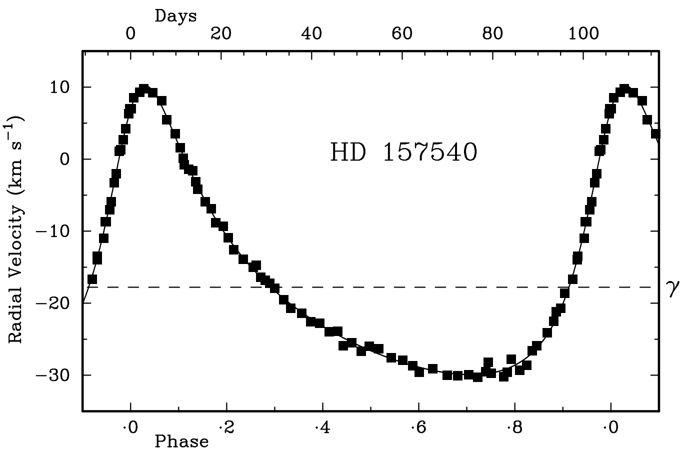

FIG. 4

The observed radial velocities of HD 157540 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. All of the 80 radial-velocity observations were made by the writer with the Cambridge *Coravel*.

The observations have been maintained, though naturally with reduced frequency, and there are now 80 of them, all made with the Cambridge *Coravel* spectrometer; they are set out in Table IV, and the orbital elements derived from them appear in the last column of Table V; the orbit is shown in Fig. 4. The uncharacteristically large r.m.s. residual of 0·55 km s^{-1} is a consequence of the unusually shallow dips given in radial-velocity traces by the star, no doubt owing to its spectral type being much earlier than the K0 attributed to it in the *Henry Draper Catalogue*.

TABLE V

Orbital elements for HD 147250, HD 151446, HD 151448, and HD 157540

Element	*HD 147250*	*HD 151446*	*HD 151448*	*HD 157540*
P (days)	2265·9 ± 3·0	1400 ± 4	1790 ± 6	106·113 ± 0·004
T (MJD)	55703·33 ± 0·21	54648 ± 20	54180 ± 21	53872·00 ± 0·12
γ (km s^{-1})	+9·58 ± 0·04	−3·23 ± 0·04	−48·86 ± 0·04	−17·78 ± 0·07
K_1 (km s^{-1})	11·56 ± 0·07	3·45 ± 0·05	2·76 ± 0·06	20·05 ± 0·12
e	0·8975 ± 0·0011	0·161 ± 0·015	0·239 ± 0·020	0·473 ± 0·004
ω (degrees)	127·1 ± 0·6	149 ± 5	173 ± 5	326·6 ± 0·7
$a_1 \sin i$ (Gm)	158·8 ± 1·3	65·5 ± 1·0	65·9 ± 1·5	25·78 ± 0·16
$f(m)$ ($M_\odot$)	0·0312 ± 0·0008	0·00572 ± 0·00026	0·00357 ± 0·00024	0·0608 ± 0·0012
R.m.s. residual (wt. 1) (km s^{-1})	0·28	0·25	0·25	0·55

References

(1) R. F. Griffin, *MNRAS*, **219**, 95, 1986.
(2) R. F. Griffin, *The Observatory*, **137**, pp. 62, 115, and 170, 2017.
(3) R. F. Griffin, *MNRAS*, **371**, 1140, 2006.
(4) R. F. Griffin, *MNRAS*, **201**, 487, 1982; **210**, 745, 1984; **212**, 663, 1985; **371**, 1159, 2006.
(5) [Announced by] E. Høg *et al.*, *A&A*, **355**, L27, 2000.
(6) R. F. Sanford, *ApJ*, **64**, 172, 1926.
(7) A. Abrami, *Mem. Soc. Astron. Ital.*, **30**, 73, 1959.
(8) M. Mayor & T. Mazeh, *A&A*, **171**, 157, 1987.
(9) T. Oja, *A&AS*, **59**, 461, 1985.
(10) R. F. Griffin, *ApJ*, **148**, 465, 1967.

CORRESPONDENCE

To the Editors of 'The Observatory'

The Sunspot Observations by Toaldo and Comparetti in 1779 November

The recovery of old sunspot observations is essential to reconstruct solar activity in the past and understand the long-term solar variability[1,2]. Some works have been focussed on this task[3,4], collecting large amounts of daily counts of sunspot group numbers. The number of days with records per year in these collections is very variable from 1610 to the present. There is, at least, one observation per day from 1848 to present, but there is a low frequency of observations in some years of the 17th and 18th Centuries. In particular, the interval 1777–1795 is one of the periods with scarcely any observations in the database[4].

We report here a sunspot observation carried out on 1779 November 3 by Giuseppe Toaldo and Andrea Comparetti. Andrea Comparetti was professor of Medicine at Padova University, and Giuseppe Toaldo was the director of the astronomical observatory of that city. In a dissertation about the severe droughts during the winter of 1779 Toaldo wrote[5]: "*Un' osservazione singolare, degna da ponderarsi in quest' anno, fu l'infinità di macchie che si obsservarono nel sole, sempre, ma segnatamente nell' Inverno continuano tuttavia, o piuttosto ripigliano e risorgono anche oggi 3 Novembre col Sig. Dottor Comparetti, dotto Fisico non meno che valente Medico, ne abbiamo contate almeno 17 in varj ammassi, e alcuna di esse aveva il diametro più grande di quello della terra, poichè certamente più d' un minuto era la loro apparente grandezza*". [One special observation, worthy to take into account in this year, was the huge number of sunspots observed in the Sun, continually, but particularly in winter still continuing, or rather recovering and rising again even today 3rd November with the Dr. Comparetti, erudite physician but not less talented medic, we have counted at least 17 in various groups, and some of them had the diameter greater than the Earth diameter, since the apparent size was greater than a minute [of arc].]

This observation is particularly interesting because nobody else observed the solar disc on that date or close to it. The previous available observation is on 1779 October 22, and the next one was made on 1780 January 14 (both by Johann Casper Staudacher in Nuremberg)[6,7]. Note that Toaldo was an active observer of aurorae[8]. Moreover, it is possible that Toaldo or Comparetti observed sunspots frequently. The discovery of relevant observations made by Toaldo or Comparetti would be of great interest for this kind of study.

We appreciate the support of EU, Junta de Extremadura, and Ministry of Economy and Competitiveness (consortium IMDROFLOOD, Research Group Grant GR15137, IB16127, and AYA2014-57556-P).

Yours faithfully,

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References

(1) J. M. Vaquero & M. Vázquez, *The Sun Recorded Through History* (Springer, Heidelberg), 2009.
(2) F. Clette *et al.*, *Spa. Sci. Reviews*, **186**, 35, 2014.
(3) D.V. Hoyt & K. H. Schatten, *Solar Phys.*, **179**, 189, 1998.
(4) J. M. Vaquero *et al.*, *Solar Phys.*, **291**, 3061, 2016.
(5) G. Toaldo, *Giornale Astro-Meteorologico per l'anno MDCCLXXX* (Gaspare Storti, Venice), 1780.
(6) R. Arlt, *Solar Phys.*, **247**, 399, 2008.
(7) L. Svalgaard, *Solar Phys.*, **292**, 4, 2017.
(8) F. Domínguez-Castro *et al.*, *J. Space Weather Space Clim.*, **6**, A21, 2016.

REVIEWS

The Amazing Unity of the Universe, and its Origin in the Big Bang, 2nd Edition, by Edward P. J. van den Heuvel (Springer, Heidelberg), 2016. Pp. 315, 23 × 15·5 cm, Price £19·99/$34·99 (paperback; ISBN 978 3 319 23542 4).

Ed van den Heuvel is known to his many astronomical friends and colleagues (among whom I am honoured to include myself) for nearly a half century of pioneering work on the evolution of close binary stars (ones whose components transfer material at some point in their lives). Curiously, binary (or close binary, or double) stars are not even an index entry in *Amazing Unity*. They just barely make the cut in the caption to Fig. 13.5 (b) as "exploding carbon–oxygen white dwarf stars in binary systems that are triggered to explode due to the feeding of mass by their binary companions ..." .

The topics that are covered are roughly those of many introductory texts and popularizations: in very rough outline, the Solar System, stars and Milky Way (distances, structure, composition), cosmology (including gravity, the Big Bang, and matter excess over anti-matter), time-scales and life on Earth, more cosmology (inflation, dark matter and dark energy, the CMB), and more speculative topics (direction of time, multiverses, and intelligent life elsewhere). Five appendices provide additional numbers and equations, and here is one point where the author and I part company! Appendix C, 'About the parameters of the Universe', has an equation (C9) for "redshifts larger than one". But it gives the special relativistic relationship between v/c and redshift, suitable for rockets passing through your sitting room and quasar jets observed from within the host galaxy. Because relativistic cosmological models include two definitions of distance (luminosity and angular-diameter distances), there can be no unique speeds, even if you knew what to do about the time coordinate. The models do provide unique relationships between redshift and the two sorts of distance, look-back time, age of the Universe, and so forth.

That glitch is not at all unique to this book or author. Some time ago, when I was asked to evaluate physics problems for the Graduate Record Exam, there was one that adopted the same erroneous expression for redshift. I pointed this out, and was not asked to evaluate physics GRE problems again. Never mind — you should hear what happened to Lemaître when he pointed out an error in a World War 1 artillery manual! (*A&G,* **58**, 2.28, 2017). On other pages we meet "Steven" Hawking and William "Herschell". Norman Lockyer, who first recognized a new element in the Sun, is forced to share credit with Jules Janssen (Biman Nath has written a whole book about this, called, unsurprisingly, *Helium*). What they both did was to figure out that you could examine the spectrum of the solar chromosphere with a pair of spectrographs, not having to wait for a solar eclipse. The claim that the Sun contains all the elements known on Earth ignores Tc, Pm, the transuranics, and the trickle down from U and Th to Ph and Bi. Well, OK, there must be a similar trickle down in the Sun to Ra, Po, *etc.*, but not seen spectroscopically.

Of course there are many things to like as well! Ed reproduces the originals of two closely-written postcards from Einstein to de Sitter. A modest magnifying glass suffices to read them (well, I suppose you also have to know a little German), and both the original text and translations appear as items 325 and 366 of Volume 18 of the Einstein Papers Project. They deal partly with the cosmological constant. There is a nice 'bush diagram' (Fig. 11.8) of the tree of life, which honourably gives equal status to the bacteria, archaea, and eukarya. It is not quite a clad diagram, however, as the bacteria and archaea are allowed to interact after separating (well, I have some Neanderthal genes, don't you?).

But the facing page, Fig. 11.7, also about the origins and evolution of life, shows a sky with an incoming comet, Saturn, and a spiral galaxy all about the same distance from Earth and the same size. And there the path of life is probably too monotonic, with a sea from which fish climb out, pass through a grove of dinosaurs to a clearing with three human beings (two large, one small). And the path up the hill beyond has a couple more human beings, about to reach the observatory on the top (you will need that magnifying glass again). If you want to be as fussy as I am about details, you might remark that even Lemaître might not have put "Big Bang; start of the expansion of the Universe" at $t =$ precisely zero (before the Planck time). And I. S. Shklovsky's *Universe, Life, Intelligence* (1962) predates the 1966 *Intelligent Life in the Universe*, collaboration with Carl Sagan.

We now take a long break, in which I reread some of my favourite passages of Shklovsky & Sagan, and I rather hope you will do the same. What they did can probably not be repeated or updated. We know too much, with too many qualifications. But *Amazing Universe* is a brave attempt. — VIRGINIA TRIMBLE.

A Fortunate Universe: Life in a Finely Tuned Cosmos, by G. F. Lewis & L. A. Barnes (Cambridge University Press), 2016. Pp. 391, 23·4 × 15·5 cm. Price £18·99/$27·99 (hardback; ISBN 978 1 107 15661 6).

The title claims that the Universe is finely tuned for the existence of life. The authors provide evidence for this, investigate various possible explanations, and rebut the most common criticisms. Many readers have probably encountered various aspects of the debate on this topic; this book provides an opportunity to learn more at an accessible level. It is a popular-science book, but one of the authors has published an extensive review of the subject. The case is well made that the Universe is finely tuned for life; the interesting question is why. It could be coincidence; there are other unlikely events which, as far as I know, have no explanation, such as the equal angular sizes of the Sun and Moon (which, moreover, are equal only near the present time). Coincidences involving basic physics, though, are usually perceived as more puzzling, and the degree of fine-tuning in the case of life in the Universe is much greater. Or could the Universe be no other way? Perhaps, but this remains to be shown. As long as it is not possible to calculate basic quantities such as the value of the gravitational constant, mass of the electron, *etc.*, it makes sense to assume that they could have been different. Was it designed? Did it evolve? Or are there many universes in a Multiverse, and we shouldn't be surprised that we live in one which allows life?

The authors, both at the Sydney Institute for Astronomy, make a good case for the existence of fine-tuning. Five of the eight chapters discuss fine-tuning as revealed by particle physics, fundamental forces, energy and entropy, cosmology, and investigating changes in the laws of Nature themselves (as opposed to free parameters within the known laws). This discussion is completely convincing, especially since common objections are debunked. Those objections are summarized and replied to in Chapter 7. Those six chapters are between an introductory and a concluding chapter presented as a dialogue between the authors; the former sets the stage and the latter, much longer, essentially summarizes the book. This discussion is mostly open-minded. Of the five possibilities mentioned above, 'just coincidence' is, in my view correctly, dismissed, and explaining the fine-tuning of the Universe *via* evolution (which does explain the fine-tuning of organisms to their environment) creates more problems than it solves. Perhaps the most interesting possibility is that the Universe must be as it is, for reasons which we don't know. This is barely discussed, not because it is unimportant, but because there is essentially nothing to say before someone proves it, at which point the rest of the discussion would be superfluous. That leaves the Multiverse and a designer as possible explanations (though it is still conceivable that someday someone will show that the Universe could not be other than it is).

The book makes a good case for the Multiverse: there is no other credible explanation for the existence of fine-tuning. (See Tegmark[1] for a book which discusses fine-tuning in the context of the Multiverse, rather than *vice versa* — reviewed in these pages by yours truly[2].) While an *ad-hoc* explanation is not necessarily false (recall that the neutrino was an *ad-hoc* explanation), it is rightly greeted with scepticism. However, the Multiverse is not an idea which was invented to explain fine-tuning, but rather arises naturally (some would say unavoidably) in the context of inflation (though the vague idea of a 'world ensemble' is much older). So, as long as there is good reason to believe that our Universe underwent inflation, there is good reason to believe in the Multiverse. Among the many myths debunked by the authors is the claim that the Multiverse is untestable and/or unscientific. Explaining fine-tuning *via* the Multiverse necessarily involves the Anthropic Principle, and the authors also clear up some misunderstanding in that area (in particular, they argue that the Carter's Strong Anthropic Principle[3] has been misinterpreted by many later authors).

The eight chapters are preceded by a short summary, biographical sketches of the authors, praise by colleagues, a foreword by Nobel laureate Brian Schmidt, a preface, and acknowledgements, and followed by a commented further-reading list, references, and an index; 49 black-and-white illustrations are scattered throughout the text. Although somewhat better in this respect than most books I've reviewed in these pages, somewhat better editing would have been useful. The book is well written and most of the attempts at humour are actually funny.

The only part of the book which didn't appeal to me was the discussion of the 'G word'. Of course, any discussion of fine-tuning, especially a book-length one, can hardly avoid mentioning the possibility of Intelligent Design (which to the authors apparently means something more enlightened than a smokescreen for smuggling creationism into the classroom) and/or God, so I don't fault the authors for that. However, such a God would not necessarily have all the properties of the traditional Christian God (which I assume is the reason for the capitalization), but the text is written so that this remains not only a possibility, but perhaps the intent. Also, I found the discussion too unbalanced: traditional arguments are explained at length, but rebuttals barely mentioned. In addition, merely the fact that many famous people pondered such questions — essentially an argument from authority — does not make their ideas true. The only thing allowing such a discussion to appear in a book about science is the potential to falsify it by observation, but the route to that assumes that one agrees with (one of) the authors that something like objective morality exists in a form which is not merely a consequence of evolution — something I find dubious and for which at best only anecdotal evidence is presented. Similarly, conclusions are presented which follow from the existence of free will, which is taken for granted, though many scientists see no possibility for true free will within a scientific worldview. Although there are many good cosmologists who are also religious (*e.g.*, Lemaître, Ellis, Barrow) but rely mainly on traditional funding, and cosmologists supported by the Templeton Foundation who don't appear to be religious, the fact that one of the authors has received significant support from the Templeton Foundation can't be neglected when the book itself mentions God. (It is no surprise that that author, Barnes, comes across as a supporter of Intelligent Design, whereas Lewis seems to support the Multiverse.) Of course, that doesn't necessarily mean that the source of funding influenced the conclusions; it could be the other way around. However, support from a foundation which, although perhaps not in the areas covered by this book, is not known for supporting objective science should at least be noted. *Caveat lector*. Fortunately, though, nothing is lost by just skipping that section (a small fraction of the book), and the rest of the book could have been written independently of it. (On the other hand, leaving it out completely could raise the accusation that this hypothesis was not considered at all.)

The arguments are clear; references are provided for those wishing to delve deeper; essentially all points of view are presented (though, at least for me and one of the authors, only one explanation really makes sense). This is an important topic and the book is a good summary of the field. I enjoyed reading it and recommend it to those interested in the Big Questions. — PHILLIP HELBIG.

References

(1) M. Tegmark, *Our Mathematical Universe* (Allen Lane, London), 2014.
(2) P. Helbig, *The Observatory*, **134**, 150, 2014.
(3) B. Carter, in M. S. Longair (ed.), *Confrontation of Cosmological Theories With Observational Data* (D. Reidel, Dordrecht), 1974.

Space, Time and the Limits of Human Understanding, edited by S. Wuppuluri & G. Ghirardi (Springer, Heidelberg), 2017. Pp. 530, 24 × 16 cm. Price £52·99/$79·99 (hardbound; ISBN 978 3 319 44417 8).

Space is blue, and birds fly through it (was that Fermi?). Time is what keeps everything from happening at once (perhaps John Wheeler?). And the limits of human understanding recede faster in some directions than in others, leaving the rim of ignorance around the edges ever larger (Trimble, but Newton wrote something along the same lines earlier). This level of understanding does not get one very far into the thoughts of editors Wuppuluri and Ghirardi and their 48 authors (including foreworder John Stachel and afterworder Noam Chomsky), though the preface says that all authors have taken care to keep the articles as nontechnical as possible and self-contained.

On 1922 April 22, Albert Einstein and Henri Bergson in Paris engaged in a definitely acrimonious debate (not sweetened by the aftermath of The Great War) partly on the nature of time. Einstein insisted that there are only two sorts, the physicists' time (as in relativity) and psychological time (as in his joke about hot stoves and beautiful women). Bergson defended philosophical time as equally important.

I asked for the review copy of this book in the hopes of finding out who won in the long run. Apparently 95 years is not long enough. Einstein wins the popular vote, in the sense of being mentioned in more than a dozen chapters, while Bergson appears in only three. But not all the appearances are complimentary, or even entirely accurate. For instance, writes Randall E. Auxier of Southern Illinois University, "the reigning dogmas today in both evolutionary theory (neo-Darwinism) and physics (the so-called Standard Model of Gravitational Cosmology, *i.e.*, Einstein's Spinozistic universe) …". But if he means standard cosmology, we generally call it "lambda CDM", and if he means the standard model of high-energy/particle physics, it has quarks and leptons, gluons, photons, and other force-carrying particles, but no input from Einstein, nor, for that matter, Spinoza.

This is the only nasty remark about Spinoza I've ever encountered. There may be others in the volume, but impossible to find in the absence of an index. There are, however, at least brief mentions of more than 80 mathematicians and physicists, 20 philosophers, and eight from evolutionary biology, plus many dozens whose names I do not recognize and so cannot classify. One chapter mentions J. Canales's 2005 book, *The Physicist and the Philosopher* (Princeton University Press), which focusses on the 1922 debate and declares Bergson to have spoken more truly.

A particularly fascinating chapter, by Doron Swade of the University of London, includes designs for the Difference Engine and Analytical Engine of Charles Babbage (with drawings that cry for magnification). No, he never actually built one (though others have), but Babbage invented the concept of computer programs and actually drafted some. Astronomy peeks out of a chapter by C. C. Unnikrishnan of the Tata Institute of Fundamental Research (Mumbai), who points out that the cosmic microwave background provides both an absolute time standard (what temperature is it when you are?) and motion (what dipole moment do you see?). Unfortunately, the author also says he has carried out various measurements of the speed of light and finds that it obeys Galilean rather than Einsteinian relativity. The secret, he says, is to measure only one-way velocities, not round trips *á la* Michelson and Morley.

And a last question you could ask this volume: is Sanskrit an Indo-European language? Well, we know it is, nearly the oldest, but, starting from scratch with a couple of papers on the Nyaya (*vs.* Vedic) tradition by Jonathan Duquette (Oxford) and Krishnamurti Ramasubramanian (Indian Institute of Techology, Mumbai), it is easy to conclude, (*i*) the language is highly agglutinive, with words like *tatpragabhavapradhvamsabhavaavacchedanupaoatteh*, (*ii*) there are particles like 'no' (which appear to carry negation), (*iii*) *tada* might mean whole or total (the Latin is *totus*), and (*iv*) *ta da*! Yes!! since *trikalabadhya* is something about three, and words like 'tri' can be chased all over the map of Indo-Europe (three, *drei*, *tres*, *trois*) and on into Hebrew and Arabic (*tiletta*).

The pages of this book, at about 15 cents each, are not, by Springer standards terribly expensive, and some of them will surely give you more complex answers than appear here, at least if you come to the volume with more complex questions than "Was Bergson or Einstein right?" — VIRGINIA TRIMBLE.

The Glass Universe: The Hidden History of the Women Who Took the Measure of the Stars, by Dava Sobel (4th Estate, London), 2016. Pp. 324, 24 × 16 cm. Price £16·99 (hardbound; ISBN 978 0 00 754818 7).

In the olden days, if women wanted to become astronomers they either had to have sympathetic (and rich) parents who believed in equality of education and opportunity (examples being Hypatia, Maria Mitchell, Agnes Clerke, Annie Jump Cannon), a brother who needed an inexpensive assistant (Caroline Herschel), or an older astronomer husband whom they could assist and eventually supersede (Elizabeth Hevelius, Margaret Huggins, Annie Maunder). But in the mid-19th Century things started to change. Certain enlightened universities introduced women's colleges, examples being Girton, Newnham, Somerville, Vassar, Wellesley, and Radcliffe. These establishments needed female lecturers and researchers. And soon certain observatories realized that women mathematicians were just as competent as the men, but could be employed for half the wages. So if you were employing 'computers' to analyse your astrophysical data, your grant would employ twice as many if they were feminine as opposed to masculine. The observatory doors were opened and the rest is history.

Dava Sobel, the author of bestsellers such as *Longitude* and *Galileo's Daughter*, has turned her attention to the Harvard College Observatory between 1880 and 1940, during the directorship of Edward Pickering and Harlow Shapley. Sobel concentrates on the flourishing of photographic photometry, stellar spectral analysis, the interpretation of Cepheids, the introduction of Leavitt's Law, and the production of the *Henry Draper Catalogue*. The works and contributions of Adelaide Ames, Catherine Wolfe Bruce, Annie Jump Cannon, Anna Palmer Draper, Williamina Paton Stevens Fleming, Cecilia Helena Payne-Gaposchkin, Henrietta Swan Leavitt, and Antonia Coetana de Paiva Maury are considered in detail. The end result is a book that is insightful, uplifting, accessible, compelling, and an absolute joy. I loved the detail (hands up who knew that in 1910 the Flagstaff Observatory owned a dairy cow called Venus; and did you know that when Cecilia Payne submitted a paper to *Nature* in 1924 she initially attributed it to C. H. Payne? Shapley asked her if she was "ashamed of being a woman". She crossed out the "C" and inserted "Cecilia".)

Everyone with even a vague interest in the early history of astrophysics and specifically the huge contribution made by women to its advancement should read this superb, rewarding book. — DAVID W. HUGHES.

Merz Telescopes: A Global Heritage Worth Preserving, edited by I. Chinnici (Springer, Heidelberg), 2017. Pp. 185, 24 × 16 cm. Price £66·99/$99 (hardbound; ISBN 978 3 319 41485 0).

I use a 41-cm Dall–Kirkham telescope at home, but if I had unlimited resources my choice would be a refractor of similar aperture. There's that elusive *something* about using a large refractor, and the feeling that you aren't a proper astronomer unless you do. In 1929, a former Editor of this *Magazine*, Dr. W. H. Steavenson, embarked upon a tour of the East Coast observatories of the USA, where he had the chance of testing and comparing many large refractors. In my own observing career I have been able to do serious work with the 60-cm Clark at Flagstaff, the 83-cm by the Henry Brothers at Meudon, the Cooke refractors at Cambridge (UK), the medium-sized Zeiss refractors in Prague and at Griffith Observatory, the 36-cm Zeiss at Arcetri, and last but by no means least, the 25-cm Merz object glass of Orwell Park, Ipswich. All gave excellent results, and they and other instruments do indeed constitute a precious global heritage.

Merz was a leading European producer of high-quality optics (and instruments) from the early 19th Century, as the successor to Joseph von Fraunhofer's Optical Institute of Munich, and this book provides a Company history and short histories of a number of continental Merz telescopes and observatories (mostly in Italy), and lists of some of the larger instruments and optics the firm turned out up till 1932. It was sad to read that the Merz telescope used by Secchi at Collegio Romano was destroyed by fire, and that someone had dropped the 45-cm objective of the Brera Observatory, Milan. But it was with the help of a Merz objective and a Merz objective prism that Secchi laid the foundations of the spectral classification of stars and Schiaparelli laid the foundations of areography. It is heartening to know that many Merz objectives and telescopes continue to provide good service today. Among many fascinating facts we learn that the glass of the extant 25-cm at Brera has a slight greenish tint, a factor which may have helped increase the contrast of the Martian surface features when Schiaparelli was mapping them. Some optical tests of Merz objectives are given, but no attempt is made critically to compare Merz with other classic opticians, but those interested can find Steavenson's optical-testing notes preserved in the RAS Archives.

This is a multi-author work, edited by Ileana Chinnici, and all the chapters are well written and highly informative. There are plenty of illustrations, in both colour and black & white, most of which I had not seen before. The index can be kindly described as 'thin', but that is not such a big drawback for those who would like to learn more about this classic European telescope manufacturer. — RICHARD MCKIM.

Space Telescopes: Capturing the Rays of the Electromagnetic Spectrum, by N. English (Springer, Heidelberg), 2017. Pp. 312, 23·5 × 15·5 cm. Price £29·99/$39·99 (paperback; ISBN 978 3 319 27812 4).

This volume is part of Springer's *Astronomers' Universe* series, which is in general aimed at active amateur astronomers as well as a wider range of astronomically-informed readers. Therefore an important question to ask is, does it fill that role? To some extent it does. The book is an engaging read which covers a wide landscape of space-astronomy missions that span the electromagnetic spectrum from gamma rays through to microwaves. Importantly, it talks about aspects of space astronomy that are rarely covered, if at all, in popular science books and gives long-overdue exposure to missions that were important stepping stones in developing our understanding of the Universe and which are much less well known outside the professional field than the *Hubble Space Telescope*, which has deserved prominence.

However, I was rather disappointed with the US-centric bias to the story which significantly downplays the European and, closer to home, UK contributions to the narrative. Any reader of this book would assume that the development of X-ray astronomy was conducted entirely in the US, as there is nothing about the major contributions from the UK, Europe, and Japan, which are on a par with the US ones, and the long track record of important missions: *Ariel V*, *EXOSAT*, *Ginga*, *ROSAT*, *Suzaku*, and *XMM-Newton*, for example. While *ROSAT* is mentioned very briefly in the preface, it is described as an infrared telescope! Furthermore, it was *ROSAT* that carried out the first sky survey in the EUV, with its *Wide Field Camera*, not *EUVE*, although I agree that that was an important mission, extending the sky coverage to longer wavelengths and providing spectroscopic capability. Correct historical attribution is important, as once errors like that get into the literature, they can be hard to correct. There are similar shortcomings in the infrared-astronomy chapter, which fails to mention *Akari* (Japan) and ESA's *ISO* and *Herschel*, even though *Herschel* is the largest space telescope yet launched.

I think there has been a missed opportunity, from a European author and publisher, to counter the biasses towards US activity often seen in the literature. I suspect any professional space astronomers, like me, who read this will feel irritated by the book's deficiencies, but then we are not likely to buy it as we do not constitute the target readership. While, technically, the science included is sound and is nicely presented, I would not recommend it, even to its target readership, because of the misleading picture of the development of space astronomy that it presents in some chapters. — MARTIN BARSTOW.

The Ethics of Space Exploration, edited by J. S. J. Schwartz & T. Milligan (Springer, Heidelberg), 2016. Pp. 267, 24 × 16 cm. Price £66·99/$99·99 (hardbound; ISBN 978 3 319 39825 9).

Do microbes have rights? What about Martian microbes? How might the rights of putative Martian microbes compare with the rights of astronaut explorers from Earth? Who is responsible for dealing with any criminal behaviour of that might occur on the *International Space Station*? All good questions and all answered — sometimes surprisingly in this book on the ethical issues surrounding space exploration. This is one of about a dozen books in the Springer *Space and Society Series* which aim to explore a broad range of topics in astronomy and the space sciences from the perspective of the social sciences, humanities, and the arts. Other titles in the series include: *The Meaning of Liberty Beyond Earth*, *The Meaning and Value of Spaceflight*, and *What We Know About Extra-terrestrial Intelligence*. The very fact that these titles adorn books of more than a few pages is perhaps the first surprise of this series, which primarily deals with humans in space in the far future — a few generations hence, although more current topics are also covered. This volume, edited by a philosopher and a theologian, Schwartz and Milligan, respectively, consists of essays by practising ethicists and space professionals from philosophy, physics, law, and space-science departments in universities and institutes from the USA, Canada, the UK, and France. It covers the ethics of space exploration in five sections: 'Cultural and historical context', 'Normative ethics' (*i.e.*, theories of value), 'Humanism and posthumanism' (the human imperative to explore beyond Earth), 'Planetary protection', and finally 'Ethical and legal issues'.

The introductory chapter quotes the opening declaration from a UNESCO/ European Space Agency discussion report (*The Ethics of Space Policy*, ed. A. Pompidou, 2000), "The Earth and Space are not ours. They are treasures, real and symbolic, which we owe to ourselves to safeguard for our descendants". There were times when reading this book that I felt that statement might be the only clear, uncontentious sentence in the entire volume.

Although ethics isn't something that immediately springs to mind when knee deep in the nitty gritty of preparing space instruments or indeed evaluating the results from them, a few moments of reflection will bring several space-related ethical and moral issues to mind. One of the more obvious surrounds the implications of terraforming other planets (Mars) to accommodate an alien species — us; or indeed the proposed modifications to our own planet's atmosphere to counter the effects of our own misuse. Is it ever right deliberately to change the entire environment of a planet? Such an intervention might seem counter to the "safeguarding the Earth and Space for our descendants" ethos of the UNESCO agenda. The whole morality issue becomes more intense if we then consider that terraforming planets might require the destruction of an indigenous species — Martian microbes, for example; do microbes have rights? There is also a whole raft of ethical issues relating to using animals as test subjects — like the early canine space pioneers that were sent to a certain death, or the use of the higher apes with attendant issues of sentience and consent. Animal experiments give rise to ethical questions in all research fields in which they are used, and at least for medical research the aim of saving human lives may be sufficient justification. However, this volume deals with ethical questions specifically associated with space exploration, where the immediate saving of human life isn't the prime motivation.

To my way of thinking, although the order of the sections appears conventional — background first and then more detail — in this case it results in subjects being addressed in reverse of what might seem a more comfortable order. Starting with science-fiction-based historical context means that the far future and space colonies fill the first chapters but leave more currently relevant issues, such as criminality and legality in space, to the last few pages. An interesting discussion on a present-day issue — the morality of using radioactive thermal generators on space probes — is left to the penultimate chapter. A particular set of extreme circumstances would be required for those robust devices to disintegrate in a manner that would affect those of us on the Earth (a fire at launch for example), but should this occur then statistically 12 humans would die from cancers induced by the resulting increase in global radioactivity. The trade-off that directly and purposefully risks 12 human lives for increased knowledge of the outer Solar System is an interesting discussion. This is a wonderful chapter to fuel coffee-time argument as it addresses what constitutes consent and acceptable risk, and as such it might have been an excellent introduction to wider questions of morality in space exploration.

The introductory first chapter to this collection gently guides one through the recent history of the subject and the topics to be reviewed in the subsequent pages, and is a worthwhile and enlightening read. Thereafter things get a little trickier. Every professional field has its own terminology and jargon; presumably that helps to curtail the need for long and repetitive explanations and descriptions, but used to excess there is the real danger of self-parody. The opening essays, which deal with humans in significant numbers living in space colonies of various designs, use science-fiction scenarios as a spring-board for discussion; I found this the toughest section to maintain interest. Maybe that was because of my unfamiliarity with O'Neill cylinders and Bernal spheres, which the authors seem to regard as standard structures for human space colonies, or maybe because the ethical issues are rather large and unfocussed.

Two essays grind through the logic and morality of the case for destroying, or at least harming, putative Martian microbes in order to accommodate human colonies on Mars — the "push and shove of the moral market place" as one author describes it. In those essays it is the matter of intrinsic (or inherent) value that carries weight, and although simply being a living entity confers some intrinsic value as far as those ethicists are concerned, there are gradations in this quantity — those with the highest are self-organizing (*i.e.*, biological) and self-legislating (biologically complex). This conveniently gives a sort of theoretical permission for humans to destroy habitats of lesser, lower intrinsic value, life forms, such as Martian microbes, in order to achieve a better environment for more humans. Now that sounds like the sort of justification once used to claim ownership of countries in colonial times. Although the authors maintain that all beings of a particular species must be assigned the same intrinsic value, it seems to me a slippery slope that could lead to further gradations in the intrinsic value, and particular populations of humans could one day be found wanting.

The sections on criminality in space are more relevant to the present day. Where a sufficiently large number of humans are in close proximity, one's own experience suggests that differences of opinion and character are sometimes strongly expressed, and at the extreme end of this distribution of strong expression is the possibility of heated argument and ultimately potentially criminal behaviour. To date the *ISS* has been occupied by just less than 400 very carefully selected men and women from many different backgrounds and cultures; it is difficult to believe that all of those encounters have been as harmonious and friction-free as the anodyne space-agency reports would have us believe. It can only be a matter of time before issues of criminality need to be considered. Apparently there is already space law in place which allocates jurisdiction to the legal system of any astronaut violator's home nation.

The final paper moves us into the very far future and deals with legal issues associated with encounters with extra-terrestrial life. There seem to be three basic scenarios: if they are dumber than us (Lower Intrinsic Value) and not as technically competent, we will probably treat them as legal entities much as we do animals, *i.e.*, as property; if they are equally smart then we treat them as other nations and through discussion come to an agreement as to the applicability of each species' law; and finally if they are smarter than us and technologically advanced, then humanity's outlook doesn't look good as we become the objects to be treated as owned creatures — our only hope might be to look cute … or very, very, dangerous.

In summary this is a well-edited selection of essays covering a human side of space exploration that few of us may have spent much time considering. Although the jargon and ethicists' language needs to be worked through, this volume is a worthwhile and enlightening read about a topic that could become very relevant in the next few decades. — BARRY KENT.

The Politics and Perils of Space Exploration, by L. Dawson (Springer, Heidelberg), 2016. Pp. 199, 23·5 × 15·5 cm. Price £22·99/$29·99 (paperback; ISBN 978 3 319 38811 3).

This is not an academic book, and also not a very technical book, but judging by the more than 300 footnotes it is well researched, but using some unusual sources. Along with US newspapers like *USA Today*, some UK newspapers such as *The Guardian*, *The Independent*, and *The Daily Telegraph* have been mined for their in-depth knowledge, together with specialist non-technical publications like the *Christian Science Monitor*. There are rather few technical journals, or primary sources and, other than NASA briefings, not many of a scientific content. The footnotes are the only references provided. The book reads like an executive summary linking press releases from various players and commentators, some associated with the space industry and others from more general sources. There seems to have been a minimal amount of editing, and it often felt as if I may have been the very first person to have read the text as there seems to have been no proof reading at all. There are several easy-to-spot typos, like missing parts of sentences, some clunky prose, and repeated sentences — a sure sign of a cut-and-paste origin. The tone is as if the author is speaking to a not-very-bright child. Dawson describes the effect of mental stresses on an astronaut "… such as the worries about family or friends or being alone and distant from Earth, can adversely affect a crew member's health, work performance and overall welfare". To which the only reasonable response must be "No way!" — or the more-earthy equivalent involving a well-known resident of Baker Street. There is probably a readership for this undemanding, rather superficial but wide-ranging, survey of the modern space industry, but that readership should not be today's non-technical students, the very one the author may have intended. There are far too many factual errors, seeming misunderstandings, and strange phrasings that even when you can see the point that the author is trying to make you can't help feeling that a student, especially a non-science student, would get the wrong impression. For example, when discussing communication delays with distant spacecraft Dawson mentions that the new laser-based communications systems are much faster than the old radio way of doing things. She notes that with current radio telecommunications the transmission delay to Mars can be significant — "over 30 minutes" — but with laser communications images can be transmitted, "going from Mars to Earth in about five minutes". Dawson explains that this is because for laser light the energy is "more focussed and less wasteful as it travels through space"! Those two times look suspiciously like the light-travel time between closest and most distant Mars-to-Earth separations, but non-technical people reading that passage could not avoid the impression that laser light, because it is "focussed" must somehow travel up to six times faster than radio. Some of the errors are serious, some less so: does it matter that the fire on *Apollo 1* actually took place on top of an unfuelled Saturn 1B rather than the Saturn V as the author tells us? Or that the first test flight of the *Orion* capsule in 2014 December did not, as the author claims, loop around the Moon before landing in the sea off San Diego five hours later? In the case of *Apollo 1* and the particular version of a Saturn booster, that may be a forgivable oversight, but to be so wrong about the *Orion* test flight does matter, and as for claiming variability in a fundamental physical constant, the velocity of light in a vacuum, well, words fail me. Maybe this is the book for a post-truth generation in which the general theme is more important than the detail. It could even be that this is one of the books of 'alternative facts' that seem to be in vogue in certain parts of the US at the moment. It certainly seems designed to be read on an internet-connected device, as many of the references are web based.

Annoyingly, there is the germ of a good book hiding in these pages, and on occasions it is engaging — the writing improves as the shuttle programme is described. The political machinations required to support the space programme is a worthy subject to be explored. With a bit of supportive editing the author may have been well placed to write it, although as she tells us in the preface her relevant space experience was over 25 years ago. As it stands the book seems to be little more than a compilation of press releases and an index to web pages; but that in itself could be useful. The layout and the order of topics is acceptable and there are indeed some interesting items. So, with the caveat that any hard factual information is to be treated as suspect until independently verified, this book did tell me things that were new information to me — for example, that the US purchases Russian rocket engines (RD-180) as they have superior reliability to the US equivalents. Or that the Space Launch System, the NASA heavy launch vehicle intended for future lunar and Mars missions, will use the existing stock of surplus main engines from the Space Shuttle. Primarily what seems to be lacking (other than comprehensive fact checking, proof reading, and a bit of fundamental physics) is the author's voice. The internet has been trawled and relevant documents found to fill paragraphs about a particular topic but there is little of the author's assessment of that information, and in a book intended to be about the politics and the humanity behind the space programme that feels like a major omission. — BARRY KENT.

Satellite: Innovation in Orbit, by D. Millard (Reaktion Books, London), 2017. Pp. 208, 21·5 × 16·5 cm. Price £16 (hardbound; ISBN 978 1 78023 659 9).

Sixty years ago, the world was stunned by a small aluminium-alloy sphere that broadcast a beeping radio signal as it sped around the Earth. To the surprise of many in the West, the Soviet Union had won the race to place an artificial satellite in orbit. Instead of welcoming this startling technological breakthrough, most American citizens, politicians, and military personnel expressed fear and indignation that their supposedly less-advanced Cold War rival, the Soviet

Union, had demonstrated a threatening new capability. As we now know, the launch of *Sputnik* opened the door to the so-called Space Age, instead of being the harbinger of Armageddon. Today, many thousands of satellites orbit the Earth, affecting most aspects of our everyday lives.

In this nicely produced book, author Doug Millard, who has studied the evolution of space hardware over several decades in his role as curator and Deputy Keeper at the Science Museum in London, uses his expertise to summarize the development of spacecraft. The book covers many different aspects of satellites, including their basic design requirements and various uses. Millard seems particularly at home discussing the pre-Sputnik years and early innovations, such as the first primitive satellites and communications satellites such as *Telstar* and *Early Bird*.

Human spaceflight is largely glossed over — Apollo is not even listed in the index — while Earth-observation and scientific spacecraft receive fairly brief coverage. The quality of the photographs is excellent, although I would have preferred less repetition. Was it really necessary to include so many pictures of *Sputnik 1*, *Explorer 1*, and *TIROS*?

I found only a few minor errors, such as the mention of *Spitzer* as an X-ray satellite, the wrong launch date for the *Infrared Space Observatory*, and the assertion that there have only been five Mars orbiters since *Mariner 9*. However, this volume is highly recommended as an introduction to satellites in all their various guises. — PETER BOND.

Atmospheric Chemistry: From the Surface to the Stratosphere, by G. Ritchie (World Scientific, Singapore), 2017. Pp. 199, 23 × 15 cm. Price £37 (paperback; ISBN 978 1 78634 176 1).

Undergraduate text-books are, of course, aimed primarily at the specific readership composed of those students that the author is teaching. That is not to say, however, that they will not have value for similar courses elsewhere in the world, or to individuals who want to teach themselves something about the fundamentals of a subject. They can also be useful to someone working at a more advanced level in research, should they need to look up a definition or derivation of some basic formula or argument. Grant Ritchie's book, written primarily for third-year chemistry students at Oxford taking his optional course on 'Fundamentals of astrochemistry and atmospheric chemistry' (there is a separate book, by a colleague, on the astrochemistry part), is one of those that is likely to be a useful addition to many bookshelves.

Tropospheric and stratospheric chemistry are treated separately because they are fundamentally different in many ways, although both involve photochemical reactions and inhomogeneous processes of various kinds, as well as simpler gas-phase chemistry. Radiative transfer, more a topic in physics but essential here, also receives a good basic treatment in a chapter of its own. So do clouds and aerosols, with their important role in scattering and absorbing radiation, and in enabling heterogeneous reactions like those that produce the notorious Antarctic ozone hole. The classic Chapman cycle for the production and loss of stratospheric ozone, and the much more recently recognized catalytic processes involving oxides of nitrogen and halogens, are particularly well covered. So are some of the more complicated aspects of tropospheric chemistry, such as the 'detergent' effect of the OH radical, which is vital for removing a lot of the nastier pollutants in the near-surface air.

This is naturally all fairly basic stuff, but accurate, up-to-date, and clearly presented. It is particularly good to find publishers like World Scientific finally achieving the production of inexpensive books that have colour figures throughout; this greatly aids the clarity of the diagrams and the optimum positioning of colour pictures, which previously had to be gathered into a central 'insert' if they were allowed at all. Each chapter has a good selection of essay-type questions and numerical problems, both at the level likely to be encountered in undergraduate final examinations. — F. W. TAYLOR.

Chicxulub: The Impact and Tsunami. The Story of the Largest Known Asteroid to Hit the Earth, by David Shonting & Cathy Ezrailson (Springer, Heidelberg), 2017. Pp. 124, 24 × 16 cm. Price £22·99/\$29·99 (hardbound; ISBN 978 3 319 39485 6).

It all started just over sixty years ago when Petroleos Mexicanos were searching for oil in the sea shelf north of the Mexican Yucatan Peninsula, and their data led to the discovery of a huge circular gravity anomaly, about 200 km across. Then in the 1980s, the retired Nobel Laureate Louis Alvarez and his geochemist son noticed that a 1–2-cm-thick sedimentary-clay layer, with an age of 65·5 million years, not only separated two completely different regions of fossil remnants but also had an overabundance of the rare element iridium. Soon this Cretaceous–Paleogene boundary was being associated with the fallout from an impact-crater explosion produced when a 10-km-or-so-diameter carbonaceous asteroid hit one of Earth's seas that was about 1 km deep. And this crater was at Chicxulub in the Yucatan.

Shonting & Ezrailson have taken on an extremely difficult task. A description of the impact event and its consequences covers a vast array of scientific disciplines, and unfortunately their end-product is a curate's egg of a book. The description of the ocean tsunami and its effects on Atlantic shorelines was thorough and gripping. The description of the asteroidal flux however was rudimentary. Too little was made of the effect of the impact on the fauna and flora of our planet, and on the evolution of life since the K–Pg boundary. The atmospheric effects of the event — a short 'nuclear winter' with devastating acid rain, followed by centuries of greenhouse summers — was dealt with far too briefly. The usefulness of this book was not helped by the lack of an index. And the quality of the figures left much to be desired. The scientific community needs a good book about the Chicxulub impact. Unfortunately the book under review is not it. — DAVID W. HUGHES.

Imaging Sunlight Using a Digital Spectroheliograph, by K. M. Harrison (Springer, Heidleberg), 2016. Pp. 278, 23·5 × 15·5 cm Price £19·50/\$34·99 (paperback; ISBN 978 3 319 24872 1).

This book comes from Springer's *Patrick Moore's Practical Astronomy Series*, a series that includes many titles of general interest and some that are highly specialized. This falls at the highly-specialized end of the spectrum. It is in fact more than its title suggests as the early part of the book is taken up with an overview of the history of solar observation, the equipment available for white light and narrowband studies, and a consideration (with relatively little maths) of the physics of the solar spectrum. That part of the book is not without errors. In the section right at the beginning on solar eclipses the author suggests that the low inclination of the Moon's orbit is important in the genesis of an eclipse.

While it certainly makes eclipses more frequent and prolonged, even if the orbits of Moon around the Earth and Earth around the Sun were mutually perpendicular there would still be occasions when both bodies arrived at a node at the same time and an eclipse would occur. Also, on p. 58, when discussing Kirchhoff's laws, the author, just before stating correctly that 1 nm = 10 Å, says that 1 nm = 10^{-10} m and 1 Å = 10^{-9} m — clearly the indices have been transposed. I was also left in some doubt about the temperature of the photosphere: Fig 5.1 fits a 5250K blackbody spectrum to the solar continuum but the paragraph on the following page, which refers to this, mentions 5600K, and section 5.3 on the next page states that the continuum is at 5780K. Perhaps anyone capable of building a spectroheliograph would be able to make their own measurement. All of which suggests that this section of the book has been bolted together without thought for the overall narrative. The same cannot be said for later sections which are fluent, comprehensive, and where the book shines.

The later sections of the book cover everything you need to know before constructing a digital spectroheliograph (SHG). The theoretical concepts and design constraints are followed by practical advice on construction. In a book that is extensively illustrated in other sections, a few diagrams would have been helpful in the construction chapter.

The software necessary for reduction of the images obtained in the use of the SHG is described. It is almost all the product of work by a small number of dedicated amateurs and is free. The science that can be done with an SHG is briefly mentioned.

This book should be required reading for anyone thinking of embarking on the construction of one of these instruments, and the first half makes a good primer for anyone embarking on less-sophisticated solar observation. — MIKE RUSHTON.

Shoot the Moon: A Complete Guide to Lunar Imaging, by N. Dupont-Bloch (Cambridge University Press), 2016. Pp. 323, 25 × 17·5 cm. Price £24·99 (paperback; ISBN 978 1 107 54844 2).

This book attempts to be a comprehensive guide to imaging the Moon and lunar phenomena (such as eclipses and occultations) from the Earth. It is aimed at amateur astronomers, and possibly photographers. The author is a French amateur astronomer who has previously published two books in French and translated a third into English. It is copiously illustrated with images and diagrams both in monochrome and colour and is well produced, with useful tables and also formulae. It covers the use of a wide range of equipment in all possible permutations, from phone cameras, consumer cameras, and standard lenses, up to the usual amateur CCD and CMOS cameras coupled to equatorially-mounted telescopes. The subject is treated with some humour (or quirkiness), with features like tables of methods where levels of difficulty are represented by social-media-type 'smiley' icons.

The focus is firmly not on research, but on the 'art of imaging'. There is a section on 'Monitoring transient lunar phenomena' towards the end, but it is brief, it rather confuses undoubtedly real meteoritic phenomena with dubious observations of the past concerning obscurations or unusual colours that were collected under the now-discredited label of 'TLPs', and fails to go into detail of how to survey for impact flashes. Similarly, an earlier section on occultations is brief and lacking in detail — these are clearly not particular areas of interest for the author. Because of this, he is unable to tell us anything about the clock accuracies required — it seems he doesn't know that the normal in-built timekeeping of PCs is not good enough. In contrast, the author provides a long section on the largely aesthetic subject of imaging lunar eclipses, including a table of future eclipses up to 2030.

There is a lot of detail on telescopes, sensors, and filters, and I gained a clearer understanding than I had before of the differences between CCD and CMOS imaging technology. Practicalities of adjusting your telescope, and even painting it to optimize performance, are exhaustively covered. There is, indeed, some sense of padding the book with general astronomical information, and that leads me on to the more critical aspects of my review. It seems to be a common trap that authors on astro-imaging fall into, that they feel the need to explain too much peripheral astronomical information that is better dealt with in general text-books. So, here, there are poor and disorderly explanations of basic phenomena. The information is more or less correct, but badly presented, so I often felt that I was only able to understand the author because I already knew what he was trying to say.

I expect much of the problem comes from English not being the author's first language. The translation often seems to miscarry, for example: "The installation has to be reasonably firmly secured by keeping the strap around one's wrist" (p. 5) (I beg your pardon, is a wrist 'reasonably firm'?); and the phrasing is often clumsy: what are "Apparent movement variations of the Moon" (p. 4) — the monthly cycle against the stars, librations, or what? How can an "intermediate telescope" have a "rather large field of view" with "high magnification" (p. 5)? And I haven't a clue what is meant by "Their mirrors [of bigger telescopes] need to be controlled often", as "alignment" (collimation?) is mentioned immediately afterwards on p. 7. Technically, there is repeated misuse of the word 'magnification', where 'image scale' is meant.

A reader slightly acquainted with astrophotography, reading from the start, would find the pace of the discussion to be turgid. Will they really be interested in the Heath–Robinson sticky-tape-and-fabric methods mentioned? On the other hand, for the beginner, there is often a failure to define terms where they first occur, and they are often (in my opinion) not normal terms. For example, what is a "hybrid camera" (p. 2) and what is "digscopy" (p. 4)? ('Digscopy' seems to be a typo as it becomes 'digiscopy' later on, where it defined as afocal imaging.)

I have always been confused as to the meaning of the term 'supermoon', and I remained confused after reading the author's section 1.2.2. In section 1.2.3, where he discusses contrast, he is in a state of immediate confusion over whether he means contrast between the illuminated and un-illuminated parts of the Moon, or contrast on the illuminated part, his footnote on refractor performance suggesting the former, but paragraph text on space-weathering suggesting the latter. His attempt to explain libration in section 1.2.5 gets into difficulty because he has not first explained captured rotation. These are just examples of the poor organization of information or just poor writing throughout the book. A compounding production issue is that figures are often on different pages from the referring text, requiring thumbing forwards and backwards for the reader.

This is the most thorough and detailed book covering the whole subject of lunar imaging I have yet seen, and for that it gets some points. In trying to be very up to date with equipment it will inevitably have features that become dated very quickly, and this does raise the question of whether printed-book form is the best format for such a discussion. The main problem I had with the book, though, was that I found it difficult to read, because of the problems with language and poor organization of an unnecessary amount of background information. — DAVID ARDITTI.

Planetesimals: Early Differentiation and Consequences for Planets, edited by Linda T. Elkins-Tanton & Benjamin P. Weiss (Cambridge University Press), 2017. Pp. 381, 25 × 18 cm. Price £110/$140 (hardbound; ISBN 978 1 107 11848 5).

In the beginning there was dust, snowflakes, and gas; and these stuck together and produced the planets. It all sounds very easy, but there are snags. You only have a few million years to do it. And you have to be further away from the Sun than about 4 AU for it to be cold enough for the snowflakes to form. Also you cannot collect up the gas until you have produced a protoplanet that is big enough to have sufficient surface gravity, such that the escape velocity well exceeds the velocity of the cold nebular-gas molecules. Then there is a problem with the dust particles. Dust is not very sticky, and there are many stages to go through when you start with an orbiting swarm of dust particles and end up with a spherical lump of rock and iron, 12 800 km across, that you can call Earth. Particle velocity also plays a vital role. Low-speed planetesimal collisions produce fragments and ejecta that can subsequently coalesce gravitationally. With high-speed smashes the resulting bits fly off all over the place and nothing grows. And inter-planetesimal collision velocities change with time, mainly due to the orbital perturbations produced by the ever-growing proto-Jupiter.

Planetesimals have sizes that vary between sand grains and lumps a thousand or so kilometres across. They come in two types. In the outer Solar System the planetesimals were dirty snowballs. The majority of them ended up as the cores of the gas-giant planets, but a small percentage were perturbed to safety and can now be seen as cometary nuclei. The rocky, inner Solar System planetesimals are more problematic. They were gathered up to form both the terrestrial planets and the parents of today's asteroids. The big question is 'were any left over?' Can we distinguish between bits of fragmented asteroids — things we call meteorites — and the original rocky planetesimal building blocks? Then we have the problems of planetesimal growth. This is very much a 'three steps forwards, two steps back' process, with accretion followed by cratering and fragmentation followed by further accretion, with end-products that can be unmelted, partially melted, and even fully molten and differentiated.

In an endeavour to answer some of these questions, a host of academic cosmogonists, meteoriticists, Solar System dynamicists, and planetary physicists have collaborated to write 17 review papers. These are collected to form this impressive book, one that is beautifully produced, well-illustrated, and an ideal introduction to the topic for a researcher qualified in maths and physics. It is a fascinating subject. The aim is to understand how the Solar System formed. The problem is that this formation occurred around 4 600 000 000 years ago, and, as they say in detective novels, the crime scene has gone cold. The reviewers investigate the collision environment in the early pre-planetary nebula and wonder if today's asteroid belt might give any clues as to the outcomes. As planetesimals grow, radioactivity heats up their interiors. Much is then made of the role of differentiation, silicate melting, volatile loss, and the possible clues we have to those processes by looking at iron, stony iron, chondritic, and enstatite meteorites. Maybe the planet Mercury, which suffered considerable mantle loss, might provide a clue? Also the surface composition of Ceres and

Vesta, recently investigated by orbiting spacecraft, could provide revealing information. Reviewers summarize the chronology of the differentiation process, and consider the possible influence of magnetic-dynamo activity in differentiated planetesimals. As is to be expected, much is made of isotopic abundances in meteorites, and the number ratios of the different types of meteorites. The possibility that the Edgeworth–Kuiper Belt contains some of the original planetesimals is also considered.

I recommend this book unreservedly. I was left with a feeling of great gratitude to the researchers who have spent so much time in explaining so clearly the intricacies of these minor Solar System bodies. — DAVID W. HUGHES.

Cometography: A Catalog of Comets, Volume 6: 1983 – 1993, by Gary W. Kronk, Maik Meyer & David A. J. Seargent (Cambridge University Press), 2017. Pp. 845, 26 × 18·5 cm. Price £190/$235 (hardbound; ISBN 978 0 521 87216 4).

For anyone interested in a specific cometary appearance, from the dawn of time up to AD 1993, the *Cometography* series of books has become a reliable and essential 'first stop' in the hunt for details. There is a minimum of a page of text on each comet. Orbital parameters are recorded as well as the heliocentric and geocentric distances and solar elongations of the first and last sightings. The constellations the comet passes through are also listed as is the comet's absolute magnitude. What I appreciated most were the detailed blow-by-blow accounts of the orbital calculations and of the visual, photographic, and CCD observations. The latter differ between the unexpected joy of discovering a long-period comet on its first recorded apparition and the experiences of the hunters searching the skies for a periodic comet creeping faintly along its predicted path in towards the Sun.

The comets are listed in the order of their discovery date, so Comet Halley, the first comet to be visited by dedicated spacecraft, is in Volume 5 (as it was recovered on 1982 October 16) and not the present volume which spans Halley's perihelion passage on 1986 February 9). Many famous comets are, however, included in Volume 6. We read of Comet IRAS–Araki–Alcock, the brightest comet in this volume. Then there is Comet Swift–Tuttle, the parent of the Perseid meteor shower. Following its 1862 discovery, the orbital period was predicted to be anywhere between 114 and 427 years. Fortunately that was refined, and it crept into binocular view on 1992 September 26. Also included is the serendipitous discovery (1993 March 15) of the score of fragments of Comet Shoemaker–Levy 9, followed by the amazing realization that it was actually orbiting Jupiter and not the Sun, and the even more startling realization that the fragments were all going to hit Jupiter in 1994 July. This volume also contains an indication of modern observing power by listing those special comets such as 2P/Encke, 26P/Grigg–Skjellerup, 31P/Schwassmann–Wachmann 2, and 74P/Sminova–Churnykh that can now be recorded all the way around their orbits.

Sadly this is the last volume of this superb, indispensable series. The internet has taken over and we will have to resort to our laptops for details of more recent comets such as Hale–Bopp and Hyakutake. Also, with sky-survey systems like *PanSTARRS*, the cometary discovery rate has increased hugely. But let me end by saying a very special thank you to Gary Kronk. His love of comets leaps from every page of this series of books and his dedication, thoroughness, accuracy, hard work, and clarity of prose is greatly appreciated. — DAVID W. HUGHES.

Astronomy at High Angular Resolution: A Compendium of Techniques in the Visible and Near-Infrared, edited by H. M. J. Boffin, G. Hussain, J.-P. Berger & L. Schmidtobreick (Springer, Heidelberg), 2016. Pp. 274, 24 × 16 cm. Price £64·99/$129 (paperback; ISBN 978 3 319 39737 5).

A thematic exploration across numerous sub-disciplines and techniques, this book resulted from a workshop held at ESO Headquarters in Garching during 2014 November, following which invited speakers were asked to submit reviews rather than mere summaries of their presentations. As a result of this approach, the volume is not an in-depth guide to any specific method. Instead, its thirteen chapters, written by specialists in their particular area, provide overviews of techniques within the overarching theme of high spatial resolution in the visible and near-IR. The observational methodologies covered include adaptive optics, lucky imaging, aperture masking, and long-baseline interferometry. Tomography in its various forms is extensively addressed. Regrettably, speckle interferometry is conspicuously absent. Additional chapters that feature more specific exploitations of those approaches shed light on such topics as interferometric image reconstruction, Stokes imaging, and the disentangling of stellar spectra. While stars, both single and in binary systems, are the natural targets of these methods, the sensitivities achieved by the *Very Large Telescope Interferometer* and the now-retired *Keck Interferometer*, as well as the development of reverberation mapping, have led to high-angular-resolution access to AGN.

For those to whom the words "astronomy at high angular resolution" have the same siren call as they do to the undersigned, this compendium offers up a smorgasbord of enticements into new realms of exploration utilizing exciting instruments and the cleverest of analyses of their data. For some, this work will serve as their embarkation upon deeper treatments toward a potentially career-inspiring journey. — HAROLD A. MCALISTER.

Oscillations of Disks, by S. Kato (Springer, Heidelberg), 2016. Pp. 261, 24 × 16 cm. Price £82/$129 (hardbound; ISBN 978 4 431 56206 1).

This book provides a detailed account of the types of oscillations that can occur in accretion discs. While this may seem at first a narrow topic, accretion discs permeate much of astronomy. They are the birth sites of stars and planets, and they power the extreme luminosities observed in X-ray binaries and galaxy centres where gas accretes onto black holes. The physics of disc oscillations is often critical to models of timing and variability in these accreting systems. Exploiting observations through comparison with predicted quantities can yield crucial insights into the structure and evolution of accretion discs around compact objects.

The book begins with an historical perspective, describing the rise of accretion discs in modern astronomy through their ability to explain the observed luminosities of distant sources. This is followed by a discussion of solar oscillations and the structural information which can be inferred, leading the reader quickly from helioseismology to asteroseismology to 'discoseismology'. The chapters then introduce a wide variety of disc oscillations, covering different disc structures, disc physics, and oscillation types. This breadth is required due to the large range of disc types and environments seen in the Universe. The early parts of the book focus on the properties of the oscillations, while the latter parts are concerned with their excitation mechanisms. The links made with observations of accreting systems, particularly in the introduction and then sporadically throughout the text, are critical as there is still no first-principles accretion theory with real predictive power.

The level is appropriate for advanced undergraduates and above. The content is aimed at the initiated, particularly because of its mathematical complexity. I expect to refer to this book many times in the future when setting undergraduate problems within accretion theory, fluid dynamics, and high-energy astrophysics. Written by Kato, one of the leading figures of this topic, this constitutes an excellent resource for the mathematical machinery and physics involved in dynamical disc variability. — CHRIS NIXON.

Astrophysical Applications of Gravitational Lensing, edited by E. Mediavilla, J. A. Muñoz, F. Garzón & T. J. Mahoney (Cambridge University Press), 2016. Pp. 290, 25·5 × 18 cm. Price £89·99/$140 (hardbound; ISBN 978 1 107 07854 3).

This is a collection of articles based on the 24th Canary Islands Winter School of Astrophysics, held in 2012 November. Each article is self-contained and as a whole it presents an overview of the field of strong lensing. I say overview, as it is strong on results, covering the astrophysics that has been learned over time, with a lot of interesting information about the historical development of the subject, and much discussion of individual lensing systems. One won't get a grounding in the underpinning theory from this book, which is more focussed on observational results and conclusions, with the exception of Charles Keeton's excellent chapter on methods of modelling of strong-lensing systems. That piece does get more into the detail of how the science is done in practice in this field. The emphasis of the book is not a problem, as there are other books and reviews that can be consulted for the reader wanting to get a deeper theoretical understanding. An interesting addition is a computational chapter on ray-tracing methods, which adds a dimension to the scope of the book. For the reader who is entering the field, this is a useful collection that summarizes it well. — ALAN HEAVENS.

Modern Fluid Dynamics for Physics and Astrophysics, edited by O. Regev, O. M. Umurhan & P. A. Yecko (Springer, Heidelberg), 2016. Pp. 680, 24 × 16 cm. Price £66·99/$119 (hardbound; ISBN 978 1 4939 3163 7).

Most modern text-books on fluid dynamics are written for engineers or applied mathematicians, so I welcome a new introduction to the subject aimed at graduate students and advanced undergraduate students of physics. This hefty tome offers a comprehensive introduction to fluid dynamics, mainly with application to planetary science and astrophysics in mind. Whilst there is little that the book covers that cannot be found elsewhere (which is perfectly acceptable because it is not a research monograph), the depth and breadth of material drawn together by the authors is very impressive. They have clearly tried to produce a treatise that is self-contained from the perspective of advanced physics students, and their efforts are nothing short of heroic. The overall structure is excellent; the authors assume that the reader knows little, if anything, about fluid dynamics and they steadily work, over several hundred pages, through aspects of fluid dynamics required for modern astrophysics and fusion research. The text is replete with in-depth discussion, and extensive mathematical derivations are supplied.

Although the book has plenty of positive aspects to it, I am not sure that I would recommend it to those who have not previously studied some aspects of fluid dynamics. I have taught the subject to undergraduate physics students in the UK for many years, and the level of mathematical sophistication demanded by the authors is probably too high for such a readership. For example, the first chapter is on the basic principles of continuum mechanics and the authors could have exploited simple one-dimensional arguments before launching into discussions that require proficiency in multivariable integral calculus. Furthermore, in later chapters the authors assume that the reader has a good grasp of integration in the complex plane, which is often only familiar to undergraduate students of theoretical physics. Bearing that in mind, I find it somewhat curious that the authors devote several pages to explaining elementary concepts in the physics of waves that are a staple of any UK physics degree. There are also a significant number of typographical errors (one or two of which are in the mathematics) that could cause confusion for a novice, so the book should be used with caution. However, I do believe that it is a useful resource for course development. It certainly offers a wealth of interesting insights into fluid dynamics and plasma physics for those with experience, and I found it to be an enjoyable and informative read. — DAVID A. BURTON.

Theoretical Frontiers in Black Holes and Cosmology, edited by Renata Kallosh & Emanuele Orazi (Springer, Heidelberg), 2016. Pp. 252, 23 × 14 cm. Price £100/$169 (hardbound; ISBN 978 3 319 31351 1).

The editors organized a school, sharing the title of the proceedings book, which took place in Natal, Brazil, in 2015 June. The intent was to summarize the most recent theoretical applications of black-hole solutions in high-energy physics. The 'only' prerequisite for a reader is a working knowledge of field theory and group theory, though a knowledge of General Relativity and supersymmetry is also desirable. Additional helpful buzzwords are FGK (Ferrara–Gibbons–Kallosh) formalism, with citation of their 1997 paper; supergravity; holographic renormalization; Palatini extensions (not cited, though with mentions of Penrose, Weyl, Einstein & Rosen, and other familiar folk); and inflation.

I am not a very good match to their ideal reader (and my copy of the volume is one sent to Another Journal, which they decided not to review). Nevertheless I managed to extract a couple of potentially useful ideas and numbers. First, in some extensions of General Relativity, including Born–Infeld gravity (not cited, though lots of self-citations by the chapter author), the centre of a black hole is not a singularity but a wormhole. Not enough numbers are mentioned to tell whether the editors prefer MKS or cgs, but $c = G = 1$ seems to be the custom. The path to confusions is paved with a decision to put references to past equations into in-line text as (34), *etc.* and references to the literature at the end of each chapter in in-line text as [34].

The inflation chapter leads well-armed readers to a few more numbers, in particular (*a*) the number of e-foldings required between the points N^* of horizon crossing and the end of inflation N (50 or 60 seems to work well); (*b*) the spectral index of scalar perturbations, which is exactly 1 according to Harrison and Zeldovich, becomes $n_s = 1 - 2/N \pm$ higher-order terms (that is 0·97 for $N = 60$); and (*c*) the ratio, r, of circular to linear polarization to be expected in the cosmic microwave background. The present observed upper limit is $r = 0{\cdot}11$, and the prediction is $r = (12\alpha)/(N^2) \pm$ higher-order terms, where α can be anything from infinity (already ruled out) to unity or less. There is also a prediction for how that spectral index should vary with length-scale of the perturbations, so that the situation is at least not under-determined.

The authors do not end by saying "more work is needed". Indeed they express hope for future observations to sort things out.

Another chapter begins by reminding us that superstring theory works best in ten dimensions and M-theory in 11. At that point, your reviewer decided to hand the volume on to a graduate student of considerably more brain. — VIRGINIA TRIMBLE.

Varying Gravity: Dirac's Legacy in Cosmology and Geophysics, by Helge Kragh (Birkhäuser/Springer, Basel), 2016. Pp. 185, 23 × 15·5 cm. Price £74·50/$139 (hardbound; ISBN 978 3 319 24377 1).

It is a little difficult to know what to make of this volume. It begins with Descartes, Newton, and Leibniz, and ends with "historiographical and other perspectives." It also very nearly begins (p. 27) by telling the reader that quantum mechanics is "a theory he [Dirac] co-founded with Werner Heisenberg and others in 1925" and ends (p. 158) by telling the reader that "quantum mechanics [is] a theory that Dirac and Jordan co-created while in their early twenties". Max Born, Bohr, Planck, and Einstein himself make cameo appearances elsewhere, but Schrödinger is nowhere to be found.

The scientific endpoint is the general acceptance of mantle convection, plate tectonics, and continental drift in the geosciences and the hot Big Bang in cosmology. Kragh also notes that there has so far been no systematic treatment of the historical relationship between cosmology and geophysics or Earth sciences more generally.

The core of the story starts with Dirac, his large-numbers hypothesis, and the implication that, as the Universe expands, Newton's G must decrease to keep the cosmological number equal to $e^2/Gm_e m_p$ (in the cgs units that Dirac — and I — prefer). Pasqual Jordan appears at length and then Robert Dicke, for the Brans–Dicke theory of gravitation, in which again G would decrease with time. The main geophysical implication is that the size of the Earth would surely increase with time (leaving cracks in the continental crust that become the ocean basins). The Moon would retreat from the Earth (well, it is doing so). The Sun would have been brighter in the past (a solution to the faint-Sun paradox?), the Chandrasekhar mass limit for white dwarfs smaller in the past, and so forth. This is deliberate cherry-picking to make the $G(t)$ hypothesis sound promising. When, however, you go on and do the calculations, nothing comes out right, though defenders of $G(t)$ have persisted down to the near present. The late astronomer, Tom Van Flandern, was one of the most persistent, reporting that he had found evidence in the orbits of the planets for G weakening, and, over this, parting company with his job at the US Naval Observatory. Wolfgang Kundt, who was Jordan's student (perhaps the last still more or less on active duty), and whose book *Physikalische Mythem auf dem Prüfstand* I reviewed here a few years ago (**134**, 300), has at one time or another in his career explored supporting evidence for every conceivable theory and its opposite.

What do the geophysicists say, since *The Observatory* used to be closely coupled to the RAS, which calls its house journal *A&G*? The 1978 second edition of *Earth* by Frank Press and Raymond Siever is plate tectonics (*etc.*) all the way, with a brisk historical hop from Leonardo and Steno to Hutton, Lyell, and Darwin, Kelvin and Helmholtz, with enough nuclear physics to be able to date rocks. In contrast, the final, third, 1978 edition of Arthur Holmes's *Principles of Physical Geology*, actually revised by his widow Doris L. Reynolds Holmes, preserves fossils of earlier views. A contracting Earth (left from the initial very hot state of Kelvin) came first. But R. A. Lyttleton's (1911–1995) contracting Earth of 1970 does not make the cut*. Then comes the expanding Earth implied by a gradual decrease in *G*. Holmes, like Kragh, begins with Dirac and nearly all the same developers of expanding-Earth theory. Curiously, 'he' is prepared to divide 'credit' for the increasing Earth–Moon distance between tidal drag and declining $G(t)$, with credit to Van Flandern for supporting astronomical data, though (Arthur) Holmes himself proposed mantle convection as far back as 1928. If there is a moral in this, it is that there is no one right time to write a text-book, and that historians run similar risks as they come closer to the present.

The various discussions in *Varying Gravity* of terrestrial expansion are clarified by a useful table (4.3) of twenty-three values published between 1935 and 2011, but somewhat confused by passages like "The longer days in the past he [Keith Runcorn] attributed to tidal friction entirely". But tidal friction has made our days lengthen at the same time as the Moon has moved away from Earth.

The most frustrating page, however, is p. 33, with a photograph of a 1963 Copenhagen conference marking the 50th anniversary of the Bohr atom. Only 18 of the 77 participants (1 or just possibly 2 women) are identified, and even the ones I met within a decade of that time (Møller, Rabi, Weisskopf, Dirac, Wheeler) are unrecognizable.

Conflict of interest: my copy of *Varying Gravity* was sent for review to Another Journal, from which I have inherited it. Some of my favourite books (admittedly fairly old and paperbacks) were purchased for less in total than the price per page of this one. — VIRGINIA TRIMBLE.

Deep Sky Companions: The Caldwell Objects, 2nd Edition, by S. J. O'Meara (Cambridge University Press), 2016. Pp. 556, 26 × 18·5 cm. Price £39·99 (hardbound; ISBN 978 1 107 08397 4).

I know of Stephen James O'Meara from his monthly column in *Astronomy* magazine though I've not had sight of the first edition of this book. There is a short but very readable foreword written by Patrick Moore in 2000 about why he came to compile his list of 109 objects in his 'Caldwell' catalogue. The preface to the first and second editions describe how the author was inspired by Moore, and what the reader will find in the rest of the book.

Messier could only list his objects to avoid by what was visible from the latitude of Paris. *The Caldwell Objects* suffers from no such limitation. Moore compiled his listing from far northern to far southern declinations, hence taking in some of the best sights in the heavens. As would be expected, the bulk of the book is given over to the Caldwell objects themselves. The author has gone to great lengths to research the discovery and history of the Caldwell objects, and the pertinent information is supplied for each, *e.g.*, discoverer, coordinates, magnitude, size, and the original description by the discoverer. Information

*The essential idea came from Sir Harold Jeffreys (1891–1989), another life-long continental-drift denier. If the terran core is a denser phase of mantle material, rather than mostly iron–nickel, then its gradual growth will shrink the Earth. The mountains are the wrinkles, as on an aged apple. Ray [Lyttleton] told me this himself, so it must be so. Admittedly he also averred that Mercury is much less dense than Earth and that traditional propeller planes are largely pushed forward by the backward jetting of exhaust. One wonders what he would have made of the proportionately even thinner blades of a modern wind farm.

garnered by modern professional telescopes and satellites is included, along with an image and finder chart. The author personally observed all the Caldwell objects with a 'backyard' telescope, and a sketch is included with each. Twenty objects not noted by Messier or Moore, but worthy of observation, are then described by the author, and the reason why the double cluster in Perseus is not included in the Messier list is discussed.

Larry Mitchell has provided a concise history of William Herschel in Appendix C. An extensive index concludes the volume. If you have an interest in hunting down celestial goodies and have exhausted the Messier list, this book is for you; even if you have not, this will complement your search. I only have an armchair interest in hunting down these objects, but I found this book a fascinating read and would thoroughly recommend it. — MALCOLM GOUGH.

OBITUARY NOTICE

Peter Chapman-Rietschi (1945–2017)

Peter Albert Leslie Chapman-Rietschi was born in London on 1945 June 25, but, after getting married, he spent most of his life in Switzerland, where he died on 2017 June 6. Peter, a Fellow of the RAS since 1989, had a particular interest in ancient astronomy, the history of astronomy, and star lore, on which topics he wrote conference papers and also contributed articles and letters to *The Observatory*, *QJRAS*, *JRAS Canada*, and *Diotima* (*Revue de Recherche Philosophique*); he was also a frequent book reviewer for this *Magazine*, especially in the field of SETI and related matters, and for that in particular I will miss his help. — DAVID STICKLAND.

Here and There

A STAR FAR, FAR AWAY

That this solar system — named TRAPPIST-1 — lies in the constellation Aquarius a mere 39 million light years away made the discovery even more breathtaking. — *Victoria Times-Colonist*, 2017 March 1, p. A8.

IN A UNIVERSE WHERE REDSHIFT IS NOT A DISTANCE INDICATOR

… a field of view about 8 light years across … shows merging galaxy clusters Abell 3411 and Abell 3412. — *A&G*, **58**, 1.6, 2017.

IN A PREVIOUS LIFE?

[in an obituary of — (1926–2016)] On Christmas Day of 1980 he ventured out as he had done 73 years before. — *The Astronomer*, **53**, 256, 2017 February.